Firm-Level Political Risk and Credit Markets*

Mahmoud Gad
Lancaster University Management School
Lancaster, LA1 4YX
United Kingdom
m.gad1@lancaster.ac.uk

Valeri Nikolaev
University of Chicago Booth School of Business
5807 South Woodlawn Avenue
Chicago, IL 60637
Valeri.Nikolaev@ChicagoBooth.edu

Ahmed Tahoun
London Business School
Regent's Park
London, NW1 4SA
United Kingdom
atahoun@london.edu

Laurence van Lent
Frankfurt School of Finance and Management
Adickesallee 32-34
60322 Frankfurt am Main, Germany
l.vanlent@fs.de

July 2023

*We thank Vivek Anand and Gabriel Varela for their research assistance. We are grateful to Joanna Wu (the editor) and Sugata Roychowdhury (the referee) for constructive suggestions. We thank Markus Schwedeler for preparing the various datasets described in Hassan, Hollander, van Lent, and Tahoun (2019) for use in this paper. We received constructive comments from Aleksander Aleszczyk, Tobias Berg, Aluna Wang, and from workshop participants at Bristol University, Carnegie Mellon University, the European Banking Centre Network Workshop, Lancaster University, Keele University, Northwestern University, the 2019 NYU Annual Accounting Summer Camp, and the University of Chicago. The Deutsche Forschungsgemeinschaft funds Van Lent's research under Project ID 403041268 – TRR 266. Van Lent and Tahoun sincerely appreciate the support from the Institute for New Economic Thinking (INET). Gad appreciates the support from the Pentland Centre for Sustainability in Business.

Firm-Level Political Risk and Credit Markets

Abstract:

We take advantage of a new composite measure of political risk (Hassan *et al.*, 2019) to study the effects of firm-level political risk on private debt markets. First, we use panel data tests and exploit the redrawing of US congressional districts to uncover plausibly exogenous variation in firm-level political risk. We show that borrowers' political risk is linked to interest rates set by lenders. Second, we test for the transmission of political risk from lenders to borrowers. We predict and find that lender-level political risk propagates to borrowers through lending relationships. Our analysis allows for endogenous matching between lenders and borrowers and indicates the presence of network effects in diffusing political risk throughout the economy. Finally, we introduce new text-based methods to analyze the distinct sources of political risk to lenders and borrowers and provide textual evidence of the transmission of political risk from lenders to borrowers.

JEL-code: G10, G12, G18, G21, M41, P16
Keywords: credit markets; political risk; financial institutions; earnings calls

Firm-Level Political Risk and Credit Markets

1. Introduction

The recent decade has witnessed unprecedented growth in political uncertainty, at least partially fueled by a deepening political polarization in society.[1] While economy-wide shocks stemming from the political system have been studied in some detail, recent work suggests that aggregate shocks are only the tip of the iceberg. Firms must also contend with political events that play out at the sector, state, and local levels. These political events can vary over time, be policy-specific, only target specific demographics, or be purely idiosyncratic. Additionally, firms differ in their sensitivity to political events at different levels. Hassan, Hollander, van Lent, and Tahoun (HHLT) (2019) establish a measure of composite political risk derived from a textual analysis of the discussion in conference calls between management and financial analysts. Their measure captures the percentage of political conversation close to words indicating risk or uncertainty in a firm's quarterly earnings call. The primary advantage of this measure is that it records variation across firms within a specific period and across time within a particular firm more powerfully than alternative measures, irrespective of whether the source of political risk is macro-level, sector-level, or firm-level. This property potentially facilitates effective identification in empirical tests.

Using this new data, we examine questions about (1) the causal link between firm-specific political risk and a firm's capital market outcomes and (2) the transmission of political risk throughout the economy from lenders to borrowers, especially when borrowers are bank-dependent. We use panel data analysis to establish a baseline link between firm-level political risk and loan interest rates in the private debt market showing that lenders price their borrowers' political risk. In our primary test, we exploit the redrawing of electoral districts as a source of plausibly exogenous variation in firm-level political risk. Redistricting followed the decennial census of 2010 and changed many firms' exposure to politicians by triggering a change in their House representatives, and hence a change in political uncertianty, as discussed next.

[1] According to https://www.policyuncertainty.com, the US and Global Policy Uncertainty Indices have both increased three to four times between 2010 and 2020.

We use a two-step process to exploit redistricting. First, we construct a treatment variable based on the interaction of three components: a firm is subject to redistricting, it encounteres a change in its representative, and it experiences a shift in district-level political risk. We argue that this variable presents a plausibly exogenous instrument for whether a firm undergoes a change in firm-level political risk. Second, we use a difference-in-differences design centered on changes in the instrument's value *among redistricted firms* to create our treatment and control groups. Control groups include firms experiencing no change in district-level political risk or retaining their representative despite redistricting.

We show that the treatment leads to an increase in firm-level political risk following the redistricting. More importantly, we document that treated firms experience a 24–27 basis-point increase in the total cost of borrowing and a 15–19 basis-point increase in the all-in-drawn spread. Comfortingly, we also document no effect in a placebo test, in which treatment is assigned based on non-political risk.

In our second set of tests, we examine the transmission of political risk from lenders to borrowers highlighting that lenders pass on the expected costs of their own political risk to their borrowers. This transmission is expected if firms are economically tied to each other. We follow several complementary approaches to understand how risk travels from lenders to borrowers. We start by providing evidence that lender-level political risk is transmitted through increased borrowing costs and that borrowers' dependence on banks as a source of capital appears to be the channel behind this result. Specifically, we find that almost all documented effect of arranger-level political risk on loan pricing comes from bank-dependent borrowers. One question arises: why do borrowers form relationships with the particular bank(s), and, similarly, why do banks specialize in lending to certain borrowers, as matching on unobservables can threaten a causal interpretation of these findings?

Because borrowers form relationships with lenders endogenously, the lender-level political risk may be correlated with borrower characteristics, such as investment opportunities, which determine the latter's demand for credit. This, in turn, can result in a spurious relation between lenders' political risk and borrowers' credit spreads. We tackle this issue in several ways. First, we adopt the Khwaja and Mian (2008) approach that effectively precludes the results from reflecting the demand side channel. Accordingly, we restrict the sample to borrowers with at least two loans underwritten by different lead arrangers. We use *firm-year* fixed effects that control for unobservable borrower-time-dependent characteristics. This design allows examining whether a firm that borrows from two different banks pays a higher spread for the loan from the bank with the increased political risk; however, it comes at the cost of limiting our ability to generalize the findings to firms that do not have multiple lenders. With this caveat in mind, we find that lenders' political risk increases the cost of borrowing by 11–16 basis points for a one standard deviation increase in *PRisk*. These estimates are economically significant and comparable to our cross-sectional tests.

Second, we address the choice to form relationships by running the analysis within lender-borrower pairs, i.e., holding constant the factors that trigger a match and using the CDS market as a laboratory. Specifically, we examine the time-series variation in CDS spreads for ongoing relationships while controlling for lender-borrower pair fixed effects. When borrowers are bank-dependent, an adverse political shock to a lender becomes relevant to the borrower as it potentially changes the lender's willingness or ability to waive covenants, roll over lines of credit, and renegotiate other contractual terms (e.g., increase limits on investments or extend maturity). For these reasons, an increase in lender-level political risk ultimately increases the risk of an adverse credit event, resulting in a higher CDS spread. Accordingly, we examine whether a change in a lender's political risk translates into a change in borrowers' CDS spreads *after a contract is in place.* This design rules out the possibility that unobserved borrower or lender characteristics, responsible for why the borrower chooses a particular lender, explain the effect of lender *PRisk* as the fixed effect structure absorbs them. We find that a one standard deviation change in lenders' political risk increases CDS spreads by 8.2 basis points within each borrower-lender pair.

Finally, we address the endogeneity of lenders' *PRisk* by using redistricting as a source of quasi-experimental variation. When a lender's portfolio experiences an increase in political risk, the lender's overall exposure to political uncertainty increases. We aggregate at the portfolio-level changes in the political risk for the redistricted borrowers and use this variation to test the effect of lenders' political risk on the loan prices charged to *non*-redistricted borrowers. This analysis further corroborates our conclusions that political risk is transmitted from lenders to borrowers.

Having documented the transmission of lenders' political risk to borrowers, we conduct an exploratory analysis of the sources of lenders' political risk and further investigate the channels that transmit these risks. We tease out the specialized political language used by either financial institutions or borrowers to learn more about their specific political risk sources. We then use our knowledge about these particular (sources of) political risks to investigate their transmission through the economy in the next step. To implement this analysis, we return to the text of the conference calls and apply the corpus linguistics' idea of "keyness," i.e., words with an *unusual* frequency in each text, to determine those political phrases distinct for borrowers and lenders. We show that the political bigrams with keyness to lenders align with theoretical predictions about banking sector regulation as an important source of political risk. We then use a penalized regression (LASSO) to examine which political bigrams matter most in explaining the borrower's cost of debt. As the keyness analysis shows whether a given bigram is distinct to lenders or borrowers, we can determine if the most pricing-relevant bigrams stem from the former or the latter. We report that "priced" political risk bigrams overwhelmingly stem from lenders, suggesting that lenders are more sensitive to their own sources of political risk relative to those of their borrowers', and the transmission of political risk runs from lenders to borrowers.

Our study makes three contributions to the literature. We are the first to use a comprehensive firm-specific measure of political risk to provide causal evidence on the relationship between political risk and credit-market outcomes. Our evidence contributes to a growing literature linking political risk to the financial and factor markets.[2] Earlier work provides initial evidence on how variation in exposure to aggregate sources of political risk, such as federal elections, the degree of disagreement between federal policy-makers, or a given industry's dependence on government contracts, affects asset prices (e.g., Kara & Yook 2018; Pham 2019). Prior studies also recognize that aggregate political risk gives rise to firm-level variation in political risk due to differences in exposure. In particular, firms build connections with politicians that affect firm value, access to information, and the cost of capital (e.g., Fisman 2001; Faccio 2006; Cooper *et al.* 2010; Akey 2015; Wellman 2017). Such connections may be a source of political risk as much as a means to reduce risk (Wellman 2017; Hassan *et al.* 2019). Houston

[2] See, e.g., Belo, Gala, and Li (2013); Besley and Mueller (2017); Gourio, Siemer, and Verdelhan (2015); Handley and Limao (2015); Kelly, Pástor and Veronesi (2016); and Koijen, Philipson and Uhlig (2016).

et al. (2014), for example, find evidence of *lower* costs of bank loans for companies that have board members with political ties. Political risk encompasses a broad set of factors beyond specific political connections. In response to this challenge, more recent work measures *firm-level* political risk based on a firm's sensitivity to Baker, Bloom, and Davis (BBD) (2016) measure of *economy-wide* policy uncertainty (see, e.g., Francis *et al.* 2014; Bordo *et al.* 2016; Berger *et al.* 2018; Drobetz *et al.* 2018; Ng *et al.* 2018; Wang *et al.* 2019; Kaviani *et al.* 2020). Yet, this approach does not capture rich variation in political risk stemming from local, sector-specific, time-specific, and idiosyncratic political factors (HHLT).[3,4] Given this, we still lack large-scale evidence based on a comprehensive firm-level measure that reflects a range of sources of political risk.

Our second contribution is to provide systematic evidence that firm-level political risk is transmitted across firms through business relationships. By relying on variation in aggregate political risk exposure, prior work is limited in understanding how political risk diffuses throughout the economy. For example, because EPU measures are common across all financial institutions, examining the effects of the considerable heterogeneity in banks' political risk is impossible. In contrast, we show that an increase in a bank's political risk causes borrowers' interest rates to increase in the presence of lending relationships. Lenders appear to be more sensitive to their own sources of political risk relative to their borrowers,' a result that has not yet been documented in the literature.

Finally, we contribute to the literature by introducing new, text-based methods to understand the distinct sources of political risk to lenders and borrowers. We apply these methods in the context of earnings calls, determine which sources are important in explaining loan pricing, and infer whether lenders can transmit their political risk to borrowers based on text evidence. These new textual analysis

---

[3] Table OS3 of our Online Appendix shows that our main findings are unchanged by including these "political risk betas," which have a limited ability (if any) to explain credit market outcomes.

[4] Political shocks are a significant source of firm-level (idiosyncratic) risk. HHLT provide two case studies that suggest that firm-level political risk arises from the interactions between firms and governments, which can be highly heterogeneous and specific to a particular firm. Regulations, government budgeting, and procurement can have different impacts on firms, even within the same sector, leading to firm-level variation in political risk. In one example, HHLT describe an energy firm exposed to changes in EPA emissions rules, court challenges, and reforms. While those regulatory changes could affect multiple firms, their impacts on firms are highly heterogeneous. The related conference call reveals that this firm relies on older coal-burning furnaces that emit a lot of mercury and is present in states subject to interstate emissions rules. Other local regulatory risks include a change in compensation for providing spare generating capacity by a regulator in Ohio and the aggregation of electricity purchases in North Carolina, which specifically affect this firm due to its presence in these states.

techniques can be used to understand the mechanism behind a causal chain, not only in the lending setting but also in any other network setting (e.g., all forms of customer-supplier relationships). This method also has many potential applications in the accounting and finance literature.

Using a time-varying, firm-level measure of political risk allows us to tackle several econometric challenges and use a unique source of exogenous variation in political risk, namely the congressional redistricting after the 2000 census, to provide causal evidence on several unanswered questions in the literature. Further, we examine whether and how shocks to lenders' political risk are transmitted to borrowers and the potential network effects when one agent's risks impact other agents in a credit market. Our design leverages that the *PRisk* measure is gleaned from earnings conference calls; these calls are available for borrowers and lenders and usually happen at different times during a given quarter.

2. Firm-level political exposure, risk, and credit market outcomes

Firm-level political risk affects credit markets beyond a systematic political risk factor like the macro-economic policy uncertainty (Pastor & Veronesi 2012). This can happen on the demand side (i.e., the borrower's political risk) or the supply side (i.e., the lender's political risk). This section provides the theoretical background for the role and consequences of firm-level political risk.

On the demand side, a borrower's political risk can affect credit market outcomes through two channels. First, political risk can affect credit spreads by creating uncertainty about the effect of political and regulatory interference on firms' operating and investment decisions. Political costs can diminish investment opportunities, decrease cash flows, and adversely affect the collateral value. Political interference can also increase the likelihood of default and the loss given default and is thus expected to influence debt pricing. One (extreme) example of this interference is that political actions can result in the seizure of assets owned by US companies in a foreign country, with creditors bearing high costs (Pagano & Volpin 2001). Politicians could also influence court proceedings or legal disputes between creditors and other stakeholders. Local politicians, for example, are incentivized to protect a debtor's labor force and local suppliers by mitigating the adverse consequences on the local economy and the voting public. Politicians influence the seizure of assets during default proceedings or can prevent firms from going bankrupt in the first place (Faccio *et al.* 2006; Tahoun & van Lent 2019). These local, state, and federal political interventions impact future cash flows and the value of assets on a firm's balance sheet.[5]

Information asymmetry is another channel through which borrower-level political risk can influence debt markets. More specifically, the scope for political interference means that some economic agents might have an informational advantage about forthcoming political events (Bertrand *et al.* 2014; Wellman 2017; Akin *et al.* 2019). This proposition is consistent with analysts often using conference calls as an opportunity to ask questions about political topics. This practice, in turn, suggests that *uninformed* market participants will price protect against political risk, which is expected to affect both yields and liquidity in credit markets.

On the supply-side, political risk can be manifested in lenders' exposure to shocks stemming from new rules and regulations both at aggregate levels and locally, as well as the enforcement of these regulations. These include changes in capital requirements, stress testing, taxation, subsidies, etc. Regulators treat banks with different leniency and practice regulatory forbearance for distressed lenders (Agarwal *et al.* 2014). Ultimately, these risks can adversely affect lenders' decisions to supply credit or renegotiate existing contracts. They can also disrupt banks' ability to form syndicates and maintain relationships with other lenders.

A growing literature suggests that in the presence of lending relationships and informational asymmetries, lenders' idiosyncratic shocks propagate to the real sector and impose costs on firms (e.g., Chodorow-Reich 2014; Chodorow-Reich & Falato 2017; Christensen *et al.* 2020b). For example, Chodorow-Reich (2014) shows that exogenous variation in lenders' financial health affects the employment choices of their client firms. This evidence makes it plausible that exogenous variation in lender-level political risk can systematically affect borrowers' loan outcomes.

[5] Firm-level political risk is expected to affect credit spreads (even when idiosyncratic) due to the nature of the creditor's claim. Consider a risk-neutral economy where all uncertainty about debt is firm specific and all loans earn a risk-free rate in expectation. Since bond investors have limited upside potential, an increase in firm-level uncertainty (and in default and loss-given default risk) needs to be compensated for by an increase in bond yields that guarantees an expected return that is equal to the risk-free rate. Indeed, theoretical work (e.g., Merton 1974; Gilchrist *et al.* 2014) and empirical evidence both confirm the importance of idiosyncratic volatility in explaining bond yields.

Furthermore, firm- or lender-specific political risk may impose negative externalities on other firms through network effects. Political shocks to one credit institution propagate to others due to their interconnectedness, potentially leading to significant disruptions in the credit market (e.g., Blume *et al.* 2011; Acemoglu *et al.* 2012; Acemoglu *et al.* 2014).

3. Data

In this section, we briefly describe the methodology used by Hassan *et al.* (2019) to construct the firm-level political risk measure ($PRisk_{it}$) and the firm-level political sentiment measure $PSentiment_{it}$ (which captures news about the mean of the political shock). We then provide summary statistics for these measures and the other key variables. Because our tests move from the borrower to the lender level, we organize our discussion of the sample selection procedure, data sources, and descriptive statistics accordingly.

3.1 PRisk measure

To create a firm-specific, time-varying political risk measure, HHLT use quarterly conference calls by publicly listed firms where financial analysts and other market participants discuss current affairs with senior management.[6] By applying a machine-based algorithm to the transcripts of these calls, HHLT can determine what percentage of the call's conversation is political. The algorithm identifies political word combinations (bigrams) by comparing training libraries consisting of political texts with non-political ones. Then, the transcripts of earnings calls are processed to count these political bigrams in the vicinity of synonyms of "risk." By requiring that political bigrams are used close to risk synonyms, as HHLT show, political *risk* can be isolated from simple political *exposure*.

In contrast with the conventional wisdom that political and regulatory decisions have a relatively uniform impact across firms in a developed economy (Pastor & Veronesi 2012), a variance decomposition of $PRisk_{it}$ shows that the political system appears to be a major source of "idiosyncratic risk." Only 0.81 percent of the variation in $PRisk_{it}$ is explained by time fixed effects (i.e., by aggregate shocks), and sector and sector-by-time fixed effects only explain another 4.38 percent and 3.12 percent, respectively. The remaining 91.69 percent is firm-level variation: 19.87 is permanent differences across

[6] This measure and several other firm-level measures of risk and sentiment are publicly available at www.firmlevelrisk.com.

firms, i.e., between-firm variation, and 71.82 is change over time, i.e., changes within firms in a given sector. This suggests that political risk has a rich firm-level impact, justifying our examination of its effects on the credit market.

