The Spillover of Corporate ES on Bank Loan Cost

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Abstract

We examine how corporate environmental and social (ES) risks influence the bank loan costs of peer firms. Utilizing a regression discontinuity approach based on shareholder votes on ES-related proposals in U.S. public companies from 2005 to 2021, we find that the approval of these proposals leads to an average 38 basis-point increase in peer firms' loan costs over the following year. This effect is more pronounced when the proposal is more salient for banks, and when peers have higher ex-ante ES risk and weaker bargaining power. Surprisingly, we find that the spillover is primarily driven by banks with less expertise or weaker ex-ante incentives to price ES risks. These findings suggest that corporate ES risks extend beyond individual firms and influence the loan costs of broader peer borrowers by shaping banks' loan pricing practices.

Keywords: Corporate ES risks; bank loan cost; spillover; regression discontinuity

JEL Classification: M14; G14; G21; G30

1. Introduction

In recent years, firms' environmental and social ("ES") profiles have gained increased importance, particularly in relation to their cost of capital. Existing research suggests that financial institutions, such as banks, are more likely to consider their borrowers' climate and social risks when making lending decisions (Chava, 2014; Houston and Shan, 2022; Huang et al., 2022; Edwards et al., 2024). Besides the direct impact of a firm's ES profile on its credit risk, banks may also be concerned about borrowers' environmental and social risks due to external pressures imposed by diverse stakeholders, such as regulators (Bellon, 2021; Wang, 2023) and depositors (Chen et al., 2023). As industries increasingly shift towards more ES-intensive practices and debates around greenwashing intensify, understanding how corporate ES risks affect peer firms becomes crucial, offering fresh insights into the banking sector's influence on corporate ES decisions. In this paper, we aim to address the following questions: Do corporate ES risks influence peer firms' borrowing costs, and if so, what is the economic mechanism driving this spillover?

Theoretically, it is unclear whether and how changes in a corporation's ES risk influence the bank loan costs of its peers. On the one hand, changes in a firm's risk profile could provide valuable information to its lending banks, prompting them to incorporate the information in pricing loans of other portfolio firms. If a firm's ES profile is part of its overall risk profile, this could lead to a spillover effect, where banks adjust loan rates for peer firms based on changes in a firm's ES-related risks. For example, Correa et al. (2023) document that banks impose higher loan spreads on borrowers exposed to physical climate risks, even when those borrowers are not directly affected. This suggests that lenders revise their expectations about the future impact of climate-related events on firms in their portfolios, whether or not those firms are directly or indirectly exposed. On the other hand, if banks typically perform comprehensive screening and monitoring (Allen, 1990; Diamond, 1984), then a change in one firm's risk profile might not provide banks with new information about the risk profiles of other portfolio firms, leading banks to maintain their existing assessments of ES risks for peer firms. In this case, no spillover effect would be expected.

Identifying potential spillover effects of a focal firm's ES policies on its peers' borrowing costs presents significant challenges, such as reflection, correlated unobservables, and endogenous peer group formation (Manski, 1993). To address these challenges, we employ a regression discontinuity (*RD*) design based on the voting outcomes for ES-related proposals in firms' shareholder meetings, focusing on the intention-to-treat effect triggered by the passage of these proposals. As Cuñat et al. (2012) argues, the passage of proposals around the 50% voting threshold can be treated as random, and a small difference in votes results in a discrete change in the likelihood of policy implementation. This creates exogenous variation in focal firms' ES commitments (Cao et al., 2019), which we leverage to analyze whether the marginal passage of ES proposals leads banks to adjust the loan costs of peer firms, as measured by the all-in-drawn spread (*aisd*) on corporate loans. Peer firms are defined as those sharing common lead lenders with a focal firm but not experiencing relevant voting events themselves, allowing us to explore the spillover effect through bank lending relationships.

We focus on U.S. public firms with ES shareholder proposals voted on during their annual meetings (*focal firms*) from 2005 to 2021, along with their peers. Our loan-level data assesses the similarity between focal firms and non-voting peers based on bank-lending relationships, classifying peers as close or distant. To validate the RD design, a McCrary test (McCrary, 2008) confirms no manipulation of voting shares around the 50% cutoff, ensuring random assignment of proposal passage. We also check the balance of pre-determined covariates between peers of firms that passed (*passing peers*) and vetoed (*vetoing peers*) ES proposals, finding no significant differences (Table 3), further supporting the validity of our RD design and reducing selection bias concerns.

Our key findings are as follows. First, we find that the passage of an ES proposal—likely reducing the focal firm's ES risks—lowers the bank loan costs for the focal firm (Table 4). However, this generates a negative spillover effect for their peers, leading to higher loan costs for these firms. Specifically, our baseline results show that passing peers experience a 38 basis point (or 0.33 standard deviation) increase in loan costs compared to vetoing peers (Table 5), with the average loan spread being 192 basis points. This spillover effect remains robust across various RD bandwidth and polynomial specifications, and is more pronounced for peers with stronger banking relationships with the focal firm (*closer peers*).

This spillover effect suggests that banks respond more strongly to the passage of ES proposals than to their vetoes. But why does the approval of an ES proposal convey information that a vetoed proposal does not? As Flammer (2015) highlights, ES proposals typically receive limited support at shareholder meetings, suggesting that shareholders often view them unfavorably. In our sample, the approval rate is about five percent, indicating that the passage of such a proposal is likely unexpected by lending banks. While the failure of an ES proposal is generally anticipated, its approval signals a heightened level of shareholder concern regarding ES issues at the focal firm. This unexpected shift likely prompts banks to reassess the ES risks across other firms in their portfolios. Consequently, banks adjust loan pricing for peer firms based on their perceived ES risks, leading to the observed spillover effect.

We further investigate the heterogeneity of the spillover effects, focusing on the information sources that likely shape how banks update their views on the relevance of ES risks (Table 6). Our findings indicate that the spillover effect is stronger when the focal firm and its passing peers are located in the same state, suggesting that geographic proximity influences how banks respond to ES proposal passage. Additionally, the effect is more pronounced in the earlier period (2005–2013), when approval rates for ES proposals were lower and these events provided more novel information. During this time, industries were less attuned to ES issues, and concerns about greenwashing were minimal, making the passage of ES proposals more impactful. Furthermore, a textual analysis reveals that the spillover effect intensifies when there is greater divergence in tone between the proponent's statement (by shareholders) and the opponent's statement (by management). This suggests that banks are more likely to revise their assessments of a firm's ES profile when there is significant disagreement between shareholders and management. This finding aligns with the notion that sophisticated investors are more likely to adjust their beliefs in response to market disagreement (Carlin et al., 2014), further highlighting how conflicting views within the firm can influence external parties' evaluations of ES-related risks.

In the next set of heterogeneity analyses, we observe that banks reprice loans differently across peer firms and loan types, consistent with their repricing incentives (Tables 7 to 9). The spillover effect on peers' bank loan costs is concentrated among firms with tighter credit or financial constraints and lower bargaining power in their banking relationships, such as smaller or younger firms, those with high cash flow volatility, or firms with a high frequency of constraining words in their 10-K filings. This suggests that banks view these firms as more vulnerable to ES risks and thus adjust loan pricing more aggressively. Notably, the spillover is more pronounced for loans issued to peers with low ex-ante ES scores or firms operating in polluting industries, providing clear evidence that banks adjust loan pricing based on observable ES performance, with more aggressive repricing for firms carrying higher ES risks. Furthermore, the spillover effect is stronger for credit lines compared to term loans, which aligns with banks' heightened monitoring incentives for credit lines, as these are typically held for longer durations. This differentiation in loan types underscores how banks' incentives and risk assessments vary across different lending structures.