PRisk is defined as the proportion of the earnings call devoted to political topics and is computed as the sum of political bigrams in the vicinity of risk synonyms divided by the total number of bigrams (multiplied by 10^6). In our sample, summarized in Table 1, Panel A, the average borrower-level $PRisk_{it}$ is 107 with a median of 70, indicating a significant right skew. Table 1, Panel B shows that lender-level $PRisk_{it}$ (measured at the lead-arranger level) is more than two times higher than the borrower measure, with an average of 231 and a median of 191. The fact that financial institutions are generally subject to high levels of political risk supports the idea that, given the concentration in the financial sector, political risks can propagate to the real sector through credit supply and lenders' contracts with borrowers.

Figure 1 depicts how average $PRisk_{it}$ evolves for borrowers and lenders. In addition to high levels of political risk for financial institutions, we observe that the borrower vs. lender time series of *PRisk* began to diverge noticeably before the 2008 financial crisis and exhibited only slow convergence in its aftermath.

To ensure that the political risk measure does not capture news about mean political exposure, i.e., political sentiment about past or future events discussed in a firm's conference call, our analysis of political risk controls for the measure of political sentiment. $PSentiment_{it}$ is constructed by counting the use of political bigrams based on their proximity to positive and negative sentiment words from the Loughran and McDonald (2011) sentiment dictionary and then by scaling the resulting count by the total number of bigrams in the transcript. Controlling for political sentiment alleviates the concern that we capture the first moment of political risk (or that senior management attributes negative news about economic performance or outlook to political events) during earnings calls.

In all subsequent analyses, we standardize $PRisk_{it}$ ($PSentiment_{it}$) to have a zero mean and a standard deviation of unity, referring to the standardized variable as $zPRisk_{it}$ ($zPSentiment_{it}$).

3.2 Redistricting data

We collect data for our redistricting tests as follows. First, we obtain data on the changing congressional districts from the US Census Bureau website and Lewis et al.'s (2013) shapefiles.[7] Second, we extract historical header information from 10-Q filings using the WRDS SEC Analytics Suite. We retrieve the 2010 address of each firm's headquarters and geocode it using google sheets to obtain coordinates (latitude and longitude). The coordinates from the firm addresses are then matched to the appropriate congressional district to identify any changes to the firm's district. Finally, we collect data about the U.S. House of Representatives election return (i.e., vote share) for each district from the MIT Election Data and Science Lab and match it to firms.[8]

3.3 Data from standard sources

We also use data from several other sources. Transcripts of quarterly earnings calls are from the Refinitiv Eikon database and cover the period 2002-2020. Data on private debt are from Dealscan and are aggregated at the deal level. Financial data on borrowers are from Compustat. We use intersections of these datasets to examine the role of $PRisk_{it}$ in credit markets.

We begin by analyzing private loan markets using the intersection of Dealscan and Compustat data, merged by the Chava and Roberts' (2008) link. Our loan-market sample for this analysis contains 11,022 observations from 2,576 firms between 2002 and 2016. Table 1, Panel A provides summary statistics for all variables used in the firm-level analyses.

Consistent with prior research, our *bank-level* analysis focuses on the sample of lead arrangers (e.g., Bharath *et al.* 2007; Ivashina & Scharfstein 2010; Giannetti & Saidi 2018). We also followed prior research (e.g., Gopalan *et al.* 2011) using the Dealscan variable "LeadArrangerCredit" to identify lead arrangers. The Dealscan database overwrites information on lenders and their ultimate parents, so we use Schwert (2018)'s Dealscan-Compustat link file that ties the most active lenders in Dealscan to their banking groups every quarter, capturing mergers and acquisitions over time. After merging with the political risk data, our final sample consists of 9,649 loans arranged by 62 unique banks between

---

[7] See https://www2.census.gove/geo/tiger/ and http://cdmaps.polisci.ucla.edu/. Primarily, we use these sources to ensure that the shapefiles are consistent and we find that this is almost always the case.
[8] We obtained this data from: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/IG0UN2.

2002 and 2016. We have one observation per arranger for loans with multiple arrangers, leading to 20,137 observations. Table 1, Panel B shows various political and non-political risk measures at the arranger level, which we use to examine how political risk is transmitted through syndicate networks and the financial institutions' loan portfolios.

4. Borrower political risk

Our first analysis examines the relation between borrower-level political risk and credit market outcomes. Specifically, we test whether debt markets price the borrower-level political risk in higher credit spreads.

4.1 Panel data analysis

We first use panel data to establish an association between *PRisk* and interest rates in private debt contracts. We estimate the following regression:

$$DepVar_{ijt+1} = \alpha_0 + \beta_1 zPRisk_{it} + \zeta X_{it} + \delta_s \times \delta_t + \varepsilon_{ijt} \quad (1)$$

where *i* denotes firm, *j* denotes loan facility, and *t* denotes time. *DepVar* is one of two outcomes: the total cost of borrowing (*Total cost*) as defined in Berg *et al.* (2016) or the all-in-spread-drawn (*All-in-drawn*), defined as the spread over LIBOR. *X* is a comprehensive set of the firm-level economic determinants of a firm's political risk. It includes (1) controls for economic performance that includes the natural logarithm of the borrower's market capitalization (*lnMCAP*), the borrower's return-on-assets (*ROA*), the change in *ROA* (*ΔROA*), and a loss indicator (*Loss*); and (2) controls for general economic uncertainty and the risks faced by borrowers ('generic risks'): stock price volatility (*ReturnVol*), market-to-book value (*MTB*), leverage (*LEV*), and a proxy for proximity to financial distress (*Zscore*).[9] *X* also includes a control for political sentiment, $zPSentiment_{it}$, to disentangle the effect of information about the variance of political shocks from information about the mean. Its inclusion alleviates the concern that a negative economic outlook is correlated with political topics in conference calls because management strategically attributes negative news about economic performance or outlook to political events. Finally, we control non-political risk (*zNPRisk*), which captures the share of the conference call conversation centered on risks and uncertainties associated with non-political topics. *NPRisk* captures

[9] We view risk as a multidimensional construct where different dimensions have different pricing effects.

all other mentions of risk or uncertainty (that are not related to political risk). In precise notation: $NPRisk_{it} = \frac{1}{B_{it}} \sum_{b}^{B_{it}} \{1[b \in \mathbb{R}]\} - PRisk_{it}$, defining $\mathbb{R}$ as the set of synonyms for risk and uncertainty taken from the Oxford English Dictionary. By including *NPRisk*, we intend to control for variation in risk related to other shocks than political. This feature of the textual method allows us to identify the firm-level effects of the risk associated with political risk, *specifically*.

The model also includes sector (δ_s) and time (calendar year) fixed effects (δ_t) as well as their interaction ($\delta_s \times \delta_t$) to isolate firm-level variation in $zPRisk_{it}$ from aggregate or sector-level variation. To examine whether over-time changes within the same borrower can explain the effect of firm-level political risk on credit outcomes, we report a specification that includes firm fixed effects.[10]

Table 2 presents the results of this analysis. Our standard specification includes sector × time fixed effects and we also add firm fixed effects in some specifications. Given that loans are issued less frequently than every quarter, we measure $PRisk_{it}$ as the average of the four quarters preceding loan initiation. We observe a statistically significant and economically meaningful association between cost-of-debt and $PRisk_{it}$ in all four columns. In Column 1, the coefficient estimate on $zPRisk_{it}$ is 8.31 (*t-value* = 3.41), which implies that a one standard deviation increase in firm-level political risk is associated with an 8.31 basis-point increase in the total cost of borrowing, or about a nine percent increase relative to the sample median. After controlling for firm-fixed effects in Column 2, the estimated coefficient drops to 5.10 (significant at the five percent level), suggesting that approximately two-thirds of the association is due to within-firm variation. The all-in spread in Column 3 also has a positive association with $zPRisk_{it}$. When we include firm-fixed effects, the drop in the coefficient estimate is similar to the observed attenuation for the total cost of borrowing. Economically speaking, increasing firm-level political risk by one standard deviation is associated with a 6.78 point higher all-in spread, or about a 3.8 percent increase relative to the sample median.[11,12]

[10] Note that Hassan et al. (2019) report that permanent differences across firms in a sector account for about 20 percent of "firm-level" variation in $PRisk_{it}$. Changes over time in the identity of firms within a sector most affected by political risk account for the remaining 80 percent.

[11] We report similar results for the bond and CDS markets in Sections 1 and 8 of the online appendix. In Section 2, we show that political risk is negatively associated with debt issuance.

[12] In Section 4 of the online appendix, we show that when teasing out *persistent* firm-level political risk the effect on debt market outcomes is more pronounced.

The analysis in this section moves us closer to a causal interpretation of results than the research in prior studies focusing on the aggregate political risk because the fixed effects structure absorbs all over-time sector-level variation in political risk confounded by macro- and industry-level trends. Absent a randomized experiment, however, we cannot rule out the possibility of correlated omitted variables. The following section uses quasi-experimental variation in $PRisk_{it}$ to further tackle this issue.

4.2 Causal effect of firm-level political risk: An analysis of electoral redistricting

To go beyond the documented association between firm-level political risk and debt pricing, we use the redrawing of federal electoral districts in states with more than one congressional district to uncover plausibly exogenous variation in firm-level political risk. In a series of rulings in the 1960s, the U.S. Supreme Court decided that legislative districts should contain roughly equal populations. Consequently, district boundaries are periodically readjusted to account for new population data, which become available after each decennial census. For a given firm, this practice could unfold such that before the census, they are represented in Congress by a moderate politician, only to find their fate tied after redistricting to a more partisan individual, potentially raising its political risk.[13]

We focus our analysis on the 2010 Census that provoked a wave of redistricting activities across the US, most of which ended in 2011 when the last legal challenges to proposed new district lines were settled. In Table 3, Panel A, we present the number of firms affected by the redistricting following the 2010 decennial Census. Redistricting affects many firms; concentrating on our loan market sample, we find that 587 out of 1,486 firms were in a different congressional district after the 2010 Census (about 39 percent of firms).[14] Figure 2, Panel A shows the distribution of redistricted firms across US states

[13] Indeed, multiple anecdotes suggest that redistricting is a salient political concern to firms. For example, consider the following exchange in the conference call of Penn National Gaming on July 23, 2002: "... don't believe that the legislators in general terms, but even throughout the leadership, they didn't really perceive they had a choice. There was a complete *redistricting* in the state of Illinois that occurred this year, so everybody's up for reelection. They have a gubernatorial race in this state, and they were faced with I believe a billion five, deficit." Similarly, on November 3, 2003, a senior manager at Los Angeles-based insurer Mercury General reflected: "…But he will get a lot done because with the initiative process in California, the referendums available to us, there is already a referendum working to repeal the bill that allowed illegal immigrants to get a driver's license. There is another one pending to do away with the political aspect of the *redistricting* in California, put in the hands of a court and I think that is going to succeed." (emphasis added in both quotes).

[14] Nearly all US districts (425 out of 432) changed boundaries between 2010 and 2012 (Autor *et al.* 2020).

using the initial *PRisk* sample. The graph shows that California has the biggest number of redistricted firms, followed by New York and Florida.[15]

Treatment group. We exploit instances in which redistricting is likely to induce a plausibly exogenous increase or reduction in political risk for a given firm. To do so, we go through the following steps, depicted in Figure 3. First, we note that whether a firm is redistricted or not is determined by general economic and demographic trends. Therefore, rather than comparing redistricted and non-redistricted firms, we limit our sample here to *redistricted* firms only. Second, redistricting does not always lead to a change in political risk. To ensure variation in political risk, as a necessary condition, we require that redistricted firms also have a new Congressional representative. Third, to distinguish between an induced increase vs. reduction in political risk caused by the change in representative, we compare whether a firm's new political district has historically exhibited a higher (lower) political risk relative to the firm's old district. To measure historical political risk for a given district, we take the average political risk of companies populating this district over the five years preceding the redistricting event. Intuitively, we assume that the political districts are riskier, i.e., more likely to be represented by high-variance politicians and vice versa, if the firms populating these districts have historically exhibited higher *PRisk*, on average. Thus, *Treated* as an interacted variable that multiplies the following three components: (a) a zero-one indicator for whether there is a redistricting that affects the firm, (b) a zero-one indicator for whether the redistricted firm ends up with a new house representative as a result and (c) a +1/-1 indicator for whether the new district for the firm has higher/lower historical aggregate PRisk than the old district.

Control group. We use the following three control groups (treatment indicator of zero): (1) firms that have a new representative, but the new district has a similar level of political risk compared to the old district (it remains in the same quartile of *PRisk* after redistricting); (2) firms that are redistricted but continue to be represented by the same politician (i.e., when the politician moves

[15] Figure 2 (B) shows the map of Democrat vs. Republican representatives in North Carolina pre and post redistricting. This graph shows that the ratio of Democrats to Republicans changed from 7:6 to 4:9 in two adjacent elections, providing evidence that redistricting could result in a considerable swing of politicians.

congressional districts with the firm); and (3) the combined set of these two control groups. [16,17] Figure 3 provides a summary of our approach.

As we do not compare redistricted vs. non-redistricted firms, we do not invoke an admittedly strong assumption about the exogeneity of redistricting. Instead, we make a milder assumption that conditional on a firm being redistricted, whether the firm's Congressional representative remained the same or turned over after the census is exogenous. This assumption is plausible as firms can generally do very little to influence redistricting outcomes (Denes *et al.* 2017). To corroborate this further, we check whether the outcome that a firm ends up with a new politician due to redistricting is correlated with state-level economic or demographic trends; this is not the case.

In sum, we follow a two-step process to exploit redistricting. First, we construct a treatment variable based on the interaction of three conditions: (a) a firm experiences a change in its political district due to redrawing of district boundaries, (b) the firm is assigned to a new House representative, and (c) the new district has a systematically higher or lower historical average level of political risk compared to the previous district. This triple interaction provides an instrument for whether a firm experiences a change in political risk. Second, we use a difference-in-difference design that centers on the treatment within the set of redistricted borrowers. Intuitively, the firms are treated if they experience a change in representative and are transitioned from a district with historically low political uncertainty to a district with elevated political risk and vice versa. This variation allows estimating the causal effect of political risk on the cost of debt.

Before proceeding with the main test, we validate our treatment variable by examining whether the change in a political representative indeed causes changes in firm-level political risk induced by redistricting. Recall that this is expected when the firm joins the district with a systematically higher or lower level of political risk. To test this, we run the following difference-in-differences specification:

$$PRisk_{it} = \beta_1 Treated_{ij} \times After_t + \beta_2 Treated_{ij} + \delta_t + \delta_i + \varepsilon_{it} \quad (2)$$

[16] In contemporaneous work, Denes et al. (2018) also use the change in the political boundaries of the firm as a source of exogenous variation in firm-level uncertainty. These authors, however, define a firm as being redistricted if its pre-redistricting representative runs for election post-redistricting, but in a different district. According their definition, about 20% of firms were redistricted after 2010.

[17] U.S. House of Representatives data were obtained from the MIT Election Data and Science Lab (see, https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/IG0UN2).

where $Treated_{ij}$ is defined as above; $After_t$ takes the value of one after the 2010 Census redistricting becomes final;[18,19] δ_t is time fixed effect; and δ_i is firm fixed effect.[20]

The results of this analysis are presented in Table 3, Panel B. We observe statistically and economically significant increases in a firm-level political risk after the firm is exposed to our treatment resulting from redrawing political districts. This evidence allows us to proceed to the analysis of the cost of debt.

To identify the effect of political risk, we use the following diff-in-diff specification:

$$DepVar_{ijt} = \beta_1 Treated_{ij} \times After_t + \beta_2 Treated_{ij} + \zeta X_{it} + \delta_t + \delta_i + \varepsilon_{ijt} \quad (3)$$

where $DepVar_{ijt}$ is one of our cost-of-debt outcome variables (the total cost of borrowing or the all-in-drawn spread); $Treated_{ij}$ is defined as above; $After_t$ takes the value of one for loans arranged in the quarters after the 2010 Census redistricting becomes final; X_{ijt} is the vector of control variables that explain interest rates (see Equation 1); δ_t is time fixed effect; and δ_i is firm fixed effect.

We present the Ordinary Least Squares estimates of Equation (3) in Table 4, Panels B-D, where each panel represents an estimation based on one of our three control groups. We find positive and significant effects of redistricting on the total cost of borrowing and the all-in-drawn spread. The results are consistent across the three control groups. Compared to control firms, firms with higher political risk after redistricting experience a 24 to 27 basis-point higher total cost of borrowing (*p-value* < 0.05) and have a 15 to 19 basis-point higher all-in-drawn spread (*p-value* < 0.05).

Parallel trends. A critical assumption in this analysis is that the cost-of-debt trends for treated and control firms would have been the same in the absence of treatment (Angrist & Pischke 2009). We follow Bertrand and Mullainathan (2004) and decompose *Treated* × *After* into separate periods to provide evidence supporting the parallel trends assumption. We estimate the following model:

$$DepVar_{ijt} = \sum_\tau \beta_\tau Treated_{ij} \times After(\tau)_t + \zeta X_{ijt} + \delta_t + \delta_i + \varepsilon_{ijt}, \quad (4)$$

[18] Most redistricting took place in 2011. Dates are available here: https://redistricting.lls.edu/resources/maps-across-the-cycle-2010-congress/

[19] While our treatment is staggerred, we did not implement Callaway and Sant'Anna (2021)'s approach because most of treatments took place in a short window (i.e. throughout 2011).

[20] For this analysis, we restrict the sample period to *PRisk* years from 2007 to 2014 to ensure that the pre- and post-event windows are approximately the same length.

where $After(\tau)_t$ is an indicator variable that takes the value of one in year τ relative to period 0 and zero otherwise. We set 2011 as period 0 and take the three years before and after as our window so that τ ranges from -3 to +3. We plot the estimated coefficients and the five percent confidence intervals of $Treated_{ij} \times After(\tau)_t$ in Figure 4. The estimated coefficients are not statistically different from zero before the redistricting event, consistent with the parallel trends assumption. Importantly, we see a significant positive coefficient on Treated after redistricting using the three defined control groups. Despite point estimates likely to be noisy, the confidence intervals are tight, and the effect appears to hold in each of the three years after 2011, consistent with companies being exposed to a different level of political risk after redistricting.[21]

Placebo tests. We repeat our analysis using our proxy for non-political risk (*zNPRisk*) to construct the placebo variable *Treated*. Our maintained assumption is that changes in political risk caused by a new representative are unrelated to economic risk factors (i.e., non-political risk). Therefore, using non-political risk as a basis for assigning firms to a pseudo-treatment category should not change the cost of borrowing after the redistricting event (as long as the event only affects political risk). We test this and report the results graphically in Figure 5, Panels A-C, for each control group. Panel A uses firms redistricted to a different politician but within the same quartile of *NPRisk* as a control; Panel B uses firms redistricted to the same politician, and Panel C uses firms redistricted to a different politician but within the same quartile of *NPRisk* and firms redistricted to the same politician. We find no evidence that being assigned to a higher *NPRisk* district affects the cost of borrowing regardless of which control group we use in the test.