The heterogeneity analyses indicate that banks revise their perceptions of ES-related risks for other firms in their portfolios and adjust loan pricing following the passage of ES proposals by their borrowers. To investigate the mechanism, we divide our loan-level sample into two groups: loans issued by *ES banks*–those with positive environmental and social scores–and loans issued by *non-ES banks*, which have zero or missing environmental and social scores. If the spillover effect is driven by banks updating their views on ES risks, we would expect the effect to be more pronounced among non-ES banks, which have less prior experience and motivation in pricing these risks. Our results (Table 10) confirm this: the spillover is significant for loans issued by *non-ES banks*, but absent for those from *ES banks*. This supports the idea that ES banks already factor in peers' ES risks when making lending decisions, eliminating the need to adjust assessments based on the passage of focal firm's ES proposals. In contrast, non-ES banks revise their beliefs or awareness of ES risks following the focal proposal passage and reprice loans accordingly. Further evidence (Table 11) shows that non-ES banks specifically reprice peer loans for peers with low ex-ante ES scores, rather than applying uniform adjustments across all portfolio loans. This targeted repricing helps rule out the alternative explanation that the spillover is driven by banks' capital constraints, reinforcing the view that non-ES banks are updating their assessments of ES risks in response to new information from the focal firm's proposal passage.

Overall, our findings underscore the role that banks play as financial intermediaries in pricing ES risks and shaping firms' cost of capital. A firm's ES profile or ES-related decisions affect not only its own borrowing conditions but also those of its peers. Furthermore, the extent of banks' responses to ES risks varies significantly based on their expertise and motivation in assessing these risks. Banks with greater experience in managing ES risks proactively incorporate these factors into their lending decisions from the outset, while banks with less experience reactively adjust their loan pricing following key ES-related events. This differentiation highlights the crucial function of banks in disseminating information about ES risks across their portfolios, ultimately influencing capital costs across firms.

We conduct a series of robustness tests and additional analyses to validate our findings. These include placebo tests with several artificial cutoffs for voting outcomes, none of which show evidence of spillover effects, reinforcing the validity of our regression discontinuity design. Furthermore, we examine other loan characteristics, such as changes in loan amount, maturity, and covenants, and find no indication that the spillover effect extends to these dimensions. Our analysis also shows no persistent or reversed impact on loan rates in the years following the voting events, suggesting that the spillover effect is limited to the immediate aftermath of the proposal's passage. Additionally, we find no significant changes in the ES scores of peer firms in the years following the event, indicating that banks' repricing decisions are not driven by immediate shifts in peer firms' ES performance. A difference-in-differences analysis confirms that the spillover effect is specifically driven by the passage of ES proposals, rather than by their veto. This further supports the idea that the spillover arises from banks updating their beliefs about ES risks when ES proposals pass, rather than from generalized adjustments in response to voting outcomes.

Our paper closely relates to the literature on the relationship between ES(G) or climate risks and the cost of capital (Krueger et al., 2020; Pástor et al., 2021; Bolton and Kacperczyk, 2021;

Pedersen et al., 2021; Bansal et al., 2022). While prior research has established a direct link between these risks and the cost of capital, particularly in the context of bank financing (Chava, 2014; Houston and Shan, 2022; Huang et al., 2022; Edwards et al., 2024), we advance this literature by documenting a novel spillover effect. Specifically, we show that a firm's ES risk reduction – triggered by the passage of ES proposals – leads to higher bank loan costs for its peers. This finding reveals a broader impact of ES risk management through the banking channel, illustrating how improvements in one firm's ES profile can raise the cost of capital for others, thereby deepening our understanding of the indirect effects of ES considerations on capital markets.

We also contribute to the literature on banks' loan pricing. Early studies suggest that private information produced in relationship banking create rents for banks, and thus affect loan pricing (Sharpe, 1990; Rajan, 1992) and that shareholder rights affect loan costs (Chava et al., 2009). Our work aligns with recent studies that suggest banks use information beyond individual firms when making lending decisions (Murfin, 2012; De Franco et al., 2021; Dell'Ariccia et al., 2021; Gao et al., 2022). The most relevant work is by Correa et al. (2023), who find that banks respond to a public signal of *physical climate risks* and adjust loan spread for firms that are potentially exposed but not directly affected. In our case, we find that such spillover also extend to the ES risk, which is more related to the *transition risk*,[1] and idiosyncratic events (focal firm's proposal passage) could also trigger banks' loan repricing. We advance this literature by documenting spillover effects in peer firms' loan costs, particularly among banks with less expertise or motivation to price ES risks. Our study also links to recent work showing that banks' ES(G) policies or regulations affect the borrowing terms for firms, including those not directly exposed to environmental risks, and thus promoting ES(G) improvements (Amiram et al., 2023; Wang, 2023).

Additionally, our study connects to the literature on peer effects or spillovers in corporate settings. Prior research has explored peer influences on financial decisions such as lending, financing,

[1] Physical and transition risks are two key categories of risk associated with climate change (Giglio et al., 2021). Physical risks refer to the direct impact on assets caused by disasters such as hurricanes or sea level rise, while transition risks arise from the potential shift towards a low-carbon economy. This transition may involve changes in climate regulation, market preferences, energy technologies, etc. Physical risk exposure is typically tied to geographic location, whereas transition risk exposure is influenced by a firm's environmental performance. Transition risk is gaining increasing relevance in the literature.

and cash holdings (Leary and Roberts, 2014; Kaustia and Rantala, 2015; Chen et al., 2019; Shakya, 2022; Shakya, 2023). While similar RD designs have been used to document peer effects on CSR policy adoption (Cao et al., 2019; Dai et al., 2021), our study differs by showing the spillover mechanism operates through non-ES banks updating their beliefs and repricing loans, rather than peer firms altering their ES policies.

The remainder of the paper is structured as follows: Section 2 discusses the institutional background of shareholder proposals and bank loans. Section 3 outlines our identification strategy and data. Section 4 presents the baseline findings and explores the economic mechanisms. Section 5 covers robustness tests and additional analyses, with concluding remarks in the final section.

2. Research background

Close-call shareholder proposals on ES issues Shareholders are increasingly involved in managerial oversight through private communication, proxy contests, and shareholder proposals. Among these, shareholder proposals are low-cost and allow shareholders to publicly express concerns about firm management. Under Rule 14a-8 of the Securities Exchange Act of 1934, shareholders can include proposals in a company's proxy statement for consideration at the Annual General Meeting (AGM), addressing issues like corporate governance, executive compensation, and environmental and social matters.

Since the rule's enactment, shareholder proposals have addressed timely concerns, from civil rights and equal opportunities in the 1960s and 1970s to anti-takeover activities in the 1980s and sustainability in the 1990s. Although many proposals fail to gain a majority, they increase pressure on management to act, particularly on environmental and social (ES) issues. Shareholder proposals offer a direct means for investors to push management to address specific concerns and raise awareness among other shareholders. Close-call proposals, those passing or failing by a narrow margin, are notable, as a proposal requires over 50% of votes to pass. While ES proposals historically received minimal support, the passing rate has increased since 2016. Our study aligns with corporate finance research using close-call shareholder proposals in an regression discontinuity (RD) design (Cuñat et al., 2012; Flammer, 2015; Flammer and Bansal, 2017; Chemmanur and Tian, 2018; Cao et al., 2019).

Corporate bank loans Bank loans are a key form of private corporate debt and the primary source of debt financing for most firms in our sample.[2] We focus on bank loans in our analysis because banks have strong incentives to screen and monitor their loan portfolios. They gather information on potential borrowers from various channels, as supported by existing research. While some studies have explored the link between responsible bank lending and borrowers' ESG behavior (e.g., Houston and Shan, 2022), none have examined the spillover effects of corporate ES issues on loan costs through bank lending.

Most corporate loans are syndicated for our sample of firms, meaning a consortium of banks or financial institutions provide funds to a single borrower under a common contract. The process is led by an arranger who assesses credit quality and negotiates terms before inviting others to participate. The average syndicate size is about four financial institutions (Bradley and Roberts, 2015).