The granularity of our firm-level political risk measure and the plausibly exogenous variation in the redrawing of congressional districts both support a causal interpretation of the documented association between political risk and the cost of debt in private debt markets. The magnitude of the effect in the redistricting tests is not directly comparable to our earlier panel regression results, but the

[21] In the Online Appendix (Table OS10), we perform an alternative test for significant differences in the pre-trends for loan spreads between the treatment and control groups and find no significant differences.

effect of redistricting is sizeable, consistent with these shocks having an important impact on a firm's political exposure.[22]

5. Transmission of political risk

This section addresses our second research question of whether political risk is transmitted across economic agents. We perform a series of tests investigating whether supply-side political risks in the credit market are passed onto borrowers through higher credit spreads. We also test whether increasing lenders' political risk adversely affects lenders' credit supply and provide evidence consistent with this conjecture in Section 6 of the Online Appendix.

5.1. Transmission of political risk from lenders to borrowers

In the absence of perfect competition in credit markets, banks can take advantage of their market power to pass on political risk to their borrowers.[23] The theoretical literature suggests that some borrowers become bank-dependent because their lender has an informational advantage over outside lenders (Sharpe 1990; Rajan 1992). To test the transmission, we use the specification from Equation (1), which examines variation in the borrower's total cost of borrowing (*Total cost*) and all-in-drawn spread (*All-in-drawn*) at the individual loan level. However, we now include *lender-level* political risk, defined as the standardized political risk for the lead arranger of the loan syndicate, *zPRisk_Arranger*, and lender-level control variables.[24] For the loan level analysis, we continue to rely on annualized versions of *PRisk*.

We present the results of this augmented specification in Table 5, Panel A. Columns 1 and 3 report the coefficient estimates without arranger (bank) fixed effects, while Columns 2 and 4 include fixed effects as additional controls. Columns 5 and 6 include bank controls: bank size, reputation, ROA, Tier1, non-performing loans, loans to assets, deposits to assets, and assets growth. While this data

[22] We show in the online appendix (Section 5) that political lobbying and donations can mitigate some of the effects of political risk. Firms exposed to political risk may also prefer to access the public bond markets to minimize political risk costs. We provide evidence consistent with that hypothesis in Section 9 of the online appendix.
[23] Note that the presence of imperfect competition does not imply that lenders maximize the rent they can feasibly extract from a borrower at any given point in time; this is because of lenders' desire to maintain a mutually beneficial long-term credit relationship. However, when creditors experience an adverse (e.g., political) shock, they are likely to sacrifice some of the benefits of long-term relationships in exchange for an increased immediate benefit.
[24] We also measure and include arranger's political sentiment, *zPSentiment_Arranger*.

requirement considerably limits the sample size, we continue to find economically and statistically significant effects of arranger-level political risk on *Total cost* and *All-in-drawn* spread. The coefficient estimate on *Total cost* is 9.84 (*t-value* = 2.98), suggesting that a one standard deviation change in arranger-level political risk increases the total cost of borrowing by almost ten basis points. The coefficient estimate for *All-in-drawn* is 6.9 (*t-value* = 3.44), which is also economically considerable. These effects are partially explained by the cross-sectional variation of political risk across lenders (Columns 2 and 4). The difference between the estimated effects on arranger and lender political risk is significantly reduced once we control for persistent arranger-level differences (arranger fixed effects).

5.2. The effect of borrowers' outside options

The assumption underlying the analysis in the prior subsection is that imperfectly competitive markets enable lead arrangers to push their own political risk onto borrowers by increasing loan prices. To shed further light on this, we examine whether the effect of arranger-level political risk on loan pricing is stronger for bank-dependent borrowers.

We use three different measures of bank dependence: (1) an indicator equal to one if the percentage borrowed from the current lead arranger(s) over the previous three years is at least 50% of the firm's total loan amount over the past three years (Santos & Winton 2019); (2) an indicator equal to one if the borrower did not access the bond market in past three years (Santos & Winton 2019), and (3) an indicator variable equal to one when the total number of a borrower's lenders over the past four transactions is below the median (see, Murfin 2012). First, we return to the augmented version of Equation (1) that includes arranger-level political risk but adds a bank dependence indicator. We then interact this indicator variable with arranger-level political risk (*zPRisk_Arranger*). For bank-dependent borrowers, we predict that the political risk of lead arrangers is reflected in loan prices and that there is a positive coefficient estimate on the interaction term.

In Table 5, Panel B, we provide evidence consistent with this prediction. We report three specifications using the three bank dependence measures defined above. Of interest in these regressions is the coefficient on the interaction of the indicator variable (*Bank dependent*) and the political risk of the arranger (*zPRisk_Arranger*). All specifications find a significant, positive coefficient on the interaction term. Note that the coefficient estimate on the main effect *zPRisk_Arranger* is no longer significant in all but one case. This coefficient estimate represents the association between the arranger-level political risk and the total cost of borrowing for not bank-dependent borrowers. Thus, almost all documented effect of arranger-level political risk on loan pricing comes from bank-dependent borrowers. Overall, the evidence presented in the table supports the hypothesis that political risk is transmitted from lenders to borrowers through relationships.[25]

6. Accounting for the endogenous matching between borrowers and lenders

6.1. Evidence against the demand-side explanation: Within-firm-year analysis

Why do borrowers become dependent on certain banks (or why do banks match up with certain borrowers)? The borrower dependent on a politically risky bank may have different reasons for forming and staying in a relationship with the lender. In particular, such lenders may exhibit larger credit capacity, better reputation, greater expertise in the borrower's projects, greater ability to monitor loans and to form and manage syndicates, and specialization in monitoring certain types of collateral. To the extent lenders and borrowers match each other based on such unobservable (to a researcher) characteristics, the association between lenders' *PRisk* and the borrowers' cost of debt can be potentially spurious. Specifically, if lenders' political risk, via the selection decision, is correlated with borrowers' investment opportunities, variation in the investment opportunities can ultimately be responsible for the variation in interest rates.

Our measure's firm- and time-specific nature allows us to address this issue from several angles. The first strategy we follow holds the borrower-year characteristics fixed and thus precludes the demand-side explanation where the borrowers with certain investment opportunities match with politically risky lenders. In particular, we use the Khwaja and Mian (2008) approach, which limits the analysis to borrowers with at least two loans underwritten by different lead arrangers and uses within borrower-year variation in interest rates to identify the effect. We use the following model:

$$DepVar_{i,l,t+1} = \beta_1 zPRisk_{l,t} + \delta_{i,t} + \varepsilon_{i,l,t}, \tag{5}$$

where $DepVar_{i,l,t}$ is the total cost of borrowing by firm i from lender l in year t, and where $\delta_{i,t}$ is a *firm-year* fixed effect. The latter absorbs borrower-time-specific controls and any borrower-level

[25] We further show in the Online Appendix (Section 7) that political risk can propagate through (1) lenders' loan portfolios (portfolio effects) and (2) through networks of co-lenders (peer effects).

unobservables. We note that this design implicitly assumes the existence of credit market frictions due to which a single lender cannot meet a borrower's demand.

The results are presented in Table 6. This analysis has fewer observations due to the sample restrictions discussed above. The evidence suggests that a one standard deviation increase in lender-level political risk within a given firm-year is associated with a 16.6 (11.2) basis-point higher total cost of borrowing (all-in-drawn spread), significant at the five (ten) percent level. Thus, the effect of bank-level political shocks on the cost of borrowing holds after controlling for differences in a borrower's investment opportunities. To the extent that borrower-year fixed effects absorb firm-specific variation giving rise to differences in the choice of lenders (Khwaja & Mian 2008), the estimated effect can be plausibly attributed to differences in banks' political risk.

As the Khwaja-Mian design uses within borrower-year variation, the endogenous choice of borrowers with certain characteristics to match with certain lenders cannot explain the results. However, the drawback of this design is that it conditions on multiple loans: borrowers who borrow from more than one lender might differ from the population. This condition potentially limits the generalizability of our results. Therefore, we also implement an alternative design based on the variation within borrower-lender pairs, as discussed next.

6.2. Analysis within borrower-lender pairs.

We next exploit the credit default swap (CDS) data to examine whether CDS market participants expect and accordingly price the possible transmission of syndicate lenders' political risks to their borrowers through loan terms. This strategy addresses the endogeneity of lending relationships by focusing on changes in lenders' political risk within the same borrower-lender relationship (pair), i.e., by fixing the ex-ante unobservable borrower and lender characteristics. Credit Default Swaps (CDS) data contains monthly time series of the cost of protection against credit risk of a given company. It is important to note that CDS data is entity specific and generally applies to multiple issues, including public and private debt (we thus cannot limiting to CDS contracts for a specific laon). In a liquid market, however, the cost of protection against credit risk is effectively equivalent to the cost of borrowing for a given firm. We use this time-series variation in monthly CDS spreads within lender-borrower pairs to examine whether over-time changes in *arranger* $PRisk_{it}$ influence *borrower* CDS rates after the loan contract is in place (relationship initiated). Our CDS sample consists of U.S. nonfinancial firms and is restricted to spreads for the most commonly sold five-year contracts.

Discussing the economic mechanism behind our prediction in more detail is useful. Because creditors depend on lending relationships, when the lender's *PRisk* increases, the lender can use either implicit bargaining power or explicit contractual control rights over the borrower to pass on the political shocks to the borrower. This spillover can manifest via less borrower-friendly renegotiations, which are known to be frequent in the syndicated loan market (e.g., Roberts & Sufi 2009; Nikolaev 2018), reduced likelihood of waiving financial covenants, increased likelihood of enforcing restrictions on investments and uses of cash, etc. In turn, such outcomes are expected to increase the likelihood of adverse credit events and possibly the value of the collateral and should thus increase CDS spreads.

It is important to note that to the extent the costs of borrowing rise to the point that default becomes more likely, the lender must be compensated for these risks. Because CDS contracts are not equivalent to loan contracts, as the lenders do not directly set their terms, we cannot directly test lenders' contractual response to changes in their political risk. In many instances, the lender does not (or cannot) immediately pass on changes in their *PRisk* to borrowers when they occur but will be compensated via future re-contracting outcomes (e.g., upon future covenant violaitions). However, changes in CDS spreads reflect market expectations of the lenders' future actions, including contract modifications. These expectations should, on average, be timely and informative about lenders' actions in relation to political risk.

To implement this analysis, we run the following model specification, where $\delta_{i,j,l}$ represents the borrower-lender-pair fixed effects for each loan *l*:

$$CDS\ Spread_{i,j,l,t+1} = \beta_1 zPRisk\_Arranger_{j,t} + \delta_{i,j,l} + \varepsilon_{i,j,l,t}, \qquad (6)$$

CDS Spread is the spread on five-year credit default swaps measured in quarter $t+1$, and all other variables are defined as above.

The results are presented in Table 7.[26] [27] The estimates indicate that a one standard deviation

[26] Because we only observe loans at origination and because there may be other outstanding loans affecting CDS spread, we expand the data using loan start and end dates where we can observe *PRisk* and CDS spread at each quarter. We assume that lenders hold the loan until maturity.
[27] Note that the reported specification includes a control for the borrower's PRisk.

increase in arranger political risk within each borrower-lender-loan pair leads to an 8.2 basis-point increase in CDS spreads. These estimates are statistically significant at the one percent level and align well with our findings in our prior subsections. This evidence suggests that unobservable borrower or lender characteristics responsible for matching at loan initiation cannot explain the link between loan pricing and arrangers' $PRisk_{it}$.

These findings corroborate the evidence on transmitting lenders' political risk to borrowers. We also affirm our results based on cross-sectional regressions that measure lender portfolio-level exposure to political risk based on the aggregate *PRisk* of its borrowers and directly controlling for borrowers' political risk (see Section 7 of the Online Appendix). Furthermore, we also implement an alternative strategy to address the endogeneity of lenders' *PRisk* by returning to our redistricting shock as a source of plausibly exogenous variation in lenders' political risk. This analysis is presented in Section 11 of the Online Appendix and is in line with the findings of Sections 6.2 and 6.3.

7. Sources of lenders' political risk and their transmission channels

In our last set of tests, we shed additional light on whether and how lenders transmit the costs of their political risk to their borrowers. We provide evidence on the sources of political risk for banks and examine whether these sources are priced in the loans extended to borrowers. To do so, we return to the text of the earnings calls to learn more about what increases banks' exposure to political risk. Recall from our discussion in Section 2 that firms (including banks) can experience political risk because political events and politicians' actions impact their operations and investments. Alternatively, information asymmetries are a possible source of political risk, with some agents having superior knowledge about future political events. In addition, lenders are exposed to more particular political or regulatory actions, including varying degrees of leniency in stress testing and regulatory forbearance.

We conduct a textual analysis of the earnings calls of both lenders and borrowers to identify how their political discussions differ. We aim to tease out the specialized political language used by either financial institutions or borrowers to learn more about their specific political risk sources. Our conceptual framework suggests that the political discussions in banks' earnings calls focus more on regulators and their actions. We take advantage of the linguistics literature's toolbox and measure the "keyness" of bigrams to borrowers and lenders. We discover *key* bigrams in a corpus of all lenders' bigrams by comparing their frequencies in that corpus to that of a reference corpus (borrowers' bigrams) and measuring the relative importance of each bigram to the target corpus using a *keyness* statistic.[28] We apply keyness to determine which political risk bigrams are more important to lenders than borrowers. These distinct bigrams provide clues as to which specific sources of risk matter to lenders.

We summarize our findings in Table 8, Panel A, and Figure 6, which shows lenders' top key bigrams. These bigrams indicate lenders' concerns linked to central bank supervision, the banking system, the mortgage market and home equity, and the regulatory issues associated with Basel II and risk-based capital. Our findings are reasonable as one would expect changes in central bank policy, regulation, and (particularly in the aftermath of the financial crisis) home mortgages to be critical sources of bank political risk. The keyness results are also consistent with the theoretical framework in Section 2 that highlights sources of supply-side political risk aligned with the banking sector regulation being a key driver of firm-specific political risk. Indeed, with few exceptions, the top bigrams reflect the institutional complexities of banking regulation. For example, the bigram "Washington Mutual" refers to the savings and loan association that was placed in the receivership of the Federal Deposit Insurance Corporation during the 2008 Financial Crisis after a seizure by the Office of Thrift Supervision. "Risk management" and "Management Practices" are central pillars of the CAMELS rating system used by the US Fed and other banking regulators to assess a bank's overall health. Within CAMELS, the growth of the bank, in particular its growth in loan portfolio ("loan growth" our bigram with the highest keyness score), is a concern when evaluating capital adequacy.

Transmission channels. The keyness analysis provides textual evidence of regulation and oversight being important sources of banks' political risk. However, this analysis does not reveal whether these sources are relevant to the cost of borrowing. We further lever the opportunities offered

[28] The most commonly used keyness statistic is the log-likelihood ratio (G^2) (Dunning 1993; Rayson and Garside 2000) calculated as follows:

$$G^2 = 2\sum_i O_i \times ln\frac{O_i}{E_i}$$

Where O refers to the observed frequencies and E refers to the expected frequencies. The observed and expected frequencies are obtained by constructing a contingency table for each bigram, where the observed frequency is the actual occurrence of a given bigram and the expected occurrence is calculated as follows:

$$E_i = \frac{N_i \sum_i O_i}{\sum_i N_i}$$

Where N is the number of *all* bigrams in corpus i and O is the observed occurrence of a given bigram in corpus i.

by textual data, combined with machine learning techniques, to investigate how the political risk is transmitted from lenders to borrowers next.

Our empirical challenge is as follows. We aim to identify which political word combinations (bigrams) used during the earnings call explain loan pricing. Having identified a collection of these bigrams, we need to determine whether lenders' or borrowers' uses of those bigrams have the largest effect on loan pricing. Should the bigrams that are important for pricing stem from lenders' transcripts rather than borrowers', this finding would be consistent with lenders passing on their political risk via the cost of borrowing.

We use LASSO regressions to identify specific political bigrams (two-word combinations) that have the power to explain loan pricing. LASSO is a penalized linear regression that functions as a "selection operator" (Huntington-Klein 2022) by minimizing the sum of absolute values of regression residuals while limiting the number of estimated coefficients. LASSO helps address the problem of having too many (correlated) explanatory variables as it drops variables with little explanatory power. This property is useful in our case as we have more than 8,000 candidate political bigrams (after removing stop words) and, thus, more than 8,000 explanatory variables. The LASSO penalty parameter λ determines how important it is to reduce the number of coefficients to zero relative to the objective of minimizing the absolute deviations from the regression line. Recommended approaches to choosing the penalty parameter include selecting the value of λ that (1) minimizes an information criterion (BIC, AIC) or (2) is selected based on a k-fold cross-validation procedure. In the latter case, the sample is divided into five or ten folds, and each fold is split into training and validation groups. The model is then fitted to the training data, and the validation data are used to find λ with the best mean squared prediction error. We report a summary of using these alternative approaches to estimating LASSO in Table 8, Panel B. Ultimately, this procedure yields a set of "selected" bigrams to which the model assigns non-zero weight when explaining loan interest rates. We use the top 100 of these selected bigrams and then determine the proportion attributed to lenders (or borrowers) based on our keyness analysis above. Recall that a bigram's keyness records its relative importance to one party over the other. We report two information criteria and four cross-validation implementations of choosing the penalty parameter. In all but one approach, the share of lenders' bigrams in the top 100 is over 75 percent. In fact, under the three choices of λ, the estimated share is larger than 95 percent.

Among the highest-ranking bigrams (related to lenders) are "monetary stimulus," "domestic policy," "credit program," "senate bill," "financial reform," "American economy," and "capital standards." Once more, consistent with the theoretical framework in Section 2 that lays out regulatory factors and oversight as sources of political risk to banks, these bigrams refer to political interventions in the economy and regulatory actions that target the financial sector.[29,30] Borrower bigrams, which, as Table 8, Panel C indicates, are more sparsely represented in the top 100, are mostly related to taxes ("payroll taxes") or litigation ("court judge" and "file suit"). Given the evidence that the political risk bigrams priced in loans are overwhelmingly specific to lenders and that these bigrams refer to political risk sources particular to banks, these findings further support our conclusions that banks pass on their political risk to borrowers via the corresponding regulatory and oversight channels.

8. Conclusion

While the effects of economy-wide political shocks have received considerable attention, recent work reveals that they only reflect a small portion of a given firm's exposure to political events. There are multiple examples illustrating that political exposure is largely a firm-specific phenomenon. We build on Hassan *et al.* (2020) and use their comprehensive measure of political risk to examine how firm-level political risk affects private credit markets. We focus on two primary questions: (1) Does a firm's political risk affect its credit market outcomes? And (2) is political risk transmitted from lenders to borrowers via lending relationships?

On the borrower side, we document that firm-level political risk is positively associated with the cost of private debt while holding the aggregate and industry-time factors fixed. To further overcome identification challenges, we use a difference-in-differences design to exploit plausibly exogenous variation in a borrower's political risk induced by the changes in congressional districts stemming from

---

[29] Note that high-ranking bigrams are those often used across the sample, and macroeconomic terms would emerge at the top of the list in our algorithm. Bigrams related to idiosyncratic political risks of individual firms, on the other hand, are mechanically not very frequent and will not be visible in lists of the most prominent terms. This fact in itself, however, does not mean that aggregate political risk is more impactful than firm-level sources.

[30] This analysis reveals that the lenders react to variation in their own political risk when pricing loans, thus transmitting own political risks. For example, such variation may be caused by systematic shocks to lenders' porfolios. However, as the analysis also indicates, in many instances changes in political risk can be driven by by direct effect of political or regulatory factors on the lender (e.g., new capital requirements).

the 2010 decennial Census redistricting. We document economically significant effects of changes in political risk on the cost of debt.

On the lender side, we provide evidence of the effect of lender-level political risk on credit supply (loan pricing). We show that lender-specific political risk changes propagate to borrowers through higher interest rates, suggesting network effects amplify political events. We can rule out several alternative explanations, including the potentially confounding effect of the unobservable demand for credit and endogenous matching.

Our study shows that researchers can address econometric challenges that have previously precluded causal interpretations of aggregate political risk measures by using a more granular measure of political risk.

References:

Acemoglu, D., Bimpikis, K., Ozdaglar, A., 2014. Dynamics of information exchange in endogenous social networks. Theoretical Economics 9, 41-97

Acemoglu, D., Carvalho, V.M., Ozdaglar, A., Tahbaz-Salehi, A., 2012. The network origins of aggregate fluctuations. Econometrica 80, 1977-2016

Akey, P., 2015. Valuing Changes in Political Networks: Evidence from Campaign Contributions to Close Congressional Elections. Review of Financial Studies 28, 3188-3223

Akin, O., Coleman, N., Fons-Rosen, C., Peydro, J.-L., 2019. Political connections: Evidence from insider trading around TARP. Financial Management in press

Amiraslani, H., Lins, K.V., Servaes, H., Tamayo, A., 2019. The bond market benefits of corporate social capital. European Corporate Governance Institute (ECGI) - Finance Working Paper No. 535/2017

Angrist, J.D., Pischke, J.-S., 2009. Mostly harmless econometrics. An empiricist's companion. Princeton University Press, Princeton.

Ansolabehere, S., De Figueiredo, J.M., Snyder Jr, J.M., 2003. Why is there so little money in US politics? Journal of Economic Perspectives 17, 105-130

Autor, D., Dorn, D., Hanson, G., Majlesi, K., 2020. Importing Political Polarization? The Electoral Consequences of Rising Trade Exposure. American Economic Review 110, 3139-83

Baker, S.R., Bloom, N., Davis, S.J., 2016. Measuring Economic Policy Uncertainty. The Quarterly Journal of Economics 131, 1593-1636

Belo, F., Gala, V.D., Li, J., 2013. Government spending, political cycles, and the cross section of stock returns. Journal of Financial Economics 107, 305-324

Berg, T., Saunders, A., Steffen, S., 2016. The total cost of corporate borrowing in the loan market: Don't ignore the fees. The Journal of Finance 71, 1357-1392

Berger, A.N., Guedhami, O., Kim, H.H., Li, X., 2018. Economic policy uncertainty and bank liquidity hoarding. Available at SSRN 3030489

Bertrand, M., Bombardini, M., Trebbi, F., 2014. Is it whom you know or what you know? An empirical assessment of the lobbying process. American Economic Review 104, 3885-3920

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust differences-in-differences estimates? Quarterly Journal of Economics 119, 249-275

Besley, T., Mueller, H., 2017. Institutions, volatility, and investment. Journal of the European Economic Association 16, 604-649

Bharath, S., Dahiya, S., Saunders, A., Srinivasan, A., 2007. So what do I get? The bank's view of lending relationships. Journal of Financial Economics 85, 368-419

Bharath, S.T., Hertzel, M., 2019. External Governance and Debt Structure. The Review of Financial Studies

Blume, L., Easley, D., Kleinberg, J., Kleinberg, R., Tardos, É., 2011. Network formation in the presence of contagious risk. Proceedings of the 12th ACM conference on Electronic commerce, 1-10

Bordo, M.D., Duca, J.V., Koch, C., 2016. Economic policy uncertainty and the credit channel: Aggregate and bank level US evidence over several decades. Journal of Financial Stability 26, 90-106

Callaway, B., Sant'Anna, P.H.C., 2021. Difference-in-Differences with multiple time periods. Journal of Econometrics 225, 200-230

CHAVA, S., ROBERTS, M.R., 2008. How Does Financing Impact Investment? The Role of Debt Covenants. The Journal of Finance 63, 2085-2121

Chodorow-Reich, G., 2014. Effects of unconventional monetary policy on financial institutions. National Bureau of Economic Research

Chodorow-Reich, G., Falato, A., 2017. The loan covenant channel: How bank health transmits to the real economy. National Bureau of Economic Research

Christensen, D.M., Jin, H., Sridharan, S.A., Wellman, L., 2020a. Hedging on the Hill: Does political hedging reduce firm risk? Available at SSRN

Christensen, H.B., Macciocchi, D., Morris, A., Nikolaev, V.V., 2020b. Financial Shocks and Corporate Investment Activity: The Role of Financial Covenants. Available at SSRN 3079996

Cooper, M., Gulen, H., Ovtchinnikov, 2010. Corporate political contribution and stock returns. Journal of Finance 65, 687-724
Denes, M., Schulz, F., Vig, V., 2017. Do Political Boundaries affect Firm Boundaries? Available at SSRN 3192213
Drobetz, W., El Ghoul, S., Guedhami, O., Janzen, M., 2018. Policy uncertainty, investment, and the cost of capital. Journal of Financial Stability 39, 28-45
Faccio, M., 2006. Politically connected firms. American Economic Review 96, 369-386
Faccio, M., Masulis, R.W., McConnell, J.J., 2006. Political connections and corporate bailouts. Journal of Finance LXI, 2597-2635
Fisman, R., 2001. Estimating the value of political connections. American Economic Review 91, 1095-1102
Francis, B.B., Hasan, I., Zhu, Y., 2014. Political uncertainty and bank loan contracting. Journal of Empirical Finance 29, 281-286
Giannetti, M., Saidi, F., 2018. Shock Propagation and Banking Structure. The Review of Financial Studies 32, 2499-2540
Gilchrist, S., Sim, J.W., Zakrajšek, E., 2014. Uncertainty, financial frictions, and investment dynamics. National Bureau of Economic Research
Gopalan, R., Nanda, V., Yerramilli, V., 2011. Does Poor Performance Damage the Reputation of Financial Intermediaries? Evidence from the Loan Syndication Market. The Journal of Finance 66, 2083-2120
Gourio, F., Siemer, M., Verdelhan, A., 2015. Uncertainty and international capital flows. Available at SSRN 2626635
Handley, K., Limao, N., 2015. Trade and investment under policy uncertainty: theory and firm evidence. American Economic Journal: Economic Policy 7, 189-222
Hasan, I., Hoi, C.K., Wu, Q., Zhang, H., 2014. Beauty is in the eye of the beholder: The effect of corporate tax avoidance on the cost of bank loans. Journal of Financial Economics 113, 109-130
Hassan, T.A., Hollander, S., van Lent, L., Tahoun, A., 2019. Firm-Level Political Risk: Measurement and Effects. The Quarterly Journal of Economics 134, 2135-2202
HOUSTON, J.F., JIANG, L., LIN, C., MA, Y., 2014. Political Connections and the Cost of Bank Loans. Journal of Accounting Research 52, 193-243
Huntington-Klein, N., 2022. The effect : an introduction to research design and causality. CRC Press, Taylor & Francis Group, Boca Raton.
Ivashina, V., Scharfstein, D., 2010. Loan Syndication and Credit Cycles. American Economic Review 100, 57-61
Kara, G., Yook, Y., 2018. Policy Uncertainty and Bank Mortgage Credit. Available at SSRN 3306884
Kaviani, M., Kryzanowski, L., Maleki, H., Savor, P.G., 2020. Policy uncertainty and corporate credit spreads. Fox School of Business Research Paper
Kelly, B., Pástor, Ľ., Veronesi, P., 2016. The price of political uncertainty: Theory and evidence from the option market. The Journal of Finance 71, 2417-2480
Khwaja, A.I., Mian, A., 2008. Tracing the impact of bank liquidity shocks: Evidence from an emerging market. American Economic Review 98, 1413-42
Koijen, R.S., Philipson, T.J., Uhlig, H., 2016. Financial health economics. Econometrica 84, 195-242
Lewis, J.B., DeVine, B., Pitcher, L., Martis, K.C., 2013. Digital boundary definitions of United States Congressional districts, 1789-2012. . http://cdmaps.polisci.ucla.edu
Loughran, T., McDonald, B., 2011. When is a liability not a liability? Textual analysis, dictionaries, and 10-Ks. The Journal of Finance 66, 35-65
Merton, R.C., 1974. On the pricing of corporate debt: the risk structure of interest rates. Journal of Finance 29, 449-470
Milyo, J., Primo, D., Groseclose, T., 2000. Corporate PAC campaign contributions in perspective. Business and Politics 2, 5-14
Murfin, J., 2012. The Supply-Side Determinants of Loan Contract Strictness. The Journal of Finance 67, 1565-1601
Ng, J., Saffar, W., Zhang, J.J., 2018. Policy Uncertainty and Loan Loss Provisions in the Banking Industry. Available at SSRN 3051494

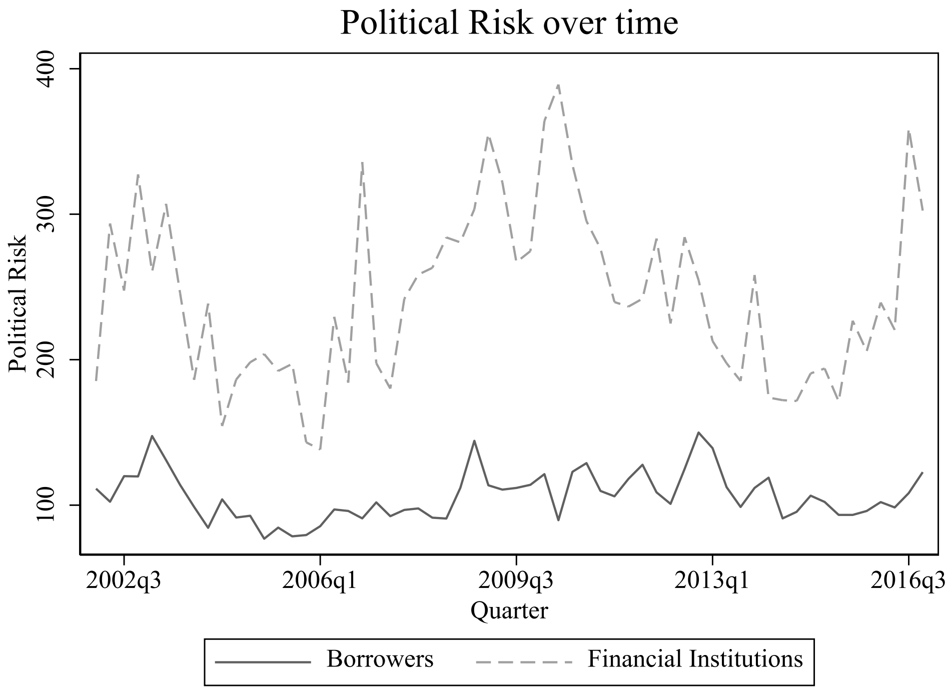
Nikolaev, V.V., 2018. Scope for renegotiation in private debt contracts. Journal of Accounting and Economics 65, 270-301
Olson, M., 1965. The logic of collective action. Harvard University Press.
Pagano, M., Volpin, P., 2001. The political economy of finance. Oxford Review of Economic Policy 17, 502-519
Pastor, L., Veronesi, P., 2012. Uncertainty about Government Policy and Stock Prices. The Journal of Finance 67, 1219-1264
Peltzman, S., 1976. Towards a more general theory of regulation? Journal of Law and Economics 19, 211-241
Pham, A.V., 2019. Political risk and cost of equity: The mediating role of political connections. Journal of Corporate Finance 56, 64-87
Rajan, R.G., 1992. Insiders and Outsiders: The Choice between Informed and Arm's-Length Debt. The Journal of Finance 47, 1367-1400
Roberts, M.R., Sufi, A., 2009. Renegotiation of financial contracts: Evidence from private credit agreements. Journal of Financial Economics 93, 159-184
Santos, J.A.C., Winton, A., 2019. Bank Capital, Borrower Power, and Loan Rates. The Review of Financial Studies 32, 4501-4541
Sharpe, S.A., 1990. Asymmetric Information, Bank Lending and Implicit Contracts: A Stylized Model of Customer Relationships. The Journal of Finance 45, 1069-1087
Tahoun, A., 2014. The role of stock ownership by US members of Congress on the market for political favors. Journal of Financial Economics 111, 86-110
Tahoun, A., van Lent, L., 2019. The Personal Wealth Interests of Politicians and Government Intervention in the Economy. Review of Finance 23, 37-74
Tullock, G., 1967. The welfare costs of tariffs, monopolies, and theft. Economic Inquiry 5, 224-232
Wang, X., Xu, W., Zhong, Z., 2019. Economic policy uncertainty, CDS spreads, and CDS liquidity provision. Journal of Futures Markets 39, 461-480
Wellman, L.A., 2017. Mitigating political uncertainty. Review of Accounting Studies 22, 217-250
Weston, J., Yimfor, E., 2018. Moral Hazard, Bank Monitoring, and Bond Spreads. Bank Monitoring, and Bond Spreads (July 23, 2018)

Table A1: Variable definitions

Variables	Description
Total Cost	Is the total cost of borrowing taking into account not only spreads but also fees (e.g. commitment fee, utilization fee, cancellation fee, etc.) based on the likelihood that each of these components will have to be paid (see Berg *et al.* (2016).
All-in-Drawn	All-in-drawn spread, defined as the spread over LIBOR plus the facility fee. (Dealscan)
zPRisk	Standardized firm-level political risk (*PRisk*) as defined in HHLT. *PRisk* is measured as the average firm-level political risk over the four quarters preceding loan origination. *PRisk* is standardized to have a mean equal to zero and a standard deviation of one.
zNPRisk	Standardized firm-level non political risk (*NPRisk*) as defined in HHLT. *NPRisk* is measured as the average firm-level non-political risk over the four quarters preceding loan origination. *NPRisk* is standardized to have a mean equal to zero and a standard deviation of one.
zPSentiment	Standardized firm-level political sentiment in a conference call, defined as in HHLT and constructed by assigning a value of +1 if the bigram is associated with positive sentiment (using Loughran and McDonald's (2011) sentiment dictionary), a value of −1 if the bigram is associated with negative sentiment, and zero otherwise.
zPRisk_Arranger	Standardized arranger-level political risk as defined in HHLT. PRisk_*Arranger* is measured as the average arranger-level political risk over the four quarters preceding loan origination.
zNPRisk_Arranger	Standardized arranger-level non-political risk as defined in HHLT. *zNPRisk_Arranger* is measured as the average arranger-level non-political risk over the four quarters preceding loan origination.
Bank dependent1	Indicator variable equal to one if the percentage borrowed from the current lead arranger(s) over the previous 3 years is at least 50% of the firm's total loan amount over the past 3 years.
Bank dependent2	Indicator variable equal to one if the borrower did not access the bond market in past 3 years.
Bank dependent3	Indicator variable equal to one if the total number of a borrower's lenders over the past four transactions is below median, zero otherwise.
ROA	Operating income before depreciation (oibdp), minus depreciation and amortization (dp), and then divided by total assets (at). (Compustat)
ΔROA	Change in ROA.
Loss	Indicator variable equal to one if ROA is negative, zero otherwise.
MTB	Market to book value of assets (at - ceq + mkvalt)/at. (Compustat)
lnMCAP	The log of the market value of equity (csho multiplied by prcc_f). (Compustat)
LEV	Long-term debt (dltt) plus debt in current liabilities (dlc) divided by total assets (at). (Compustat)
ReturnVol	Standard deviation of monthly stock returns (ret) over the past two years. (CRSP)
Zscore	Altman's (1968) Z-score = (1.2*(act-lct)/at + 1.4*re/at + 3.3*(pi)/at+0.6*mkvalt/lt + 0.999*revt/at). (Compustat)

Online Appendix variables	
Loan growth	Change in loans scaled by lagged loans: ΔTotal loans_t/Total loans_{t-1}. (FR Y-9C)
Deposit growth	Change in deposits scaled by lagged deposits: $\Delta\text{Deposits}_t/\text{Deposits}_{t-1}$. (FR Y-9C)
Bid-ask spread	The quarterly median trade-weighted bid-ask spread. (WRDS Bond Database)
Bond yield	The difference between the quarterly median yield-to-maturity and the yield of a treasury bill with matched maturity. (WRDS Bond Database)
Liquidity	Log of the total traded dollar volume divided by the total par volume. (WRDS Bond Database)
zPRisk (bond)	Standardized firm-level political risk (*PRisk*) as defined in HHLT. *PRisk* is measured as the lagged quarter firm-level political risk.
zNPRisk (bond)	Standardized firm-level non political risk (*PRisk*) as defined in HHLT. *NPRisk* is measured as the lagged quarter firm-level political risk.
Net long-term debt issuance	The net debt issuance in year t (dltis - dltr), scaled by assets (at) at the end of year $t - 1$. (Compustat)
CDS spread	The amount a protection buyer must pay a protection seller.
Recovery rate	The percentage of par value that bondholders will receive after a credit event.
lnLobby	Log of one plus the average lobby expenses over the past four quarters. (CRP)
lnDonation	Log of one plus the sum of the average Political Action Committee contributions paid to federal election candidates over the past four quarters. (CRP)
zPRisk_bhc	Standardized bank-level political risk (*PRisk*) as defined in HHLT. *PRisk* is measured as the lagged quarter bank-holding company-level political risk.
zPRisk_Portfolio	The standardized political risk from the arranger's portfolio of borrowers, where the portfolio includes all borrowers with outstanding loans originated by the current arranger. Once the portfolio of borrowers is identified, the four-quarter-average *PRisk* of each borrower starting the quarter before the current loan date (along with the count of loans for each borrower over the past three years) is used to compute the weighted average portfolio *PRisk.*
zPRisk_Network	The standardized political risk from the arranger's network (constituted of all co-lenders with whom the arranger has co-syndicated loans in the past three years, starting the quarter before the current loan date). Once the network is identified, the four-quarter-average *PRisk* of each co-lender starting the quarter before the current loan date (along with the count of joint loans with each co-lender) is used to compute the weighted average network *PRisk*.