Interest rates on corporate loans are typically floating, with a base rate (commonly LIBOR) plus a spread specified in the loan contract (Carey and Nini, 2007). The base rate is market-determined and periodically reset. To control for fluctuations in the base rate, our analysis focuses on the interest rate spread (the difference between the charged rate and LIBOR) and any associated fees.

3. Data and empirical strategy

3.1. Data

Proposal data We collect shareholder proposals related to companies' environmental or social issues and their voting outcomes during 2005–2021 from RiskMetrics and SharkRepellent

[2] Private corporate debt includes instruments like commercial paper and corporate bonds issued through private placements. Bradley and Roberts (2015) note that private debt issuance has been two to three times higher than public debt annually since 1994. Between 1993 and 2001, private debt issuance in the U.S. totaled \$2.855 trillion, compared to \$1.325 trillion in public debt. Houston and James (1996) estimate that only 17% of firms' debt is public, with most relying on private debt. Over 90% of loans in Dealscan are either 364-day facilities, revolving loans (allowing capital drawdowns), or term loans (providing a lump sum with scheduled repayment). A 364-day facility is a short-term revolving facility that can be extended or converted to a term loan.

datasets.[3] We use the former for all S&P 500 companies during 2006–2021 and the latter for firms in the Russel 4,000 Index from 2005 to 2021. We begin with 3,613 ES proposals, which are then matched to their corresponding Compustat identifiers, resulting in a total of 3,286 proposals. To address the issue that companies occasionally vote on several ES proposals in a single meeting, we apply two filters: when multiple proposals at the same meeting are all rejected, we select the proposal that is nearest to meeting the approval threshold. If there are both approved and rejected proposals, we retain only the approved one. With basic loan data and financial data being matched on the focal and at least one peer firm associated with a proposal, its gives us 1,875 proposals in total. We provide the summary statistics of the ES proposals and their voting outcomes in Table 1. Consistent with Flammer (2015), only a small fraction of proposals receive majority support: 63% of the proposals receive less than 30% of favorable votes in our sample. However, the number of close-call proposals is large enough to give statistical power for the RD design: 107 proposals received a vote share within the $\pm$ 5% interval around the majority threshold of 50%; and 247 within the $\pm$ 10% interval.

[Insert Table 1 here.]

Panel A of Table 1 shows the number of proposals experiences an increasing trend until 2017, and the proportion of favorable votes increases from around 20% in the first half of the sample (2005-2013) to about 30% in the second half (2014–2021). Panel B reports the breakdown of the proposals into nine types, with the classification obtained from SharkRepellent. Two types are environment-related (environmental issues and sustainability reports) while the rest are related to social issues (minorities/women on board, animal rights, health issues, human rights, labor issues, political issues, and other social issues).

Corporate Loan data Following Chava and Roberts (2008), we obtain the data on bank loans from the Loan Pricing Corporation's (LPC) Dealscan database. We focus on the dollar-denominated

[3] Table A.II provides four examples of close-call ES proposals. The proposals in Examples 1 and 2 are marginally vetoed, while Examples 3 and 4 are marginally approved.

private loans issued by banks and other financial institutions to U.S. public companies. Our basic unit of observation is a loan (also referred to as a facility or a tranche), although loans are often grouped into deals (or packages) when firms enter into multiple loans at the same time. We collect information on each loan including the borrower, the lender(s), loan type, deal purpose, loan amount, maturity and pricing, covenant restrictions, etc. When capturing bank-lending relations, we focus on lead lenders in corporate loan syndicates, most of which are banks. Our sample includes 1,011 lender-year observations, representing 166 unique lead lenders.[4] Loans are often amended after the initial issuance, otherwise, the loan terms and conditions remain the same before maturity. To examine the potential spillover, we focus our analysis on the newly issued and amended loans, and do not include ongoing loans in the calculation of loan cost.[5]

Bank loan peers Two firms are considered peers if they share a common lead bank in their borrowing relationships. Specifically, for a focal firm undergoing shareholder voting on ES proposals in year t, any other firm with corporate loans from the same lead bank is classified as a peer. Next, we calculate the bank loan similarity between a focal firm j and a non-voting firm i in year t as:

$$\text{bank loan similarity}_{i,j,t} = \frac{\upsilon_{i,t} \cdot \upsilon_{j,t}}{|\upsilon_{i,t}| \times |\upsilon_{j,t}|} \in [0,1]. \tag{1}$$

This measure captures the extent of overlap in the firms' lending relationships, where $\upsilon_{j,t}$ is a vector representing firm j's proportion of loans from a set pool of banks in year t. A positive bank loan similarity indicates that firm i is a peer of focal firm j. The dot product $\upsilon_{i,t} \cdot \upsilon_{j,t}$ reflects the overlap, and $|\upsilon_{j,t}|$, the length of the vector, is calculated as $\sqrt{\upsilon_{j,t} \cdot \upsilon_{j,t}}$.

For example, in Year t, Firm 1 borrows equally from Banks A and B (50% each), while Firm 2 borrows 40% from Bank A and 60% from Bank C. Their loan vectors are $\upsilon_{1,t} = [0.5, 0.5, 0]$ and

[4] A lead lender corresponds to 3 (at the median) focal firms in a year, and a focal firm corresponds to 4 (at the median) lead banks in the event year.

[5] The ES proposals are likely to contain important information. Passing an ES proposal may trigger banks to reprice their loan portfolio. The terms and conditions of the newly issued and amended loans reflect this adjustment.

$\upsilon_{2,t} = [0.4, 0, 0.6]$, respectively. The bank loan similarity between these firms is

$$0.2/(\sqrt{0.5} \cdot \sqrt{0.52}) = 0.392 > 0.$$

This makes Firm 2 is a *bank loan peer* of Firm 1. Among the bank loan peers in our sample, only two to five percent are also industry peers based on SIC (2-4 digits) or Hoberg-Phillips TNIC3 classifications. Peers with higher bank loan similarity values are considered *closer peers*. In our analysis, we focus on the closest 10, 20, and 50 peers, as they are more likely to capture potential spillover effects.

The sample and main variables Our sample consists of (peer-year, focal) observations, where each peer firm in a given year is linked to a focal firm with an ES proposal in the prior year. Using data on shareholder proposals related to environmental and social issues for U.S. public firms from 2005 to 2021, we identify peers based on the bank loan similarity measure in Equation (1). The dataset includes proposal characteristics and peer firms' financial information. The final sample contains 1,875 ES proposals and 58,250 (peer-year, focal) observations for the 50-closest peers.

The main dependent variable is the bank loan cost of a peer firm in the year following the focal firm's ES proposal. Loan cost is measured by the loan spread, defined as the basis points a borrower pays over LIBOR (or its equivalent) per dollar drawn, along with the annual facility fee (Chava, 2014). If a peer firm has multiple newly issued or amended loans in a year, we calculate its loan cost as the weighted average of the loan spreads, with loan amounts as weights. To address potential small sample biases in the RD regression, we include covariates such as firm size, market-to-book ratio, leverage, return on assets (ROA), and age (Cao et al., 2019), as described in Table A.I.

Summary statistics in Table 2 reveal that focal firms tend to be larger, more mature, more profitable, and have higher market-to-book ratios than their peers. As expected, focal firms also enjoy lower average loan rates compared to their peers.

[Insert Table 2 here.]

3.2. Empirical strategy

We adopt the regression discontinuity (RD) design to estimate the effect of the passage of focal firm's shareholder proposals on peers' bank loan costs. The RD framework that exploits the shareholders' votes on ES proposals around the majority voting threshold of 50% is valid if the proposal passages are random outcomes. In that case, these close-call ES proposals provide a source of random variation of a firm's commitment to ES policies and that can be used to estimate the causal effect of passing an ES proposal on peer firms' bank loan cost.