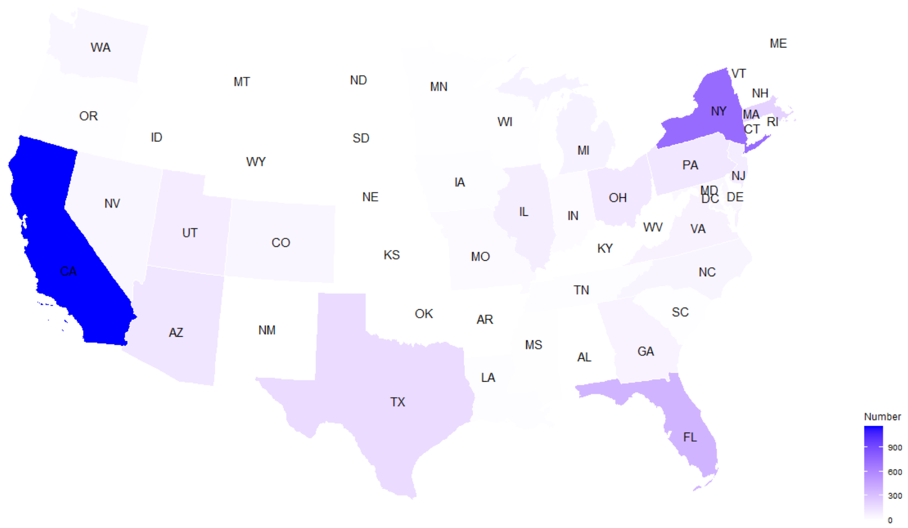
Figure 1: Fluctuations in the political risk of borrowers and financial institutions over time



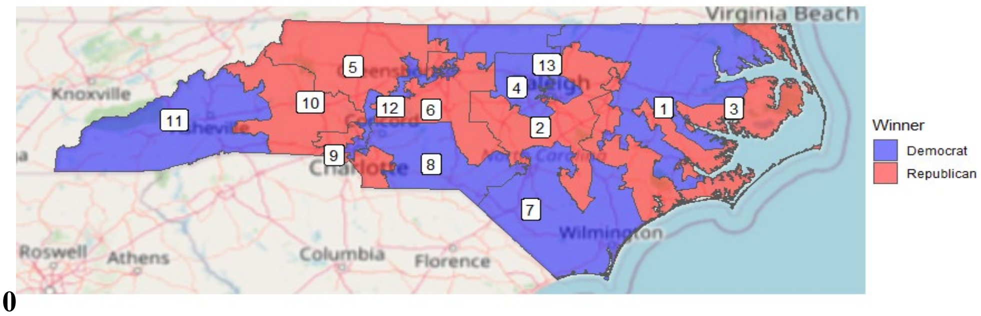
This figure shows the time-average of political risk across borrowers in each quarter along with the time-average of political risk across lenders in each quarter.

Figure 2: Redistricted firms across the US

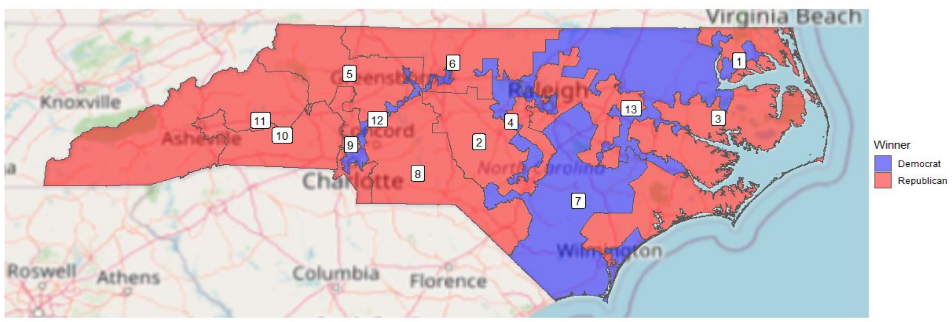
(A) Redistricted firms by state



(B) North Carolina pre- vs post-redistricting



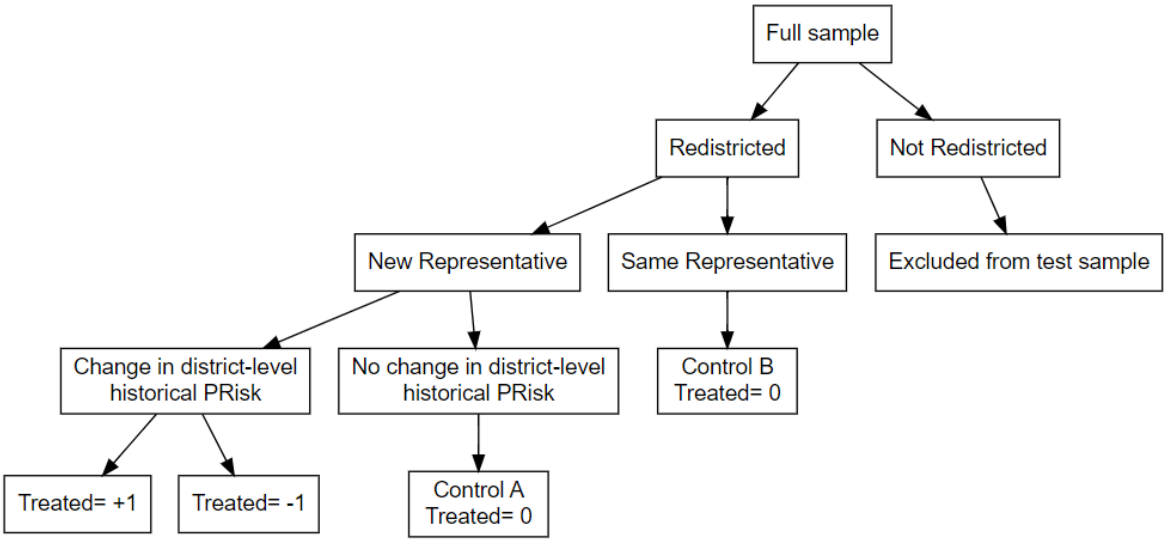
2010



2012

Panel A shows the distribution of redistricted firms across US states using the initial *PRisk* sample. Panel B shows the map of Democrat vs. republican representatives in North Carolina pre and post redistricting.

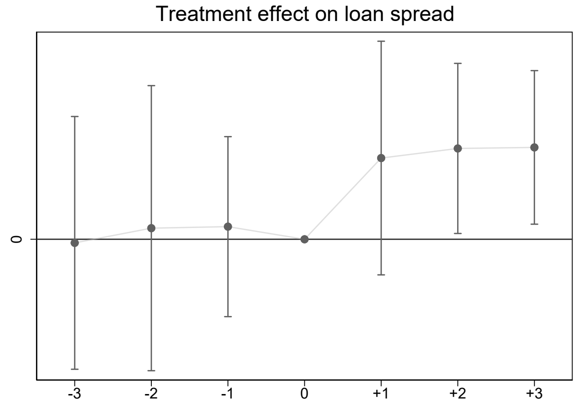
Figure 3: Treatment and control groups construction



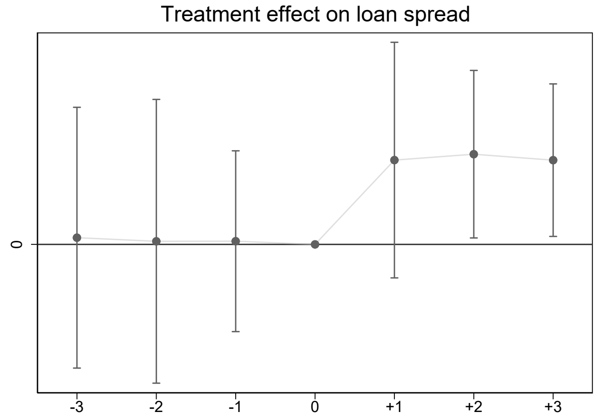
This figure summarizes our approach in identifying the treatment and control groups. The treatment indicator takes the value of 1 (-1) if (a) a firm is affected by redistricting, (b) the firm has a new house representative, and (c) the new district has historically been in a higher (lower) political risk quartile than the old district. We use the following three control groups (treatment indicator of zero): (1) firms that have a new representative but the new district has a similar level of political risk compared to the old district (it remains in the same quartile of PRisk after redistricting); (2) firms that are redistricted but continue to be represented by the same politician (i.e., when the politician moves congressional districts with the firm); and (3) the combined set of these two control groups.

Figure 4: Changes in loan spread around redistricting

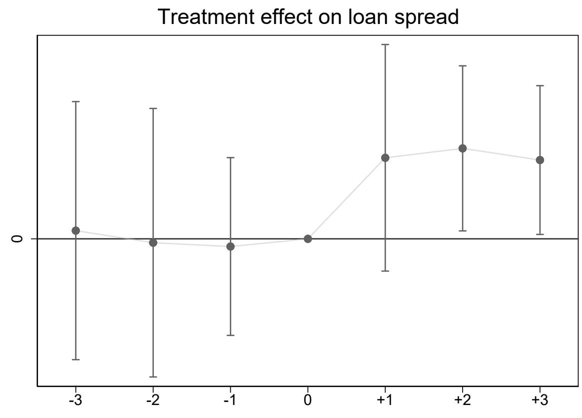
Panel A: Using firms redistricted to a different politician but within the same quartile of *PRisk* as a control group



Panel B: Using redistricted firms that keep their original representative as a control group



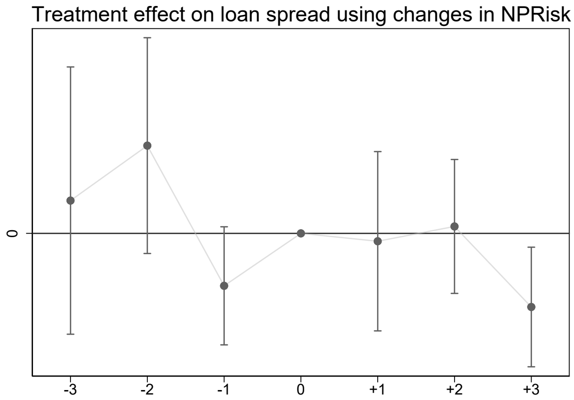
Panel C: Using the combined control group



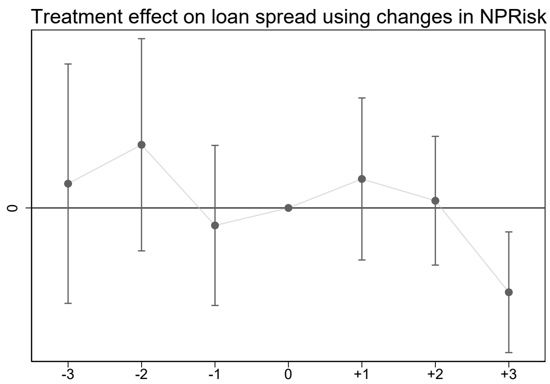
This figure plots the coefficients from a regression of *Total Cost* on dummy variables for the redistricting year (2011); it also includes leads and lags. The specification controls for year and firm fixed effects. Each graph is based on a different control group: Panel A uses firms redistricted to a different politician but within the same quartile of *PRisk*; Panel B uses firms redistricted to the same politician; and Panel C uses firms redistricted to a different politician but within the same quartile of *PRisk* and firms redistricted to the same politician.

Figure 5: Changes in loan spread around redistricting: Plaecbo effect of changes in *NPRisk*

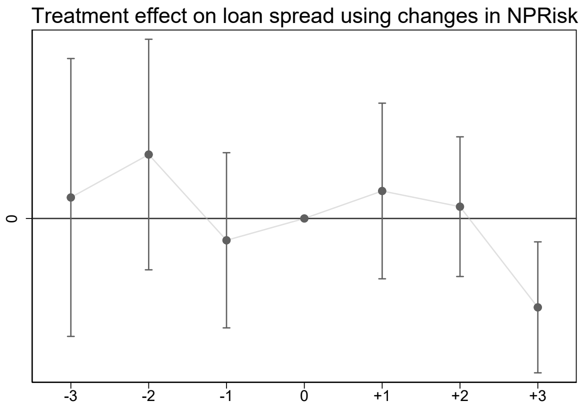
Panel A: Using firms redistricted to a different politician but within the same quartile of *NPRisk* as a control group



Panel B: Using redistricted firms that keep their original representative as a control group

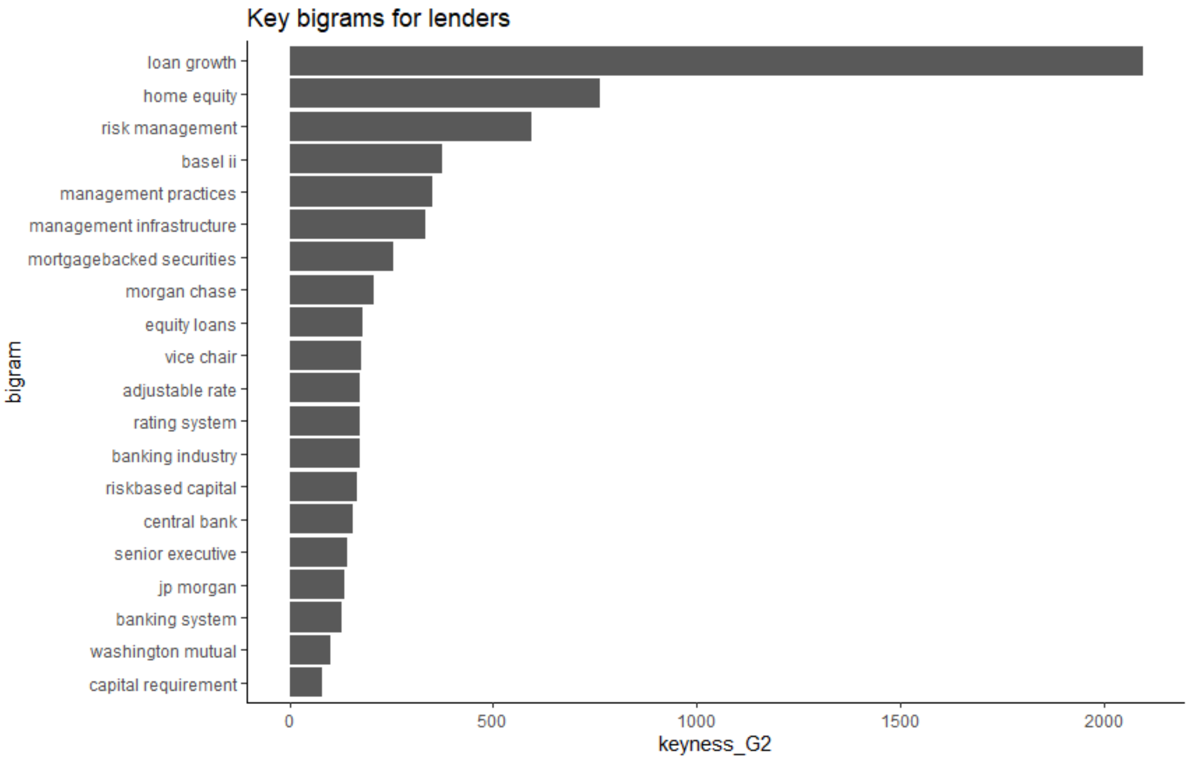


Panel C: Using combined control group



This figure plots the coefficients from a placebo regression of *Total Cost* on dummy variables for the redistricting year (2011); it also includes leads and lags. The specification controls for year and firm fixed effects. Each graph is based on a different control group: Panel A uses firms redistricted to a different politician but within the same quartile of *NPRisk*; Panel B uses firms redistricted to the same politician; and Panel C uses firms redistricted to a different politician but within the same quartile of *NPRisk* and firms redistricted to the same politician.

Figure 6: Key bigrams for lenders



The figue shows lenders' top 20 distinct bigrams according to the keyness statistic (log-likelihood ratio (G^2)). Distinct bigrams are identified by comparing frequencies in lenders' corpus of bigrams to that of a reference corpus (borrowers' bigrams) and measuring the relative importance of each bigram to the target corpus using a *keyness* statistic

Table 1: Summary statistics

	N	Mean	St.dev.	p25	Median	p75
Panel A: Loan markets (Borrower-Facility)						
Total Cost (bps)	8,526	154.826	157.058	51.346	91.141	209.283
All-in-Drawn (bps)	11,022	200.984	150.451	100	175	250
PRisk	11,022	107.75	138.377	35.856	70.557	129.034
PSentiment	11,022	1135.393	983.468	559.645	1069.186	1675.979
NPRisk	11,022	763.035	844.87	302.887	552.741	957.066
lnMCAP	11,022	7.714	1.786	6.495	7.677	8.947
ROA %	11,022	8.478	7.728	4.959	8.063	12.21
ΔROA	11,022	-.022	4.979	-1.478	.153	1.739
Loss (Indicator)	11,022	.078	.269	0	0	0
LEV %	11,022	29.404	19.973	15.358	27.477	40.374
MTB	11,022	1.724	.877	1.166	1.456	1.97
ReturnVol	11,022	11.179	6.363	6.796	9.56	13.66
Zscore	11,022	3.105	3.392	1.369	2.555	4.079
Panel B: Loan markets (Lender-Facility)						
Total Cost	15,984	143.286	138.874	51.292	85.382	194.9
All-in-Drawn	20,137	186.338	129.332	100	150	250
PRisk Arranger	20,137	231.307	152.931	129.349	191.042	284.823
PSentiment Arranger	20,137	409.454	872.347	-135.692	475.478	923.808
NPRisk Arranger	20,137	1562.226	716.232	1064.444	1438.73	1977.891
lnMCAP	20,137	8.274	1.718	7.115	8.26	9.491
ROA	20,137	8.905	7.176	5.319	8.225	12.13
ΔROA	20,137	-.071	4.523	-1.329	.135	1.509
Loss	20,137	.054	.226	0	0	0
LEV	20,137	31.003	19.612	17.811	28.816	41.645
MTB	20,137	1.73	.864	1.185	1.462	1.969
ReturnVol	20,137	10.112	5.735	6.255	8.674	12.245
Zscore	20,137	2.909	3.055	1.289	2.399	3.831

This table provides descriptive statistics. Data in Panel A are at the firm level, data in Panel B are at the arranger level, and variables are defined in Appendix Table A1.

Table 2: Political risk and loan markets: Panel data analysis

VARIABLES	(1) *Total Cost*	(2) *Total Cost*	(3) *All-in-Drawn spread*	(4) *All-in-Drawn spread*
zPRisk	8.314***	5.105**	6.781***	3.845*
	(3.41)	(2.12)	(3.52)	(1.93)
zPSentiment	0.897	-0.675	-1.898	-3.051*
	(0.45)	(-0.33)	(-1.20)	(-1.84)
zNPRisk	-2.120	-1.555	-2.459*	-1.567
	(-1.24)	(-0.93)	(-1.78)	(-1.19)
lnMCAP	-18.323***	-47.328***	-23.608***	-41.869***
	(-11.06)	(-9.82)	(-17.74)	(-11.83)
ROA	-2.769***	-0.952	-2.119***	-0.816*
	(-5.63)	(-1.55)	(-5.82)	(-1.86)
dROA	-0.600	-0.732	-0.007	-0.118
	(-1.34)	(-1.58)	(-0.02)	(-0.30)
Loss	43.560***	20.760	31.635***	22.019**
	(3.47)	(1.60)	(3.19)	(2.21)
LEV	1.856***	0.978***	1.147***	0.572***
	(12.17)	(4.25)	(10.02)	(3.12)
MTB	12.938***	9.243*	6.509**	4.349
	(3.23)	(1.90)	(2.00)	(1.16)
ReturnVol	3.355***	0.972*	4.020***	1.497***
	(6.57)	(1.83)	(8.80)	(2.68)
Zscore	-0.549	0.697	-1.100	0.225
	(-0.50)	(0.47)	(-1.51)	(0.25)
Observations	8,526	7,936	11,022	10,381
R-squared	0.350	0.637	0.411	0.644
Industry × Year FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes

This table reports the effect of firm-level political risk on loan pricing. The dependent variable in Columns 1 and 2 is the total cost of borrowing (*Total cost*). The dependent variable in Columns 3 and 4 is the all-in-drawn spread (*All-in-drawn*). The main independent variable is the standardized firm-level political risk (*PRisk*) defined in HHLT. *PRisk* is measured as the average firm-level political risk over the four quarters preceding loan origination; the prefix '*z*' indicates that the measure is standardized. For each dependent variable, we estimate a specification with industry-year fixed effects and a specification with industry-year and firm fixed effects. Data are at the borrower level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the borrower level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Redistricting and political risk

Panel A: Sample distribution

Sample	*All firms*	*Redistricted*	*Redistricted with a different politician*	*Redistricted with the same politician*
Full PRisk sample	3,299	1,491	844	647
Loan market sample	1,486	587	363	224

Panel B: redistricting and change in politicians

	Firms	*Change in politicians*	*Change in party*
Redistricted	1491	844 (57%)	311 (21%)
Not Redistricted	1808	465 (26%)	117 (6%)
	3299	1309	428

Panel C: redistricting and change in Political Risk

VARIABLES	(1) *PRisk*	(2) *PRisk*	(3) *PRisk*
Treated1 × After	16.968**		
	(2.42)		
Treated2 × After		16.892**	
		(2.42)	
Treated3 × After			16.699**
			(2.38)
Observations	18,140	27,904	32,433
R-squared	0.330	0.323	0.307
Time FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Panel A shows the sample distribution. Panel B reports the number of firms that experience a change in politicians/party post-redistricting. Panel C reports the difference-in-difference analysis of redistricting and political risk using the full PRisk sample. *Treated* is a categorical variable that takes the value of one if political risk increased due to redistricting, -1 if political risk decreased due to redistricting, and zero if political risk did not change (control group). We use three distinct control groups: redistricted firms that remain in the same quartile of firm-level political risk (Treated1), redistricted firms that keep their original representative (Treated2), and a combination of the previous two control groups (Treated3).

Table 4: Redistricting, political risk, and debt pricing

Panel A: Sample distribution
Unique firms

Treatment	*Sample 1*	*Sample 2*	*Sample 3*
1	138	138	138
-1	123	123	123
0	102	224	326

Panel B: Using redistricted firms that remain in the same quartile of political risk as a control group

VARIABLES	(1) *Total Cost*	(2) *Total Cost*	(3) *All-in-Drawn Spread*	(4) *All-in-Drawn Spread*
Treated × After	24.994**	22.691**	16.220**	16.909**
	(2.36)	(2.18)	(2.08)	(2.08)
Controls	No	Yes	No	Yes
Observations	956	882	1,226	1,109
R-squared	0.687	0.715	0.672	0.703
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Panel C: Using redistricted firms that keep their original representative as a control group

VARIABLES	(1) *Total Cost*	(2) *Total Cost*	(3) *All-in-Drawn Spread*	(4) *All-in-Drawn Spread*
Treated × After	24.614**	26.693**	15.594**	19.419**
	(2.32)	(2.58)	(2.01)	(2.48)
Controls	No	Yes	No	Yes
Observations	1,259	1,150	1,614	1,460
R-squared	0.689	0.727	0.692	0.729
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Panel D: Combination of the two control groups

VARIABLES	(1) *Total Cost*	(2) *Total Cost*	(3) *All-in-Drawn Spread*	(4) *All-in-Drawn Spread*
Treated × After	24.969**	24.905**	15.874**	17.976**
	(2.38)	(2.42)	(2.06)	(2.29)
Controls	No	Yes	No	Yes
Observations	1,513	1,384	1,948	1,761
R-squared	0.682	0.705	0.676	0.712
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Panel A shows the sample distribution. Panels B through D report the coefficient estimates for the difference-in-difference regression on the cost of debt. The dependent variable *Total cost* is the total cost of borrowing. The dependent variable *All-in-drawn spread* is the all-in-drawn spread. *Treated* is a categorical variable that takes the value of one if political risk increased due to redistricting, -1 if political risk decreased due to redistricting, and zero if political risk did not change (control group). We use three distinct control groups: redistricted firms that remain in the same quartile of firm-level political risk (Panel B), redistricted firms that keep their original representative (Panel C), and a combination of the previous two control groups (Panel D). *Controls* indicates the inclusion of control variables described on page 12 of the paper. Data are at the borrower level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the district level, are in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Lender's political risk and loan pricing

Panel A: The transmission effect

VARIABLES	(1) *Total Cost*	(2) *Total Cost*	(3) *All-in-Drawn Spread*	(4) *All-in-Drawn Spread*	(5) *Total Cost*	(6) *All-in-Drawn Spread*
zPRisk_Arranger	9.842***	2.893**	6.912***	2.970***	3.395**	4.959**
	(2.98)	(2.50)	(3.44)	(3.57)	(2.29)	(2.43)
zPRisk_Borrower	5.073***	4.706***	4.553***	4.224***	5.716**	6.593***
	(3.38)	(3.25)	(3.89)	(3.67)	(2.60)	(4.63)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Controls	No	No	No	No	Yes	Yes
Observations	15,984	15,974	20,137	20,129	4,994	6,623
R-squared	0.398	0.442	0.474	0.500	0.472	0.517
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	Yes	No	Yes	Yes	Yes

Panel B: The effect of outside options

VARIABLES	(1) *Total Cost*	(2) *All-in-Drawn Spread*	(3) *Total Cost*	(4) *All-in-Drawn Spread*	(5) *Total Cost*	(6) *All-in-Drawn Spread*
zPRisk_Arranger	-3.263	-1.816	-1.429	-0.495	1.618	1.615*
	(-1.46)	(-1.36)	(-0.94)	(-0.47)	(1.35)	(1.75)
Bank_dependent1	12.769***	12.063***				
	(3.75)	(5.11)				
zPRisk_Arranger× Bank_dependent1	9.894***	7.871***				
	(2.94)	(3.33)				
Bank_dependent2			5.559**	0.882		
			(2.10)	(0.41)		
zPRisk_Arranger× Bank_dependent2			6.848***	5.120***		
			(3.16)	(3.16)		
Bank_dependent3					41.058***	23.225***
					(5.70)	(4.51)
zPRisk_Arranger× Bank_dependent3					10.941**	8.015**
					(2.12)	(2.25)
zPRisk_borrower	4.758***	4.242***	3.071	4.013***	4.150**	3.382***
	(3.35)	(3.72)	(1.55)	(2.76)	(2.64)	(2.69)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,974	20,129	11,152	13,553	14,622	18,268
R-squared	0.444	0.502	0.491	0.549	0.465	0.518
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel A reports the effect of the lender's political risk on loan pricing. The dependent variable in Columns 1 and 2 is the total cost of borrowing (*Total cost*). The dependent variable in Columns 3 and 4 is the all-in-drawn spread (*All-in-drawn*). The main independent variable is the standardized arranger-level political risk as defined in HHLT. *PRisk_Arranger* is measured as the average arranger-level political risk over the four quarters preceding loan origination. The prefix '*z*' indicates that the measure is standardized. For each dependent variable, we estimate a specification with industry-year fixed effects and a specification with industry-year and bank fixed effects. *Controls* indicates the inclusion of control variables described on page 12 of the paper. *Bank controls* include bank size, reputation, ROA, Tier1, non-performing loans, loans to assets, deposits to assets and assets growth. Panel B reports the effect of relationship-based lending on the relation between a lender's political risk and loan pricing. The dependent variable in Column 1 is the total cost of borrowing (*Total cost*). The dependent variable in Column 2 is the all-in-drawn spread (*All-in-drawn*). The main independent variable is the standardized arranger-level political risk as defined in HHLT. *PRisk_Arranger* is measured as the average arranger-level political risk over the four quarters preceding loan origination. The prefix '*z*' indicates that the measure is standardized. *Bank dependent1* is an indicator equal to one if the percentage borrowed from the current lead arranger(s) over the previous 3 years is at least 50% of the firm's total loan amount over the past 3 years, zero otherwise. *Bank dependent2* is an indicator equal to one if the borrower did not access the bond market in past 3 years, zero otherwise. *Bank dependent3* is an indicator variable equal to one when the total number of a borrower's lenders over the past four transactions is below the median, zero otherwise. *Controls* indicates the inclusion of control variables described on page 12 of the paper. All results are estimated with industry-year and bank fixed effects. Data are at the arranger level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the arranger level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Lender's political risk and loan pricing: Within firm-year results

VARIABLES	(1) *Total Cost*	(2) *All-in-Drawn*
zPRisk_Arranger	16.573**	11.243*
	(2.23)	(1.82)
Observations	2,213	2,213
R-squared	0.734	0.797
Firm × Year FE	Yes	Yes

This table reports the effect of the lender's political risk on loan pricing using a firm-year fixed effect approach; this approach tests whether a firm that borrows from multiple banks in a given year experiences larger loan costs from the bank facing greater political risk. The dependent variable in Column 1 is the total cost of borrowing (*Total cost*). The dependent variable in Column 2 is the all-in-drawn spread (*All-in-drawn*). The main independent variable is the standardized arranger-level political risk as defined in HHLT. *PRisk_Arranger* is measured as the average arranger-level political risk over the four quarters preceding loan origination. The prefix '*z*' indicates that the measure is standardized. We estimate the relationship with firm-year fixed effects. Data are at the arranger level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the borrower level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 7: CDS pricing and political risk within arranger-borrower pairs.

VARIABLES	(1) *CDS Spread*	(2) *CDS Spread*
zPRisk_Arranger	11.800***	8.088***
	(3.16)	(2.78)
Observations	281,202	281,179
R-squared	0.025	0.511
Year FE	Yes	Yes
Pair FE	No	Yes

This table reports the effect of political risk on the CDS spread within lender-borrower pairs. The dependent variable is the amount a protection buyer has to pay a protection seller (*CDS Spread*). The main independent variable is the standardized arranger-level political risk defined in HHLT. *PRisk_Arranger* is measured as quarterly arranger-level political risk. Each political risk quarter is merged with the following three-month CDS spread. The model includes for *zPRisk_borrower* to control for time variation in borrowers' political risk not captured the parir fixed effects. *zPRisk_Borrower* is measured as quarterly borrower-level political risk. Each political risk quarter is merged with the following three-month CDS spread. The prefix '*z*' indicates that the measure is standardized. The regression model includes a control for *zPRisk_Borrower*. We estimate a specification with year fixed effects and a specification with year and pair fixed effects. All variables are defined in Appendix Table A1. Robust t-statistics, clustered at the borrower level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Textual evidence of transmission of political risk from lenders to borrowers

Panel A: Lenders' key bigrams

Bigram	Keyness
loan growth	2096.2
home equity	764.09
risk management	596.18
basel ii	374.51
management practices	351.77
management infrastructure	333.32
mortgagebacked securities	255.23
morgan chase	206.4
equity loans	179.48
vice chair	176.27
adjustable rate	174.44
rating system	174.14
banking industry	173.87
riskbased capital	165.84
central bank	154.14
senior executive	141.39
jp morgan	136.84
banking system	129.21
washington mutual	102.26
capital requirement	81.25
federal reserve	76.89
international banking	72.83
financial system	71.33
mortgage market	70.11
policy committee	67.89
longterm rates	65.49
capital planning	58.72
home loans	51.11
economic growth	50.17

Panel B: Lasso results

λ method	Share of lenders bigrams in the top 100
Cross validation 5 folds (minimum MSE)	80%
Cross validation 5 folds (minimum MSE+1se)	99%
Cross validation 10 folds (minimum MSE)	75%
Cross validation 10 folds (minimum MSE+1se)	98%
Information criterion (BIC)	97%
Information criterion (AIC)	39%

Panel C: LASSO's top bigrams (five-fold cross-validation)

Lenders	**Borrowers**
monetary stimulus	permanent tax
domestic policy	court judge
credit program	payroll taxes
senate bill	home health
bank lending	special committee
financial reform	current trends
american economy	file suit
deliberate actions	
economic condition	
capital standards	
monetary policy	
money markets	
minimum capital	
home ownership	

Panel A shows the top *key (distinct)* bigrams used by lenders sorted by the *keyness* statistic (the log-likelihood ratio (G^2). Panel B shows the relative importance of lenders vs borrowers bigrams in explaining loan pricing. We use LASSO regressions to identify specific political bigrams (two-word combinations) that have power to explain loan pricing using different penality parameters λ that (1) minimizes an information criterion (BIC , AIC) or (2) selected based on 5(10)-fold cross-validation procedure. Panel C shows the top bigrams based on five-fold cross- validation.

Online Appendix
to
Firm-Level Political Risk and Credit Markets
by
Mahmoud Gad, Valeri Nikolaev, Ahmed Tahoun, and Laurence van Lent

In this online supplement, we discuss analyses omitted from the main text for brevity.

1. Public bond market analysis

To supplement our findings on the private loan market from the paper's main text, we examine the association of firm-level political risk with the cost of borrowing and our proxies for information asymmetry in the bond market. We conduct this analysis using the TRACE data on the WRDS Bond Returns database. We also use bond data from Mergent's Fixed Income Securities Database (FISD). Because bond-level data is trade-by-trade, we measure the median yield, bid-ask spread, and trading volume in a given quarter. Following prior studies (e.g., Weston & Yimfor 2018; Amiraslani *et al.* 2019), we exclude bonds that are variable, perpetual, in a foreign currency, preferred, puttable, convertible, exchangeable, or have credit enhancements in addition to private placements. The final bond-market sample consists of approximately 150,000 firm-quarter observations from 1,515 firms.

We examine three outcome variables: the quarterly median bid-ask spread (*Bid-ask spread*), the difference between the quarterly median yield-to-maturity and the yield of a treasury bill with a matched maturity (*Bond yield*), and liquidity (*Liquidity*), defined as the natural logarithm of the quarterly median total traded dollar volume that is divided by the total par value. Table OS1 presents two sets of results for each outcome variable. In Columns 1, 3, and 5, the specification exploits the firm-level variation in $PRisk_{it}$ by controlling for sector-time fixed effects, i.e., the identifying variation is across firms within a sector and within a firm over time. We also report a specification in Columns 2, 4, and 6 that controls for permanent differences across firms in a given sector by including firm fixed effects, which implies that the identifying variation in these regressions comes from changes in political risk within a firm.

We find a robust positive association between $zPRisk_{it}$ and bid-ask spreads in Columns 1 and 2. A one standard deviation increase in firm-level political risk is associated with a 1.15 basis-point (*t-value* = 3.04) increase in bid-ask spread or an increase relative to the sample median of about 2.3 percent. After controlling for permanent differences across firms (i.e., firm-fixed effects), the estimate is about 50 percent smaller but still significant at the ten percent level, consistent with the idea that non-permanent changes in firm-level political risk are priced. The results are similar when examining bond yield in Columns 3 and 4; we find a strong positive association between bond yields and political risk. The coefficient estimates are 2.5 times larger for the specification with permanent and changing firm-level political risk than for the specification where we only control for permanent differences. A one standard deviation change in firm-level political risk leads to a 7.4 basis-point (*t-value* = 3.32) increase in bond yields, equivalent to about a 5.4 percent increase relative to the sample median. Finally, we consider *Liquidity* in Columns 5 and 6. In Column 5, we find a coefficient estimate of -0.002 (significant at the five percent level) on $zPRisk_{it}$, implying that trading volumes are negatively associated with firm-level political risk. Column 6 shows that the estimate is -0.001 (significant at the five percent level) when considering within-firm changes in firm-level political risk.

These findings support that firm-level political risk is priced in bond markets. The effect of measured political risk on bond prices comes partially from changes in firm-level political risk over time; the rest stems from permanent differences in political risk across firms. Despite including a comprehensive set of controls, the residual variation in political risk is not entirely exogenous, so these results should be interpreted cautiously.

2. Debt issuance

The analyses in Table 2 and OS1 suggest that a borrower's political risk affects debt-market outcomes along the intensive margin, i.e., through the cost of borrowing. This begs the question of whether political risk affects credit markets along the extensive margin, i.e., on debt issuance decisions. More specifically, we examine the association of *PRisk* and net long-term debt issuance as a percentage of total assets using the approach in Equation (1). In an ideal experiment, we would use a sample of firms that *intend* to access debt markets, examining how the realization of this intention varies with firms' political risk. Unfortunately, we do not observe firms' intentions, so we err on caution and construct our sample to include all Compustat firms for which we have *PRisk* data. Because these firms will not all consider accessing the market simultaneously, this design choice works against us finding an association between *PRisk* and debt issuance.

Table OS2 presents the results. As above, we show the analysis with and without firm-level fixed effects because we are interested in variation across and within firms. When we do not include firm fixed effects (Column 1), the coefficient on *zPRisk* is negative and statistically significant (-0.001). The economic magnitude is moderate, consistent with the attenuation effect discussed above. A one standard deviation increase in political risk is associated with a decrease in net debt issuance of 0.1 percent of total assets. The statistical significance disappears in Column 2 when we isolate cross-sectional variation, which is not surprising given the discussion above. However, the economic magnitude remains similar.