To obtain the RD estimators, we perform local-polynomial nonparametric regressions by following procedures in Calonico et al. (2017). We implement a data-driven bandwidth approach and use the corresponding small (optimal) bandwidths on the left- and right-hand sides of the voting threshold to estimate discontinuities in peer firms' loan costs.[6] Equation (2) illustrates the RD specification we use to identify the potential spillover effect:

$$Y_{i,j,t+1} = \alpha + \beta \times \mathbb{1}_{pass,j,t} + P_l\left(v_{j,t}, \gamma_l\right) + P_r\left(v_{j,t}, \gamma_r\right) + \delta X_{i,t} + \varepsilon_{i,t}. \quad (2)$$

$Y_{i,j,t+1}$ is the natural logarithm of one plus the adjusted interest rate spread of firm i, $ln(1+\text{aisd})$, in year $t+1$. Firm i is a peer firm of a focal firm j which has an ES proposal voted in its shareholder meeting in year t. $\mathbb{1}_{pass,j,t}$ is a dummy equal to 1 if Firm j passes an ES proposal on the margin in year t, and 0 otherwise. With $v_{j,t}$ representing the vote share of firm j's proposal, $P_l\left(v_{j,t}, \gamma_l\right)$ and $P_r\left(v_{j,t}, \gamma_r\right)$ are polynomial functions on the left and right side of the 50% cutoff, with polynomial orders equal to γ_l and γ_r, respectively.[7] $X_{i,t}$ is a vector of control variables for peer firm i in year t, which includes size, market-to-book value, leverage, ROA, and age. The inclusion of covariates help to mitigate small sample biases when the number of observations close to the threshold is small (Cattaneo et al., 2019).[8] The point estimate of β captures the discontinuity at the majority

[6]We choose the data-driven MSE bandwidth to minimize the mean squared error of the RD point estimator, see Imbens and Kalyanaraman (2012) for more details. We follow Calonico et al. (2017)'s package on calculating the optimal bandwidth.

[7]We use polynomial regressions of order 3 in main analysis, and use the second or fourth order polynomials in robustness checks.

[8]The simplest way to implement RD local polynomial analysis is to fit the outcomes on the vote shares alone.

threshold, and it is the difference of bank loan interest rates between *passing peers* (i.e., peers of focal firms which have a marginally passed ES proposal) and *vetoing peers* (i.e., peers of focal firms which have a marginally vetoed ES proposal). β provides a consistent estimate of the causal effect of passing an ES proposal on peer firms' loan cost.

3.3. RD validation and falsification

To identify the spillover by regression discontinuity, we rely on the local randomization assumption. This assumption in our setting means passing or rejecting a close-call ES proposal on the margin is random, and the proposal passage treatment is not associated with firm characteristics that may affect firms' bank loan cost. To check the validity of the assumption, we perform two diagnostic tests: (1) continuity in the distribution of shareholder votes;and (2) the comparison of predetermined covariatesbetween the passing peers and the vetoing peers.

McCrary test on the voting density The validity of RD using close votes has been challenged by Bach and Metzger (2019), who suggest close votes on general shareholder proposals can be disproportionately more likely to be won by management, indicating a potential manipulation of vote outcomes. We conduct the McCrary test (McCrary, 2008) on our sample of ES proposals to detect any potential manipulation of voting share around the 50% cutoff. The null hypothesis of the continuity of the density function cannot be rejected (p-value = 0.36), which suggests the voting outcome in our setting is not subject to a large and discontinuous drop around the threshold. This test result gives us confidence that our RD design captures a random variation of the effect of ES proposal passage.

Figure 1 provides a graphical representation of the test by exhibiting both a histogram of the data and the density estimate with 95% confidence intervals. The density appears smooth around

Cao et al. (2019) argue that the estimated effect using the RD approach is not affected by omitted variables, even if the variables are correlated with the vote, as long as the effects are continuous around the threshold. Although this basic specification is sufficient to conduct the analysis, Cattaneo et al. (2019) argue that including additional covariates in addition to the votes is useful in order to increase the precision of the treatment effect estimation. As illustrated by Imbens and Lemieux (2008), covariates could mitigate small sample biases particularly when the number of observations close to the threshold is small such that one has to include observations in the estimation that are further apart and may potentially differ in their covariates. In other words, controlling for covariates might eliminate some of the bias that is introduced by observations further away from the threshold (Black et al., 2007).

the threshold, and the confidence intervals (shaded areas) overlap. The graph shows no sign of votes manipulation around the cutoff and that the smoothness assumption around the voting cutoff of 50% is validated.

[Insert Figure 1 here.]

Difference in the predetermined covariates If the passage of close-call ES proposals is a random assignment, then the passing status should be unrelated to peer-firm characteristics prior to the vote (Cao et al., 2019). We follow Cattaneo et al. (2019) and estimate local linear RD effects on each covariate of interest of peer firms in the year prior to the focal firm's voting, and test if there is any significant local differences in these characteristics.Table 3 shows the test result with triangular kernel weights and the optimal mean-squared-error (MSE) bandwidths.

[Insert Table 3 here.]

We find no empirical evidence that these predetermined covariates are discontinuous at the cutoff. The loan rates are not significantly different between passing peers and vetoing peers before the voting year (p-value = 0.49), and all point estimates for the covariates are small and the 95% robust confidence intervals all contain zero, with p-values ranging from 0.16 to 0.87.[9] Figure 2 visualizes the comparison.

[Insert Figure 2 here.]

4. Empirical results

4.1. The focal effect

With the randomness assumption of the RD design validated, we first test for the presence of a focal effect. Specifically, we assess whether the passage of ES proposals influences the cost of bank loans for focal firms, applying an RD regression analogous to Equation (2), but focusing on focal firms as in Flammer (2015). Table 4 presents RD estimates (β) using third-order polynomial functions

[9] The numbers of observations vary for each covariate due to the difference in the MSE optimal bandwidths.

on either side of the voting threshold. Each column displays estimates based on one of the MSE optimal bandwidths (Calonico et al., 2017). The negative and statistically significant estimates across all columns indicate a focal effect, with a reduction in bank loan costs of 25 to 48 basis points for focal firms following the passage of ES proposals. This finding aligns with Chava (2014).[10] The approval of an ES proposal signals shareholder support for enhancing the firm's environmental and social policies, which may lead to the firm's commitments to improve ES practices, reduced regulatory scrutiny, lower litigation risk, and lower compliance costs—ultimately reducing the credit risk of focal firms. Consequently, banks lower the interest rates on loans to these firms. Alternatively, such events may ease lenders' concerns about a firm's environmental and social profile, independent of default risk, as banks no longer face potential liability or reputation risks from lending to firms with low ES profiles.

[Insert Table 4 here.]

4.2. The baseline spillover effect

Next, we test the impact of a focal firm's passage or rejection of a close-call ES proposal on the loan costs of peer firms in the following year. We estimate β in Equation (2) to determine the average spillover effect, which measures the average impact of the focal firm's passage of a close-call ES proposal on peers' bank loan cost. Table 5 presents RD estimates using third-order polynomial functions on both sides of the majority voting cutoff, and reports both the total and effective number of observations. Our results suggest that the spillover on peers' bank loan cost is statistically significant, and this significance remains consistent across varying bandwidth specifications. The different panels list the RD estimates based on the proximity of peer firms to the focal firms, measured by the bank loan similarity as specified in Equation (1).

[Insert Table 5 here.]