We find evidence consistent with the hypothesis that firm-level political risk affects credit markets along the intensive and extensive margins. The evidence suggests that firm-level (*borrower*) political risk has a robust and economically meaningful association with pricing in credit markets, even after we include several firm-level controls, such as political sentiment.

3. Exposure to aggregate political risk

This section examines the possibility that our firm-level proxy for political risk captures heterogeneous exposure to *aggregate* political uncertainty and that we capture creditor response to overall political uncertainty. The evidence in HHLT is inconsistent with this possibility, as it shows that aggregate political risk only accounts for a small (less than a hundredth) part of the firm-level variation in political risk. To further rule out this explanation, we return to Tables 2 and OS1 and add two alternatives measures of exposure to aggregate political uncertainty (or "political risk beta"): (1) $EPUbeta_i$, which is obtained from a regression of a firm's daily stock returns on BBD's daily EPU Index, and (2) a time-varying beta based on EPU, $EPUbeta2yr_{it}$, which is obtained by running the same regressions using observations from the two years before t that are based on a rolling estimation window. This second measure allows us to include more over-time flexibility in the firm-specific loadings. Panels A and B of Table OS3 show that our results are unaffected when controlling for a firm's exposure to aggregate political risk. Furthermore, exposure to aggregate risk (measured at the firm level or based on a two-year rolling window) does not exhibit a statistically significant positive association with private or public debt costs.

4. Persistent political risk and credit markets

Throughout our borrower-level analysis, we have presented results with and without firm fixed effects to accommodate the idea that some variation in firm-level political risk is persistent and some is time-varying. We now examine whether the persistent firm-level political risk has a more pronounced effect on debt market outcomes. The pricing response of long-term debt to transitory fluctuations in political risk should be weaker because market participants are likely to anticipate that ups and downs will revert to the mean. To isolate the persistent component in firm-level political risk, we measure average *PRisk* over the five years preceding the measurement of our outcome variables and examine whether these variables affect the cost of debt captured by bond yields and loan interest rates. We use the same regression model as Equation (1) and cluster standard errors at the firm level. Because we require five years of data on political risk, the sample used in this analysis is smaller than in Equation (1). We do not include firm-fixed effects since five-year averages are (by construction) highly persistent.

Table OS4 presents the results of this analysis, which indicate that a change of one standard deviation in persistent firm-level political risk leads to a significant increase in the cost of debt financing (measured across three different markets). In particular, the coefficient of interest associated with bond yields is 9.89 (*t-value* = 2.47), which is considerably higher than the corresponding estimate of 7.4 in Table OS1. Similarly, the effects of political risk on the cost of borrowing and the all-in-drawn spread for the private debt market are 10.13 (*t-value* = 2.99) and 9.18 (*t-value* =3.45), respectively. These magnitudes are higher than the corresponding coefficient estimates based on the $PRisk_{it}$ measured in the most recent year (quarter). The findings are consistent with the economic intuition that persistent firm-level political risk is priced by credit markets rather than by temporary fluctuations.

5. Borrower's active political risk management

In this section, we perform an additional and largely exploratory analysis. Given the pervasive evidence for the effect of the borrower- and lender-level political exposure on credit market outcomes, the question naturally arises of whether firms can reduce the adverse impact of political risk. To this end, we investigate whether politically active borrowers can manage the effect of political risk.

We explore two potential avenues for how firms could manage their firm-level political exposure. We hypothesize that *borrowers* manage their political exposure through direct participation in the political process, either through lobbying or by donating campaign money through Political Action Committees (PACs) (Olson 1965; Tullock 1967; Peltzman 1976; Cooper *et al.* 2010; Tahoun 2014).[31]

We start by using two proxies to examine borrowers' political activism: *lnLobby* is the natural logarithm of a borrower's lobbying expenses, and *lnDonation* is the natural logarithm of a borrower's total PAC donations. We return to Equation (1) and interact both of these measures with the borrower's annualized political risk, $zPRisk_{it}$. The results from this interaction term provide evidence on whether borrowers who engage in political activities can obtain lower loan pricing. Standard errors are clustered at the borrower level.

In Table OS5, Column 1, we show that lobbying is associated with a muted relation between borrowers' political risk and loan pricing. The estimated coefficient on the interaction term is -0.71 (*t-value* = -1.66). We find a similar result when we use borrowers' campaign donations as a proxy for political activism in Column 3. The interaction term has a negative, significant coefficient (-1.16, *t-value* = -2.87), suggesting a weak relation between political risk and *Total cost* for donating borrowers. We find similar results when we use *All-in-drawn* in Columns 2 and 4, though the estimated interaction coefficient is no longer significant in Column 4. While we cannot draw a causal conclusion, the results suggest that politically active companies are able to mitigate political risk.[32]

5.1. Additional analysis: Partisan PRisk Management

Our setting can also address a long-standing question from the literature on political connections: whether political relationships help firms manage political risk (as we assume above) or whether they are a source of political risk. We start by identifying firms that appear to donate to only one party. More specifically, the time-varying variable *Partisan* captures the group of firms that are, in

[31] While political activism can take many shapes, political science research tends to use lobbying and PAC donations as *pars pro toto* (Milyo *et al.* 2000; Ansolabehere *et al.* 2003). The benefit of borrowers' political participation likely extends to favorable outcomes other than mitigating the pricing effects of political risk in credit markets.

[32] Due to data limitations, we do not examine the effect of political activism by lenders.

a given year, in the top quartile of the distribution of the absolute difference between donations to Republican and Democratic political campaigns. *Non-partisan* firms use more moderate donation strategies, such as giving to both parties simultaneously.[33] Our intuition is that non-partisan firms are more likely to use their political donations to manage political risk by increasing their access to political decision-makers, regardless of which party is in power. Firms that connect only with one particular political party are more likely to expect other benefits (beyond risk management) from building these political relationships.

For this reason, we augment our regressions in Table OS5 by including the three-way interaction term *zPRisk* × *lnDonations* × *Partisan* and by having the associated lower-order terms (Columns 5 and 6). Donating to political campaigns significantly lowers the extent to which political risk is priced in debt contracts for non-partisan firms—consistent with the idea that lenders consider political giving as a way to manage risk as long as the giving is not severely skewed to a single party. When firms pursue a *partisan* donation strategy, however, we find a significantly stronger debt pricing response to their campaign donations in relation to political risk. We interpret this latter finding as lenders viewing partisan political activity as a source of political risk instead of a risk mitigation strategy.

6. Lender firm-level political risk and credit supply

Having documented in our main analyses that borrower-level political risk is priced in loan markets, the next question is whether financial institutions with higher levels of political risk also have a lower credit supply, suggesting slower loan growth. This could happen if political risk affects the perceptions of a bank's ability to comply with capital requirements and regulatory scrutiny or if it changes the perceived likelihood of a depositor run. Bank depositors are likely to avoid banks exposed to an elevated level of political risk. Thus, we also examine whether political risk is associated with lower deposit growth. We use the following empirical specification estimated at the *lender* level:

$$DepVar_{it} = \alpha_i + \beta_1 zPRisk_{it} + \zeta X_{it} + \delta_t + \varepsilon_{it}, \quad (6)$$

where *DepVar* is either *Loan growth*, defined as the change in loans scaled by lagged loans, or *Deposit growth*, defined as the change in deposits scaled by lagged deposits. *zPRisk* is defined as above, and *X* is a vector of control variables that includes salient bank characteristics like *zPSentiment*, *zNPRisk, Tier 1 Capital Ratio*, *Asset Risk*, *lnAssets,* and profitability (*ROA*). α_i denotes bank fixed effects, and δ_t is the quarterly time fixed effects. Bank and year-quarter fixed effects control for variation in the demand for credit at the aggregate level and across lenders, respectively. To analyze the effect of political risk on loan supply/deposit growth, we use information from the quarterly bank-holding company reports (FR Y-9C reports) filed with the Federal Reserve. We use the PERMCO-RSSD links from the Federal

[33] HHLT refer to this donation pattern as "hedging" and show the first evidence of the association between political activism and political risk; see also Christensen (2020a).

Reserve Bank of New York website to identify each bank's GVKEY, which we then link to HHLT's political risk data. This yields a final sample of 4,479 quarterly observations.

Table OS6 reports the results of these regressions. In Column 1, we document a negative association between $zPRisk_{it}$ and loan growth in the standard specification that controls for changes in the aggregate demand for credit by including year-quarter fixed effects. Within-lender variation in $PRisk_{it}$ is mostly responsible for this result (unlike the results for the borrower-level political risk); when we add bank fixed effects in Column 2, the coefficient estimate on $zPRisk_{it}$ is almost unaffected (-0.002 in both Columns 1 and 2). Both coefficient estimates are significant at the one percent level. Column 4 also finds a significant negative association (at the five percent level) between lender-level political risk and deposit growth. The coefficient is similar in magnitude (but no longer significant at conventional levels) when considering the specification without bank fixed effects in Column 3. In terms of economic significance, when we focus on the specification that controls for persistent differences in political risk between banks, we find that a one-standard-deviation increase in $zPRisk_{it}$ is associated with a 0.002 (0.003) percent decrease in loan (deposit) growth, or about a 16.6 (15.3) percent decrease relative to the sample median.

In sum, the loan- and deposit-growth regressions provide evidence supporting our conjecture that *lender*-level political risk is a determinant of the credit supply. However, it is important to recognize that to the extent that political risk varies with over-time fluctuations in demand for bank-level credit, the evidence cannot be interpreted as causal.

7. Further evidence of network effects: Sources of political risk

In the main paper, we document that the political risk of lead arrangers can be pushed to borrowers through increased loan prices when borrowers cannot easily switch to another lender. Here, we provide more evidence on the network effects of political risk by investigating two channels that could transmit political risk across market participants. Specifically, while lenders' political risk can come directly from politicians and regulators, it can also propagate through (1) lenders' loan portfolios (portfolio effects) and (2) through networks of co-lenders (peer effects).[34] To provide evidence on these potential channels, we construct the two following variables: *zPRisk_Portfolio*, which captures the political risk from an arranger's portfolio of borrowers (i.e., all the loans the arranger originated over the past three years), and *zPRisk_Network*, which reflects the political risk associated with all co-lenders in a given arranger's *network* (i.e., banks where the lead arranger has co-syndicated loans over the past three years). Once the portfolio of loans and the associated borrowers are identified, we weigh each borrower's annualized $PRisk_{it}$ (measured at the end of the quarter before the current loan) by the count of borrower loans in the arranger's portfolio. The political risk associated with the lead arranger's

[34] Recall that lenders form relationships with other lenders, which can expose lead lenders to the political risks of their partners

network is computed by weighing the annualized $PRisk_{it}$ of each co-lender (measured at the end of the quarter before the current loan) by the count of co-syndicated loans. We include portfolio and network proxies to explain the cost of borrowing and to control for borrower- and arranger-level political risk.

Table OS7 presents the results. We find a significant positive association between the political risk from lenders' portfolios and the cost of borrowing. Because we obtain this result while controlling for direct borrower- and lender-level political risk, the effect appears to be driven by variation in political risk across banks. More specifically, in Column 1, the coefficient on *zPRisk_Portfolio* is 10.018 ($t = 2.14$). However, when we account for persistent differences in arrangers' loan portfolios in Column 2, the coefficient on *zPRisk_Portfolio* is no longer statistically significant.

The effects are even larger for the political risk from co-lenders in a lead arranger's syndicate. In Column 3, we find that without controlling for persistent differences between arrangers, the coefficient estimate on *zPRisk_Network* is 19.966 (*t-value* = 4.07), which suggests that a one standard deviation increase in the political risk of the arranger's syndicate loan network is associated with about a 20 basis-point increase in the total cost of borrowing. The effect size is attenuated when we focus on changes in the lead arranger's network risk over time; the estimated coefficient is similar in magnitude to the direct effects from *zPRisk_Borrower* and *zPRisk_Arranger*.

These findings suggest the presence of network connections through which political risk can propagate and create sector-wide effects (e.g., Acemoglu 2012).[35] More specifically, an increase in the political risk of an arranger's loan portfolio is associated with higher loan pricing for new borrowers. Similarly, if one co-lender in an arranger's preferred network comes under close regulatory or political scrutiny, the arranger seems to pass the associated risk to borrowers.

8. Market for credit default swaps

We examine the link between political risk and credit insurance premiums to corroborate our borrower-level analysis from Section 2 of the main text. If political risk affects credit market outcomes, we should observe that higher exposure to political risk is associated with higher CDS spreads (another proxy for default risk). In addition to CDS spreads, we have data on the recovery rate (representing the value of securities emerging from default), which allows us to estimate the loss from default. We use monthly five-year CDS spreads from the Markit database to re-estimate Equation (1).

We present the results in Table OS8. We find that firm-level political risk is positively associated with credit default swap spreads. A one standard deviation change in overall firm-level political risk increases the five-year spread by 5.98 basis points (*t-value* = 1.91). Interestingly, the response to within-firm variation in political risk (Column 2) has a similar order of magnitude with a coefficient estimate of 6.24 (*t-value* = 2.16), which is statistically significant at the five percent level.

[35] HHLT explain how firm-level political risk can have macroeconomic consequences through network effects. In particular, they highlight the effect of supply relations on total factor productivity as a potential mechanism; our results open the possibility of another channel that operates through credit markets.

Turning to the recovery rate, we find a negative effect of firm-level political risk, consistent with the idea that higher exposure to political risk increases the loss-given default. The estimate does not meaningfully change with the inclusion of firm fixed effects, though its precision increases.

9. Political risk in the choice between bank loans and bonds

One of the fundamental questions in credit markets research is why some firms borrow mainly from banks while others rely much more on public bondholders. We show in Table 2 and Table OS1 that political risk is priced across private and public debt markets. However, this begs the question of whether firms facing high political risk choose the type of debt that minimizes their cost of financing. Therefore, we examine whether firms' political risk affects borrowers' public or private debt choices. This question contributes to our broader argument that political risk affects credit markets.

Because private lenders can collect information about a firm's political exposure, they can extract information rents; thus, public debt might be preferable for minimizing the costs of political risk. We investigate this in Table OS9, which excludes sample firms issuing both bonds and loans in a given year (who did not have to choose between the two markets) and firms issuing no debt in a given year (following Bharath *et al.* (2019) and Hasan *et al.* (2014)). The dependent variable is an indicator variable equal to one if a firm issued a loan and zero if it issued a bond in a given year. In addition to our variable of interest $zPRisk_{it}$, we include our standard vector of control variables. We find a negative association between a firm's political risk and the choice to borrow from a bank. The coefficient estimate equals -0.016 (significant at the five percent level), which suggests that a one standard deviation increase in political risk decreases the probability of bank borrowing (in favor of issuing bonds) by 1.6 percent in a given year.

10. *Further test of the parallel trends assumption*

Table OS10 provides a formal test of the assumption that there is no meaningful difference in loan spread between the treatment and the control group before treatment by estimating regressions of the loan spread onto Treated × Year in the period before redistricting was finalized in 2011. We find insignificant coefficient estimates on the interaction term in both Columns 1 and 2 (for the total cost of borrowing and the all-in-drawn spread, respectively), which suggests no violation of the parallel trends assumption.

11. *Revisiting the redistricting experiment*

Another complementary strategy to address the endogeneity of lenders' *PRisk* is to return to our redistricting shock as a source of plausibly exogenous variation in lenders' political risk. To induce such variation, we use the portfolio-level changes in political risk stemming from the redistricted borrowers who experienced a change in political representative. Specifically, for each lender, we use treated borrowers (as defined in Table 4) in the portfolio and then construct a variable ("Exposed") that measures a given lender's exposure to treated borrowers using the average "Treated" multiplied by the proportion of loans raised by treated borrowers. We use this lender-level variation to test the effect of lenders' political risk on interest rates charged to non-redistricted borrowers.[36] When a part of a lender's portfolio experiences an increase in political risk, this increases the lender's exposure to political uncertainty. Consequently, lenders are expected to pass on some of this uncertainty to non-redistricted borrowers.[37]

We test this prediction using a modified version of Equation (3), in which we replace "Treated" with "Exposed". Exposed is the average of the variable "Treated" (as defined in Table 4) calculaeted across firms in a lender's porfolio and multiplied by the proportion of loans raised by the treated borrowers.[38] Intuitively, "Exposed" measures the degree to which a lender's political risk is affected by the redistricting shock depending on the loans they had issued to "Treated" borrowers. We present this analysis in Table OS11. As before, we use the total costs of debt and all-in-drawn to proxy for the cost of debt. Our estimates are economically similar for both proxies, though, and we find a meaningful effect of the heightened political risk post redistricting of having exposed loan portfolios. Their order of magnitude ranges between 23 and 37 basis points.

12. *The effect of outside options – borrower-level PRisk*

We extend our analysis of the influence of political risk on loan pricing described in Section 4 by considering the role of bank dependency on the link between the borrower's own political risk and the cost of borrowing. We ask how a borrower's dependence on their lenders affects the extent to which lenders can negotiate more favorable interest rates in response to the borrower's political risk. By comparing the coefficients of borrower's political risk across different levels of bank dependence, we can assess more closely when and how political risk is priced.

This analysis is shown in Table OS12. We estimate a regression analogous to that in Panel B of Table 5, but replace *zPRisk_Arranger* with *zPRisk_Borrower*, and evaluate the estimated coefficient on the interaction of this variable with the various proxies for bank-dependence (*Bank_dependent*). All specifications find a significant and positive coefficient on this interaction, while the main effect of

---

[36] Recall that *Treated* is based on our definition of treated firms in equation 3, firms that are represented by a *new* House representative *and* that move to a higher or lower *PRisk* quartile whereas the control group is a combination of (1) redistricted firms with a new representative but that remain in the same quartile of firm-level political risk, and (2) firms that are redistricted with no change in representative.

[37] One might wonder why lenders have to charge higher loan prices for borrowers who are not affected by a change in political risk (PRisk) due to redisticting, when they can simply adjust the interest rates for those who are. This would imply that PRisk is not transmitted to other borrowers. However, this is not always the case. Sometimes, lenders cannot change the interest rates for borrowers who face PRisk, for example, when they have already signed a contract with them. In such situations, some of the PRisk may spill over to other borrowers. Moreover, lenders may also face PRisk directly from political events, regardless of whether their borrowers are affected or not.

[38] Since the average loan matures in about 4-5 years, we restrict the sample to 2009-2013 to ensure that loans to treated borrowers continue to affect lenders post-redistricting.

zPRisk_Borrower is insignificant in all but one case. Together, these estimates imply that borrowers with higher political risk incur higher interest costwhen their lenders have greater bargaining power.