Columns 1–3 of Panel A report a β estimate of approximately 0.20 using data-driven optimal bandwidths. Given that the loan spread has a sample mean of 1.92% and a standard deviation

[10] Chava (2014) finds that banks charge higher interest rates to firms with environmental concerns, while firms with more environmental strengths are charged lower, though the relationship is not statistically significant.

of 1.16%, the point estimate suggests that the closest 10 passing peers experience a substantial increase of 38 basis points (or 0.33 standard deviations) in bank loan costs compared to peers whose proposals were vetoed.[11] These positive RD estimates suggest a negative spillover effect: the passage of an ES-related shareholder proposal by a focal firm leads to an increase in its peer firms' bank loan costs in the subsequent year. Figure 3 visualizes this spillover effect. Overall, we interpret this negative spillover as preliminary evidence that banks reassess the risk of their loan portfolios and raise the loan rates for peer firms following majority votes on ES proposals.

[Insert Figure 3 here.]

4.3. Spillover heterogeneity

We present evidence suggesting the documented spillover is related to banks update their belief on ES risks, and banks reprice portfolio loans in ways that are consistent with their incentives. We report two sets of spillover heterogeneity.

4.3.1. Information source

Heterogeneity of geographic proximity We explore the commonality between focal and peer firms, extending beyond shared lenders, to better understand the nature of the documented spillover and infer the type of information banks might obtain from the passage of ES proposals by focal firms. Panel A of Table 6 shows that the spillover effect is significant when focal and peer firms are located in the same state.[12] However, we find no consistent evidence of spillover when focal and peer firms operate in the same industries or product lines, based on unreported analysis.[13] These findings suggest that banks are likely gaining geographically specific information from the passage

[11] All estimates use the triangular kernel and a third-order polynomial. In untabulated analyses, similar results are obtained using the rectangular or Epanechnikov kernel, or polynomials of order 2 or 4. Table A.III reports the bias-corrected RD estimators and standard errors, following Calonico et al. (2014), the same table also reports the RD estimate with a first-order polynomial, while the global third-order polynomial estimates range from 0.04 to 0.08 depending on the closeness of peers.

[12] This result remains robust when using Bill McDonald's firm location data (source: https://sraf.nd.edu/sec-edgar-data/10-x-header-data/).

[13] In our sample, only 4.2% of peer firms share the same SIC2 industry, 1.6% the same SIC3 industry, and 2.5% the same Hoberg-Phillips industry as the focal firms.

of ES proposals, complementing studies that show geographic linkages among firms in banks' loan portfolios can drive spillovers in loan pricing (Shakya, 2022, 2023). While previous research emphasizes how banks learn from other banks, our results highlight how banks also learn firm-specific information from geographically proximate firms, reinforcing the importance of regional linkages in shaping lending decisions.

[Insert Table 6 here.]

Heterogeneity across sample periods If the spillover originates from banks' update of their beliefs on the relevance of ES risks, then such update is more likely to occur when the ES proposal passage is more informative. This would suggest that the spillover effect is stronger in the earlier period, when ES proposal passing rates were lower, making these events more informative, as industries were less focused on ES issues and green-washing concerns were minimal. Panel B in Table 6 reports the analysis based on the first vs. the second half of the sample period. The spillover is statistically significant in the early period (2005–2013), with the economic magnitude approximately two times higher than our baseline result. This suggests that banks were repricing ES risks in corporate loan rates more aggressively during this time, likely due to growing awareness of these risks and uncertainty about their potential financial impacts. As banks updated their beliefs on the relevance of ES risks, they likely integrated this understanding into loan pricing to mitigate potential credit and reputational risks. However, in the later period (2013-2022), as regulatory frameworks and market standards surrounding ES risks became more established, banks may have already priced in these factors or developed more sophisticated risk assessment tools, leading to less frequent or drastic repricing, as reflected in the diminished spillover effect. Additionally, increased investor and stakeholder pressure on firms to address ES risks during the later period may have reduced the perceived risk profile of borrowers, lessening the need for further repricing. This heterogeneity in the spillover aligns with Chava (2014), which finds a stronger effect of firms' environmental concerns on expected returns around the same time, argued to be in line with the growing environmental sensitivity over time.

Proposal heterogeneity We argue that banks are more likely to update their beliefs when the marginally passed proposal generates higher degree of disagreement between the proposing shareholder(s) and the company's management. Disagreement over the importance of a corporate ES issue signals the materiality of the associated ES risks, prompting banks to reassess the potential impact on not only the focal firm but also peer firms when determining loan rates. Using the proponent statements (from shareholders) and opponent statements (from managers) related to ES proposals, we apply textual analysis to categorize our sample ES proposals based on the level of disagreement between the two sides. The degree of disagreement is measured by the absolute value of the tone difference associated with the statements. Using the same RD approach, Panel C of Table 6 presents our findings. Greater divergence in the tone of proponent and opponent statements is associated with more significant spillover effect. These results are robust across various bandwidths and peer groups. This suggests that banks update their assessment of portfolio firms' risk when disagreement arises in the focal firm, much like sophisticated investors update their beliefs in response to market disagreements (Carlin et al., 2014).

4.3.2. Banks' repricing incentives

Heterogeneity in peer firms' bargaining power We begin by examining how spillover effects differ based on whether peer firms are financially constrained. Prior research suggests that financially constrained firms' business fundamentals are systematically more sensitive to broader economic fluctuations compared to unconstrained firms (Kashyap et al., 1996; Bernanke and Gertler, 1995; Whited and Wu, 2006). Since financially constrained firms likely have less bargaining power with banks, we expect more pronounced spillover effects for these firms. To assess a firm's degree of financial constraints, we consider factors such as firm size, age, and cash flow volatility. Table 7 presents a sub-sample analysis based on peer firms' size, age, cash flow volatility, and financial constraints following Bodnaruk et al. (2015).[14] The RD estimates are significantly positive for

[14]The financial constraint measure in Bodnaruk et al. (2015) utilizes a list of words related to financial constraints from firms' 10-K disclosures. This measure has advantages over alternative accounting based measures. We divide the sample of peers into two groups based on their proportion of constraining words relative to the total word count in that year's 10-K disclosures, compared to the yearly median value of this measure of financial constraints.

smaller and younger peers, and peers with higher cash flow volatility and tighter financial constraint, but mostly insignificant for the alternative groups. The significant RD estimates are robust across various peer neighborhoods and proposal bandwidths. Overall, we interpret these heterogeneous spillover effects as evidence that the impact of corporate ES risk on peers' bank loan costs is more pronounced for peers with weaker bargaining power in their banking relationships.

[Insert Table 7 here.]

Heterogeneity in peer firms' ES scores We proceed by examining whether the spillover effects are likely associated with peer firms' ES risks, which would align with banks' repricing incentives. If spillovers are uniform across firms with varying ES profiles, it would undermine the argument that banks' repricing on peer loans, as reflected in the spillovers, is driven by their ES risks. Alternatively, if the spillover is indeed driven by ES risks, we expect a more significant adjustments for peer firms with lower ES profiles. To do so, we conduct a sub-sample analysis based on peer firms' ES scores from Refinitiv, recorded prior to focal firms' voting, and report the results in Panel A of Table 8. The spillover effect is primarily observed among peer firms with low ES scores, indicating higher ex-ante ES risk.[15] This finding, that firms with higher ex-ante ES risk see more pronounced loan rate adjustments, strengthens the conclusion that spillover is linked to banks reassessing ES risks across their portfolios. We also divide our sample based on whether peer firms are in polluting industries, defined by the Inter-governmental Panel on Climate Change (IPCC) as major emissions sources (Choi et al., 2020).[16] Panel B of Table 8 shows that the spillover effect on loan costs is concentrated in polluting industries, which likely have weaker environmental performance. This supports the hypothesis that peer firms with poor environmental records experience more pronounced spillover effects. Additional sub-sample analysis in Table A.X confirms that spillover effects are more significant among peer firms with lower environmental scores.[17]

[15]The table also shows that peer firms with better ES ratings may experience a reduction in loan rates (though statistically insignificant), suggesting that banks reprice ES-related risks as they receive new information.