Table OS1: Political risk in public debt markets

Panel A: Main results

VARIABLES	(1) *Bid-Ask Spread*	(2) *Bid-Ask Spread*	(3) *Bond Yield*	(4) *Bond Yield*	(5) *Liquidity*	(6) *Liquidity*
zPRisk	1.157***	0.549*	7.400***	2.727*	-0.002**	-0.001**
	(3.04)	(1.98)	(3.32)	(1.75)	(-2.14)	(-2.38)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	122,315	122,260	115,392	115,327	150,599	150,565
R-squared	0.194	0.288	0.345	0.517	0.292	0.478
Quarter ×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes

This table presents the effects of firm-level political risk on bid-ask spread, bond yield, and a volume-based measure of liquidity. The dependent variable in Columns 1 and 2 is the quarterly, median, trade-weighted bid-ask spread (*Bid-ask spread*). The dependent variable in Columns 3 and 4 is the difference between the quarterly median yield-to-maturity and the yield of a treasury bill with matched maturity (*Bond yield*). The dependent variable in Columns 5 and 6 is the log of the total traded dollar volume divided by total par volume (*Liquidity*). The main independent variable is the standardized firm-level political risk (*PRisk*) defined in HHLT. *PRisk* is measured in the firm-quarter before the bond trading date; the prefix '*z*' indicates that the measure is standardized. *Controls* indicates the inclusion of control variables described on page 12 of the paper. For each bond feature, we estimate a specification with industry-quarter fixed effects and a specification with issuer fixed effects. Data are at the issuer level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the district level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS2: Political risk and debt financing

VARIABLES	(1) *Net Long-Term Debt Issuance*	(2) *Net Long-Term Debt Issuance*
zPRisk	-0.001**	-0.001
	(-2.13)	(-0.85)
Controls	Yes	Yes
Observations	34,143	33,573
R-squared	0.117	0.298
Year × Industry FE	Yes	Yes
Firm FE	No	Yes

This table reports the effects of political risk on debt financing. The dependent variable in Columns 1 and 2 is *Net long-term debt issuance*, which is a percentage of total assets that is computed by subtracting long-term debt reductions from long-term debt issuances and then dividing by lagged total assets. The main independent variable is the standardized firm-level political risk defined in HHLT. *PRisk* is the average firm-level political risk over the four quarters before debt issuance; the prefix '*z*' indicates that the measure is standardized. *Controls* indicates the inclusion of control variables described on page 12 of the paper. For each dependent variable, we estimate a specification with industry-year fixed effects and a specification with industry-year and firm fixed effects. All variables are defined in Appendix Table A1. Robust t-statistics, clustered at the firm level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS3: Exposure to aggregate political risk

Panel A: Firm-level exposure to aggregate political risk

VARIABLES	(1) *Total Cost*	(2) *All-in-Drawn*	(3) *Bond Yield*
zPRisk	8.336***	6.786***	7.378***
	(3.41)	(3.52)	(3.31)
zEPUbeta$_i$	1.219	0.387	3.261
	(0.94)	(0.49)	(0.45)
Controls	Yes	Yes	Yes
Observations	8,526	11,022	115,392
R-squared	0.350	0.411	0.346
Industry × Time FE	Yes	Yes	Yes
Firm FE	No	No	No

Panel B: Time-varying exposure to aggregate political risk

VARIABLES	(1) *Total Cost*	(2) *Total Cost*	(3) *All-in-Drawn*	(4) *All-in-Drawn*	(5) *Bond Yield*	(6) *Bond Yield*
zPRisk	8.895***	4.843**	7.284***	4.070**	7.409***	2.948*
	(3.58)	(1.98)	(3.69)	(1.99)	(3.12)	(1.73)
zEPUbeta2yr$_{it}$	-10.079	4.691	-1.620***	-3.391***	-11.103*	-5.046
	(-1.27)	(1.49)	(-3.52)	(-4.99)	(-1.89)	(-1.49)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,176	7,584	10,571	9,928	98,830	98,762
R-squared	0.352	0.641	0.412	0.647	0.359	0.531
Industry × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes

This table replicates results from Tables 2 and 3 of the main text after controlling for exposure to aggregate political risk. *EPUbeta$_i$* is obtained from a regression of a firm's daily stock returns on BBD's EPU Index, and *EPUbeta2yr$_{it}$* is obtained by running the same regressions using observations from the two years prior to *t* that are based on rolling estimation windows; the prefix '*z*' indicates that the measure is standardized. *Controls* indicates the inclusion of control variables described on page 12 of the paper. Data are at the borrower level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the borrower level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS4: Persistent political risk and credit markets

VARIABLES	(1) *Total Cost*	(2) *All-in-Drawn spread*	(3) *Bond Yield*
zPRisk5Y	10.131***	9.179***	9.892**
	(2.99)	(3.45)	(2.47)
Controls	Yes	Yes	Yes
Observations	6,447	8,561	98,439
R-squared	0.323	0.399	0.339
Industry × Time FE	Yes	Yes	Yes

This table reports the effect of persistent firm-level political risk on debt market outcomes. The dependent variable in Column 1 is the total cost of borrowing (*Total cost*). The dependent variable in Column 2 is the all-in spread drawn (*All-in-drawn spread*). The dependent variable in Column 3 is bond yield *(Bond yield*), measured as the difference between the quarterly median yield-to-maturity and the yield of a treasury bill with matched maturity. The main independent variable is the standardized firm-level political risk (*PRisk*) defined in HHLT. *PRisk* is measured as the average firm-level political risk over the five years preceding loan origination or trading date; the prefix '*z*' indicates that the measure is standardized. *Controls* indicates the inclusion of control variables described on page 12 of the paper. Data are at the borrower level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the borrower level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS5: Borrower's active political risk management

VARIABLES	(1) *Total Cost*	(2) *All-in-Drawn Spread*	(3) *Total Cost*	(4) *All-in-Drawn Spread*	(5) *Total Cost*	(6) *All-in-Drawn Spread*
zPRisk	6.615	8.627**	8.033***	4.028	9.234***	5.206**
	(1.59)	(2.52)	(2.67)	(1.29)	(3.31)	(1.98)
lnLobby	1.080	-0.043				
	(1.23)	(-0.06)				
zPRisk × lnLobby	-0.705*	-0.758**				
	(-1.66)	(-2.16)				
lnDonation			0.328	1.177	-0.022	1.539*
			(0.29)	(1.31)	(-0.02)	(1.65)
zPRisk × lnDonation			-1.164***	-0.525	-1.724***	-0.943**
			(-2.87)	(-1.39)	(-3.85)	(-2.33)
Partisan					-13.041	-4.204
					(-0.36)	(-0.12)
zPRisk × Partisan					-53.290	-65.489**
					(-1.57)	(-2.39)
lnDonation x Partisan					2.125	-0.113
					(0.54)	(-0.03)
zPRisk × lnDonation × Partisan					5.747*	6.637**
					(1.82)	(2.56)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,301	4,324	3,143	3,937	3,143	3,937
R-squared	0.664	0.670	0.667	0.677	0.668	0.678
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the effect of a borrower's lobbying and PAC donations on the relationship between political risk and loan pricing. The dependent variable in Columns 1, 3 and 5 is the total cost of borrowing (*Total Cost*). The dependent variable in Columns 2, 4 and 6 is the all-in-drawn spread (*All-in-drawn*). The main independent variable is the standardized firm-level political risk (*PRisk*) defined in HHLT. *PRisk* is measured as the average firm-level political risk over the four quarters preceding loan origination. The prefix '*z*' indicates that the measure is standardized. *lnLobby* is the log of one plus the average lobby expenses over the past four quarters. *lnDonations* is the log of one plus the sum of average contributions to federal election candidates over the past four quarters. *Partisan* is a dummy variable that takes one for firms that, in a given year, are in the top quartile of the distribution of the absolute difference between donations to Republican and Democratic political campaigns, zero otherwise. We estimate the relationship with a specification that has industry-year fixed effects and a specification with industry-year and firm fixed effects. *Controls* indicates the inclusion of control variables described on page 12 of the paper. Data are at the borrower level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the borrower level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS6: The effect of political risk on credit supply and deposit growth

VARIABLES	(1) *Loan Growth*	(2) *Loan Growth*	(3) *Deposit Growth*	(4) *Deposit Growth*
zPRisk	-0.002***	-0.002***	-0.002**	-0.003**
	(-2.79)	(-3.41)	(-2.09)	(-2.42)
zPSentiment	0.003***	0.003***	0.004***	0.003**
	(3.12)	(2.72)	(2.81)	(2.51)
zNPRisk	0.001	0.001	0.002	0.002
	(0.97)	(1.58)	(1.65)	(1.62)
lnASSETS	-0.001	0.015***	-0.000	0.011
	(-1.26)	(2.64)	(-0.05)	(1.52)
ROA	0.006**	0.005**	0.000	0.001
	(2.56)	(2.24)	(0.07)	(0.33)
ΔROA	-0.278	-0.229	-0.337***	-0.317**
	(-1.37)	(-1.31)	(-2.66)	(-2.43)
Loss	-0.018***	-0.014***	-0.020***	-0.014**
	(-3.88)	(-3.10)	(-3.09)	(-2.06)
Tier1 Capital Ratio	-0.001***	-0.003***	-0.000	-0.001
	(-3.52)	(-5.05)	(-0.19)	(-1.27)
Asset Risk	-0.033***	-0.037	-0.014	-0.037
	(-2.71)	(-1.57)	(-0.86)	(-1.22)
Observations	4,479	4,469	4,479	4,469
R-squared	0.109	0.229	0.035	0.138
Quarter FE	Yes	Yes	Yes	Yes
Bank FE	No	Yes	No	Yes

This table reports the effect of banks' political risk on loan and deposit growth. In Columns 1 and 2, the dependent variable is quarterly loan growth (*Loan growth*), defined as $\Delta Total\ loans_q$ divided by $Total\ loans_{q-1}$. In Columns 3 and 4, the dependent variable is quarterly deposit growth (*Deposit growth*), defined as $\Delta Deposits_q$ divided by $Deposits_{q-1}$. The main independent variable is the standardized political risk (*PRisk*) defined in HHLT. *PRisk* is measured at the level of the bank-holding company and is lagged by one quarter. The prefix '*z*' indicates that the measure is standardized. We estimate the relationship with quarter and with bank fixed effects. Data are at the bank holding company level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the bank level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS7: Channels of political risk and the cost of borrowing

VARIABLES	(1) *Total Cost*	(2) *Total Cost*	(3) *Total Cost*	(4) *Total Cost*
zPRisk_Portfolio	10.018**	-1.159		
	(2.14)	(-0.40)		
zPRisk_Network			19.966***	3.385*
			(4.07)	(1.94)
zPRisk_Arranger	11.281***	2.940**	12.445***	3.650***
	(3.15)	(2.03)	(4.01)	(3.13)
zPRisk_Borrower	4.099***	3.909***	4.759***	4.657***
	(2.75)	(2.70)	(3.25)	(3.25)
Controls	Yes	Yes	Yes	Yes
Observations	15,957	15,951	15,993	15,983
R-squared	0.400	0.443	0.402	0.442
Industry × Year FE	Yes	Yes	Yes	Yes
Bank FE	No	Yes	No	Yes

This table reports the channels for the relationship between the lender's political risk and loan pricing. The dependent variable is the total cost of borrowing (*Total cost*). In Columns 1 and 2, the main independent variable is the standardized political risk from the arranger's portfolio of borrowers (which includes all borrowers with outstanding loans from the current arranger). Once the portfolio of borrowers is identified, we use the four-quarter-average *PRisk* of each borrower (measured before the current loan date) and the count of loans for each borrower over the past three years to compute the weighted average portfolio risk (*PRisk_Portfolio*). In Columns 3 and 4, the main independent variable is the standardized political risk from the lead arranger's network (which is comprised of all co-lenders with whom the lead arranger has co-syndicated loans for the three years starting during the quarter before the current loan date). Once the network is identified, we use the four-quarter-average *PRisk* for each co-lender (measured before the current loan date) and the count of joint-loans with each co-lender to compute the weighted average network risk (*PRisk_Network*). The prefix '*z*' indicates that the measure is standardized. *Controls* indicates the inclusion of control variables described on page 12 of the paper. For each channel, we estimate a specification with industry-year fixed effects and a specification with industry-year and bank fixed effects. Data are at the arranger level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the arranger level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS8: Political risk in CDS markets

VARIABLES	(1) *CDS Spread*	(2) *CDS Spread*	(3) *Recovery Rate*	(4) *Recovery Rate*
zPRisk	5.983*	6.239**	-0.063*	-0.064***
	(1.91)	(2.16)	(-1.96)	(-3.07)
Controls	Yes	Yes	Yes	Yes
Observations	50,140	50,133	50,050	50,043
R-squared	0.339	0.506	0.219	0.598
Industry ×Year FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes

This table reports the effect of firm-level political risk on CDS markets. The dependent variable in Columns 1 and 2 is the amount a protection buyer has to pay a protection seller (*CDS Spread*). The dependent variable in Columns 3 and 4 is an estimate of the percentage of par value that bondholders receive in the case of a credit event (*Recovery rate*). The main independent variable is the standardized firm-level political risk (*PRisk*) defined in HHLT. *PRisk* is measured in the firm-quarter preceding the CDS spread date; the prefix '*z*' indicates that the measure is standardized. Each political risk quarter is merged with the subsequent three-month CDS spread. *Controls* indicates the inclusion of control variables described on page 12 of the paper. For each dependent variable, we estimate a specification with industry-year fixed effects and a specification with firm fixed effects. Data are at the firm level, and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the firm level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS9: The effect of political risk on the choice between bank loans and bonds

VARIABLES	(1) *Loan_Issue*	(2) *Loan_Issue*
zPRisk	-0.016**	-0.098**
	(-2.23)	(-2.35)
Controls	Yes	Yes
Observations	5,640	5,027
R-squared/ Pseudo R2	0.262	0.161
Industry × Year FE	Yes	Yes

This table reports the effect of firm-level political risk on the choice between bonds and bank loans using linear probability (Column 1) and using a logit model (Column 2). The dependent variable in Columns 1 and 2 (*Loan_issue*) is an indicator variable equal to one if a firm issued a loan in a given year, and zero for public bonds. The main independent variable is the standardized firm-level political risk (*PRisk*) defined in HHLT. *PRisk* is measured as the average firm-level political risk over the four quarters preceding debt choice; the prefix '*z*' indicates that the measure is standardized. *Controls* indicates the inclusion of control variables described on page 12 of the paper. Data are at the firm level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the firm level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS10: The time-trend in loan spread before treatment

VARIABLES	(1) *Total Cost*	(2) *All-in-Drawn Spread*
Treated × *Year*	-12.006	-1.919
	(-1.16)	(-0.18)
Observations	615	792
R-squared	0.778	0.731
Time FE	Yes	Yes
Firm FE	Yes	Yes

This table tests for before-treatment (i.e., before 2011) differences in loan-spread time trends for the treatment and the control groups. The dependent variable in Column 1 is the total cost of borrowing (*Total cost*). The dependent variable in Column 2 is the all-in spread drawn (*All-in-drawn spread*). *Treated* is a categorical variable that takes the value of one if redistricting increased political risk, -1 if redistricting decreased political risk, and zero if the level of political risk remained the same. *Year* is a time-trend variable before 2011. Data are at the borrower level. Robust t-statistics, clustered at the borrower level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS11: Portfolio redistricting and loan pricing

VARIABLES	(1) *Total Cost*	(2) *All-in-Drawn*
Exposed	-23.496**	-12.117**
	(-2.24)	(-2.06)
Exposed × *After*	37.687**	23.473**
	(2.39)	(2.61)
Controls	Yes	Yes
Observations	2,798	3,386
R-squared	0.814	0.782
Industry × Time FE	Yes	Yes
Lender FE	Yes	Yes
Borrower FE	Yes	Yes

This table reports the effect of lenders' exposure to redistricted borrowers on loan pricing. The dependent variable in Columns 1 and 2 is the total cost of borrowing (*Total cost)* and all-in-drawn spread (*All-in-drawn*) respectively. *Exposed* is the average of the variable *Treated* (see Table 4) for all borrowers in lender's portfolio over the past year multiplied by the proportion of loans issued by redistricted borrowers, where *Treated* is based on Panel D in Table 4. *After* is an indicator variable that takes the value of one if the year is 2011 or greater, and zero otherwise. Since the average loan matures in about 4-5 years, we restrict the sample to 2009-2013. Data are at the arranger level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the district level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table OS12: The effect of outside options – borrower-level *PRisk*

VARIABLES	(1) *Total Cost*	(2) *All-in-Drawn*	(3) *Total Cost*	(4) *All-in-Drawn*	(5) *Total Cost*	(6) *All-in-Drawn*
zPRisk_borrower	1.151	1.688*	0.406	-0.403	1.258	1.672
	(0.74)	(1.94)	(0.31)	(-0.38)	(0.94)	(1.61)
Bank_dependent1	11.955***	11.624***				
	(3.11)	(4.40)				
zPRisk_borrower × *Bank_dependent1*	6.931*	4.634**				
	(1.89)	(2.01)				
Bank_dependent2			5.223*	0.412		
			(1.94)	(0.18)		
zPRisk_borrower × *Bank_dependent2*			5.539	9.586***		
			(1.21)	(3.28)		
Bank_dependent3					41.436***	23.265***
					(5.59)	(4.31)
zPRisk_borrower × *Bank_dependent3*					20.550***	10.184**
					(4.49)	(2.24)
zPRisk_arranger	2.962**	3.026***	1.592	1.720*	3.135**	2.819***
	(2.56)	(3.71)	(1.07)	(1.68)	(2.60)	(3.24)
Observations	15,974	20,129	11,152	13,553	14,622	18,268
R-squared	0.443	0.502	0.491	0.549	0.467	0.518
Industry x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports the effect of relationship-based lending on the relation between a borrower's political risk and loan pricing. The dependent variable in Column 1 is the total cost of borrowing (*Total cost*). The dependent variable in Column 2 is the all-in-drawn spread (*All-in-drawn*). The main independent variable is the standardized borrower-level political risk as defined in HHLT. *PRisk_borrower* is measured as the average arranger-level political risk over the four quarters preceding loan origination. The prefix '*z*' indicates that the measure is standardized. *Bank dependent1* is an indicator equal to one if the percentage borrowed from the current lead arranger(s) over the previous 3 years is at least 50% of the firm's total loan amount over the past 3 years, zero otherwise. *Bank dependent2* is an indicator equal to one if the borrower did not access the bond market in past 3 years, zero otherwise. *Bank dependent3* is an indicator variable equal to one when the total number of a borrower's lenders over the past four transactions is below the median, zero otherwise. *Controls* indicates the inclusion of control variables described on page 12 of the paper. All results are estimated with industry-year and bank fixed effects. Data are at the arranger level and variables are defined in Appendix Table A1. Robust t-statistics, clustered at the arranger level, are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.