[16]We manually matched the IPCC categories with SIC 4-digit industries by industry names.

[17]In untabulated analysis, we further split our sample based on social scores, finding that spillover effects are stronger among firms with lower social performance.

[Insert Table 8 here.]

Heterogeneity in loan types The majority of bank loans in the sample consist of either term loans or credit lines. Compared to credit lines, term loans are more likely to be sold in the secondary market shortly after issuance (Drucker and Puri, 2009). Consequently, banks have less incentive to continuously monitor firms that receive term loans, unlike credit lines. Therefore, new information from ES-related proposals is more likely to influence the repricing of credit lines during amendments. To evaluate whether spillovers are more pronounced in loan costs associated with credit lines versus term loans, we perform an RD analysis using reconstructed loan rate measures, examining term loans and credit lines separately. The results in Table 9 indicate that the increase in peers' loan rates is significant primarily for credit lines, rather than term loans. This result further supports the notion that the spillover is linked to banks' incentives for monitoring loans.

[Insert Table 9 here.]

4.4. The channel for banks' repricing

In Section 4.3, the heterogeneity analyses suggest that banks may update their beliefs about the importance of a corporate ES profile for credit risk and other pricing factors following the passage of significant ES proposals. Consequently, they adjust the pricing of loans to other firms in their portfolios. A potential alternative explanation is that banks mechanically raise loan rates for peer firms to offset losses incurred from offering lower rates to focal firms, driven by capital constraints (Holmstrom and Tirole, 1997). However, two key pieces of evidence refute this. First, the repricing is concentrated on peer firms with low ES scores, indicating that it is related to ES risks rather than capital constraints. Second, the financial impact of increasing rates for peer firms exceeds the losses from lowering rates for focal firms, as the gain from repricing peer loans is approximately 4.6 times the loss from focal firms' rate cuts.[18] This supports the notion that ES risks, rather than capital constraints, drive the observed peer effects. We further explore the learning channel.

[18] The average share of loans granted to focal firms by a typical lead bank is around 15%, and the average focal effect is −47 b.p. (mserd bandwidth in Table 4) while the average spillover is 38 b.p..

4.4.1. Banks' ES risk attitudes or pricing specialities

The observed spillover effect, where banks adjust the loan rates for peer firms after a focal firm passes an ES proposal, could depend on the banks' expertise and motivation in assessing ES risks. Banks with a stronger focus or greater capability in pricing ES risks are likely to have already factored in these risks, potentially leaving peer firm loan rates unaltered while reducing rates for focal firms. On the other hand, banks with weaker incentives or less expertise may only recognize the relevance of ES risks after the focal firm's proposal passes, resulting in smaller rate reductions for focal firms and increases in peer firm rates as they adjust their loan pricing models.

To test the conjecture on focal firms' loan costs, we separate bank loans based on whether they are led by banks with a positive average environmental and social score from Refinitiv in a given year (referred to as "ES banks") and those led by banks with zero or missing environmental and social scores (referred to as "non-ES banks"). As shown in Panel A of Table 10, only ES banks significantly lowered loan rates for focal firms that narrowly passed ES proposals, while non-ES banks showed no statistically significant changes. This aligns with the findings of Chava (2014), which demonstrate that the spread on bank loans is partly driven by the environmental sensitivities of the lenders.

[Insert Table 10 here.]

Panel B of Table 10 supports the hypothesis regarding peer firms' loan costs: loan rate increases are statistically significant only for loans led by non-ES banks, whereas ES banks do not show this spillover effect. This indicates a key difference in how ES-related proposals impact banks based on their expertise or motivation regarding ES risks. Specifically, when focal firms pass ES-related proposals, ES banks adjust loan rates for these firms but leave peer firm rates unchanged, while non-ES banks increase rates for peer firms as they reassess the associated ES risks.

To further test this idea, Panel C provides a robustness check using an alternative measure of banks' ES attitudes or specialties: whether they are signatories to the Equator Principles (EP banks) or not (non-EP banks).[19] The Equator Principles represent an environmental and social risk management framework for global financial institutions (Amiram et al., 2023), aiming to encourage businesses to focus on the environmental and social impacts of large-scale projects, implying that EP banks may have stronger incentives or capabilities to incorporate ES risks into their lending decisions. The results show that the spillover effect is driven by non-EP banks, suggesting that banks not specialized in or motivated to price ES risks are more likely to update their beliefs and reprice loans for peer firms following the passage of a focal firm's ES-related proposal. This aligns with the view that non-EP banks learn new information from these events, while EP banks are already factoring in such risks.

4.4.2. Non-ES banks' repricing relates to peers' ex-ante ES risk

To further investigate how non-ES banks update their beliefs on the relevance of ES risks or profiles, we split the sample based on peer firms' ex-ante ES scores, conditional on loans issued or led by non-ES banks. The rationale is that if non-ES banks reassess ES risks and adjust their loan rates following the passage of an ES-related proposal by one of their portfolio firms, we would expect loan rates to increase for peer firms with lower ex-ante ES performance and decrease for those with higher ex-ante ES performance. Focusing on peer firms with ES scores from Refinitiv and loans issued or led by non-ES banks, we split the sample by the yearly median ES scores. The results in Table 11 indicate that the spillover effect is primarily driven by non-ES banks raising loan rates for firms with low ES reputations. In contrast, for firms with high ex-ante ES reputations, non-ES banks decrease loan rates, although the effect is not statistically significant. These findings provide further confidence that the spillover effect arises from banks updating their beliefs about the relevance of ES risks.

[Insert Table 11 here.]

[19] One concern about ES scores is that they have been criticized for subjectivity and inconsistencies among different rating agencies (Berg et al., 2022). We thus use the EP signatories as an alternative measure of bank's ES attitude. The list of EP banks is obtained from https://equator-principles.com/epfis-reporting/. In our sample, 36% of lead banks associated with a parent bank that has adopted the EP.

5. Robustness and further analyses

5.1. Placebo tests - RD using artificial cutoffs

As a placebo test, we replace the 50% voting cutoff with several artificial thresholds and rerun the main RD regression. If the spillover is indeed due to proposal passage, we should not see a change in peers' loan costs when the voting outcome crosses a threshold other than 50%. By assigning an artificial cutoff of 25% on the voting outcome, that is, to assign a proposal the passing status if the voting outcome is more than 25%, Table A.IV in the appendix shows the RD estimates of our main regression is not statistically significant. The estimates remain insignificant when we use 30%, 60%, or 65% (and more in unreported results) as the proposal passing cutoff. These results strengthen our confidence that the observed spillover effect is driven by the passage of the focal firm's proposals.

5.2. Excluding annual meetings with mixed information

In a few instances, focal firms may have both passed and vetoed ES proposals in the same annual meeting, which could make the information conveyed by the voting outcomes less clear. As a robustness check, we exclude focal firms that had both passed and failed proposals during the same meeting and rerun the RD regression, consistent with the baseline analysis. The results, shown in Table A.V, indicate that the negative spillover effect remains statistically significant across all reported bandwidths and for neighborhood peers. However, the economic significance of the spillover is slightly reduced. This suggests that while the presence of both passed and failed proposals may introduce some noise, it does not fundamentally alter the overall findings.

5.3. Driving force: the passage or veto of proposals?

Our identification relies on the assumption that passing the majority voting threshold is random and thus the passage drives the spillover. While passed proposals likely garner more attention from banks, previous literature suggests that vetoed proposals can also convey relevant information about ES risk (He et al., 2023). Therefore, it is plausible that vetoed proposals on the margin could also contribute to the spillover effect. To explore this, we conduct a difference-in-differences (DID) analysis to determine whether the spillover is driven by passed proposals increasing peers' loan costs or by vetoed proposals reducing them. In this analysis, the treated groups consist of passing peers or vetoing peers, while the control groups are firms with similar characteristics as the treated peers. Panel A of Table A.VI in the appendix confirms that passing and vetoing peers are generally comparable to their control firms, although vetoing peers tend to be slightly older on average. Importantly, the propensity to become vetoing peers is similar between the two groups. Panel B reveals that passing peers experience a significant increase in loan rates following the proposal vote, whereas loan rates for vetoing peers do not change significantly. This evidence strengthens our argument that the spillover effect is indeed driven by the passage of ES proposals at the margin, rather than by vetoed proposals.

5.4. Control for peers' ES scores

An alternative explanation for non-ES banks adjusting loan rates for their portfolio firms is the *matching hypothesis*. This hypothesis suggests that non-ES banks are more likely to be paired with firms that already have higher ES risks (Houston and Shan, 2022), which might lead these banks to adjust loan costs more aggressively for their portfolio firms. This explanation differs subtly from our proposed channel, as we argue that non-ES banks recognize and react to ES-related risks, adjusting the loan terms accordingly. However, we do not find evidence supporting the matching hypothesis. First, when comparing the ES scores of firms that borrow from ES (or EP) banks versus those borrowing from non-ES (or non-EP) banks, the results do not indicate that non-ES banks are systematically matched with higher-ES risk firms. Second, when we include firms' ES scores in our RD regressions, our baseline results remain qualitatively unchanged. This suggests that firms' ES scores are not the primary driver of the observed spillover, further weakening the case for the matching hypothesis as an alternative explanation.

5.5. Alternative dependent variables

Besides loan rates, banks could also adjust peer firms' loan size, maturity, or covenants, among other terms. For example, as noted by Chava (2014), banks might refrain from lending to a firm based on its environmental profile, either due to social responsibility concerns or to avoid potential lender liability and reputational risk. In our sample, however, we do not find significant changes in peer firms' loan growth, loan maturity (Table A.VII in the appendix), or loan covenants before or after the passage of focal firms' ES proposals.[20]

It is unlikely but possible that peer firms change their ES profiles after the focal firm's event, which explains the negative spillover. In untabulated analysis, we find no significant changes in peer firms' ES scores (measured by Refinitiv or MSCI KLD) from the voting year to the following year or the year after. This finding suggests that the spillover effect on bank loan costs is not driven by peers altering their ES-related risk profiles in the subsequent years.

5.6. Persistence of the spillover

To investigate if the spillover of ES risk on peers' loan cost persists, we analyze peers' loan rate at $t+2$ using the same RD setting as the baseline. As shown in Table A.VIII in the appendix, the spillover effect largely disappears two years after the passage of focal firms' ES-related proposals. This suggests that banks may have overreacted in initially repricing the ES risks of peer firms. Our findings align with Correa et al. (2023) who documents that while banks attempt to price climate change risk into corporate loans based on new information, much of this pricing reflects an overreaction to climate-related risks.

5.7. Loans through common vs. non-common lenders

We posit that spillover effects primarily result from the influence of common lenders, with significant impacts expected to manifest in the loan rates they charge. In contrast, we do not expect

[20] Additionally, we employ RD analysis to assess the cumulative abnormal returns (CAR) from the stock market, to determine if there is any spillover effect on firms' cost of equity. Figure A.I in the Appendix shows no statistically significant discontinuity in CARs around the voting threshold for either focal or peer firms. The impact of ES proposal passage is predominantly reflected in loan costs, with no discernible effect on the cost of equity in our sample, possibly because the financial markets have already priced in ES-related credit risks.

non-common lenders to substantially adjust the overall loan rate following the passage of focal firms' ES proposals. To test this hypothesis, we reanalyze loan rates, distinguishing between loans from common and non-common lenders. Notably, the firm-year loan rate used in our main analysis reflects the average loan spread across all loans held by a firm, irrespective of whether they are from common or non-common lenders. Table A.IX in the appendix provides RD estimation results, which show that spillover effects on peer firms' loan rates are primarily driven by common lenders.

6. Concluding remarks

Our study provides new evidence on the spillover effects of corporate ES risks on peer firms' bank loan costs, highlighting a novel channel through which banks reassess their loan portfolios. Utilizing a regression discontinuity design, we demonstrate that the approval of ES-related shareholder proposals, which typically reduces the focal firm's ES risks, results in an increase in loan costs for peer firms. This spillover effect is primarily driven by banks with less expertise or weaker incentives to price ES risks, as they update their beliefs regarding the relevance of such risks for the firms in their portfolios. Our findings contribute to the growing literature on the indirect effects of ES risks on financial markets, suggesting that corporate ES decisions can have broader implications beyond individual firms.

Our results have meaningful implications for both corporate managers and lenders. For corporate managers, the finding that improvements in a firm's ES profile can unintentionally increase borrowing costs for peer firms highlights the complex, interconnected nature of corporate finance decisions related to ES risks. For banks, this study underscores the importance of refining tools and developing deeper expertise in assessing ES risks, as these factors are becoming increasingly integral to loan pricing. Future research could examine other dimensions of ES risks and their influence on different types of financing, as well as the broader, long-term consequences of these spillovers for corporate decision-making.

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Figure 1: Continuity Check of Vote Margin Distribution

This figure presents a visualization of the McCrary (2008) test for the continuity of the vote share distribution around the 50% threshold. The horizontal axis represents the vote margin (vote share minus 50%), while the vertical axis shows the density. The p-value for the null hypothesis of continuity at 0 is 0.3594, suggesting no evidence of manipulation around the threshold. Further details can be found on Page 14.

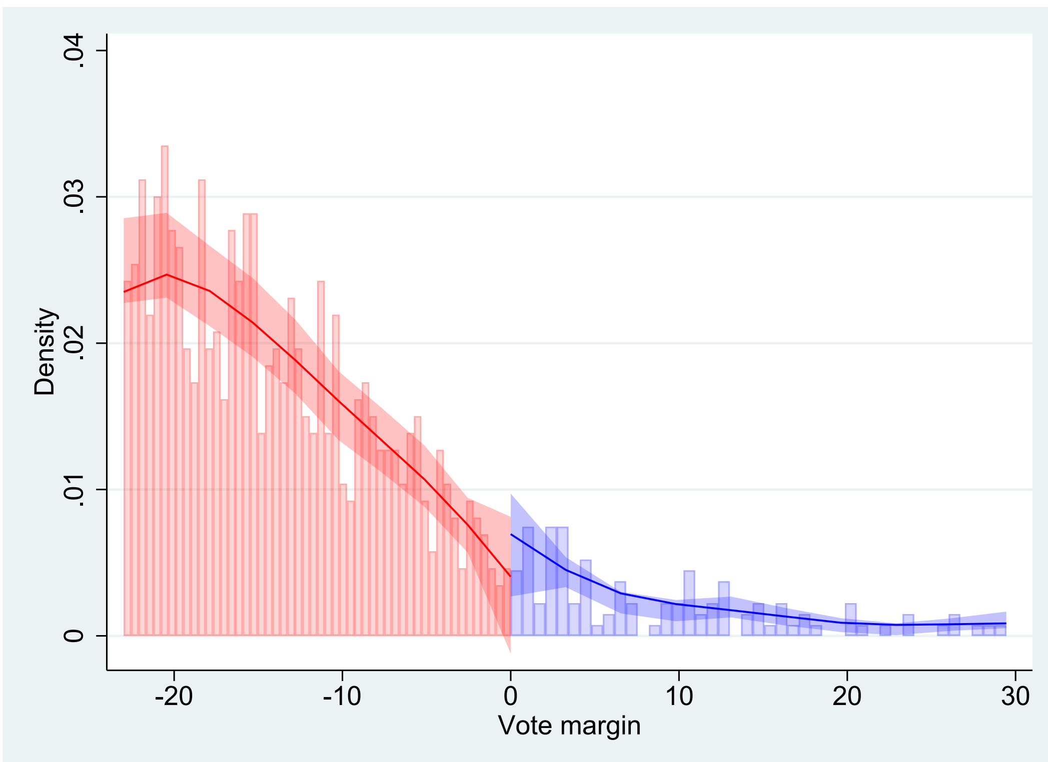


Figure 2: RD Effect for Predetermined Variables

This figure presents a visualization of the RD plot for peer firms' characteristics one year before the vote occurs. The variables included are log aisd, log total assets, log market value, ROA, Market-to-book, debt-to-capital ratio, leverage, cash over asset, and the ES score from Refinitiv. Each plot illustrates the relationship between the vote margin and these firm characteristics, providing insight into potential discontinuities around the 50% voting threshold.

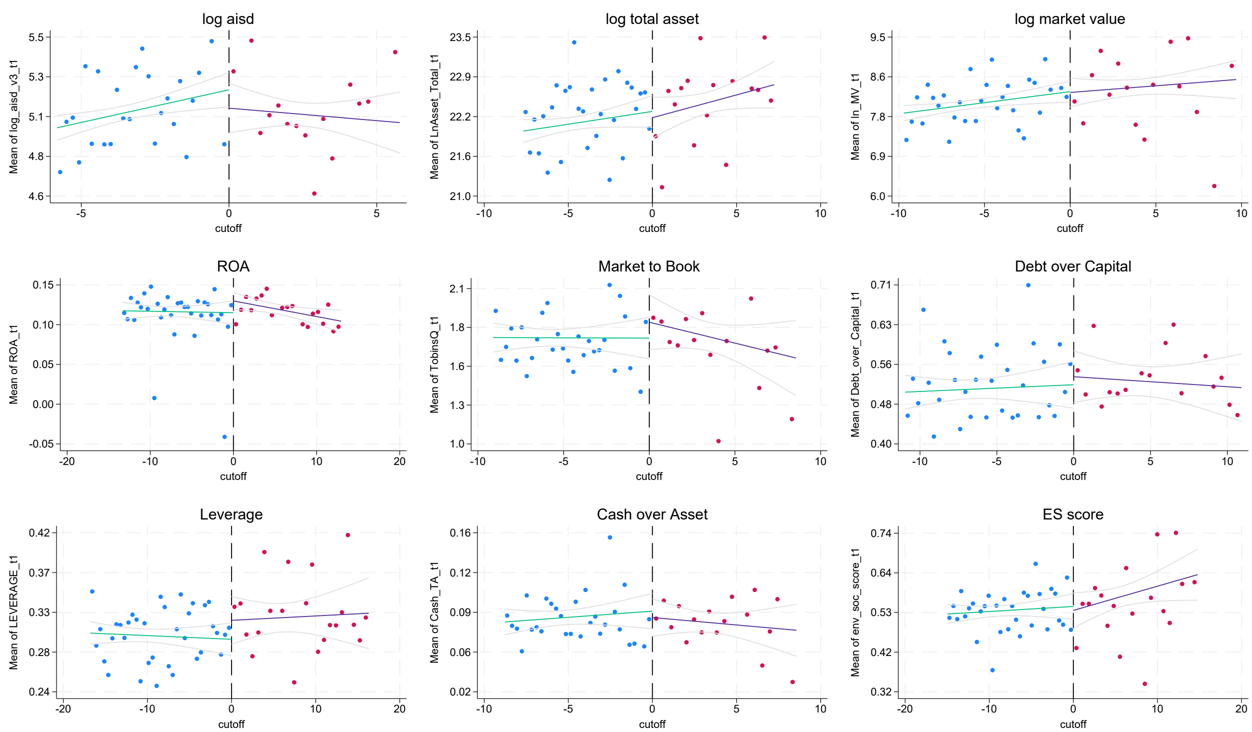


Figure 3: RD Plot for Peers' Bank Loan Cost

This figure presents a visualization of the RD plot for peer firms' $ln(1+\text{aisd})$ in year $t+1$, near the cutoff. The three subfigures depict the results for different groups of peers based on the lending-relation similarity described in Equation (1). The fitted curves are based on quadratic regressions, and the lighter-colored lines represent the boundaries of the 90% confidence intervals. The MSE optimal bandwidth for each case is selected according to Calonico et al. (2017).

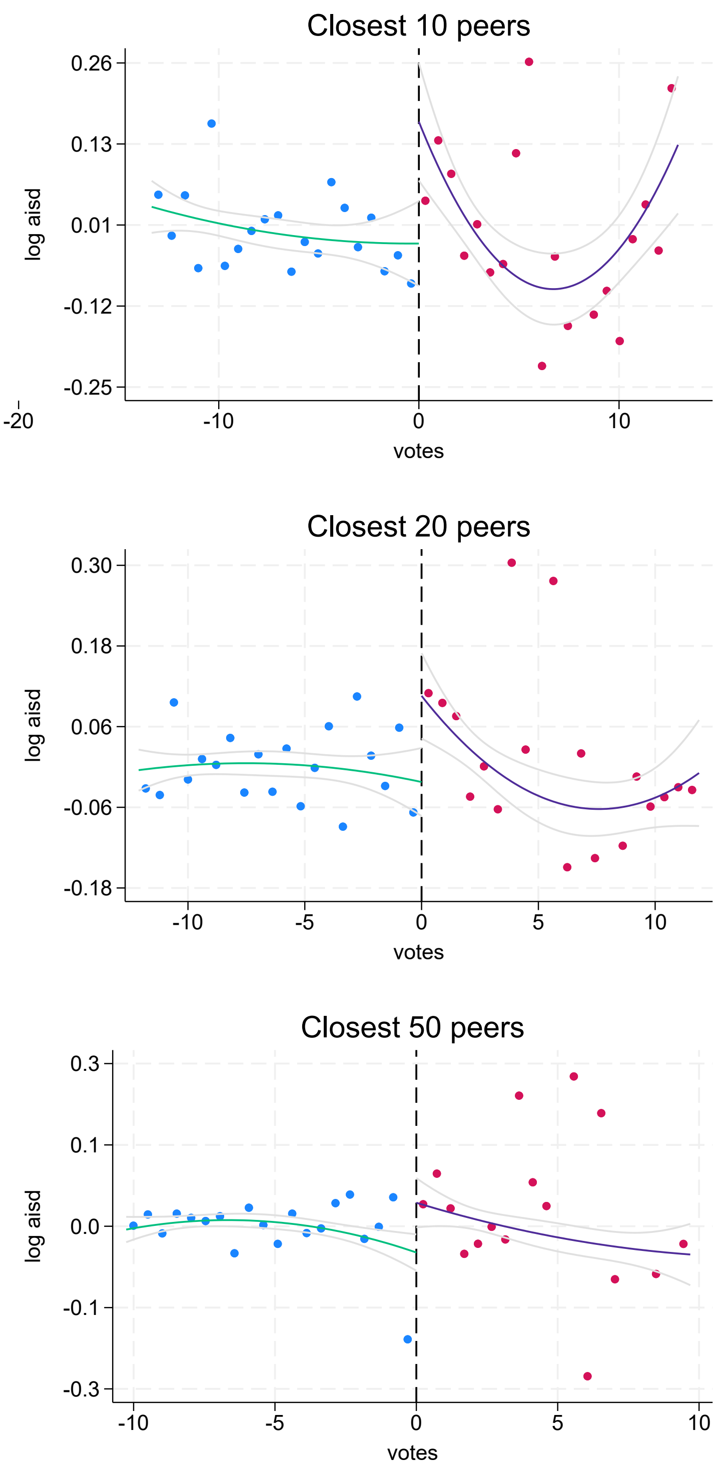


Table 1: Summary Statistics for the Sample of ES Proposals

This table presents the statistics of shareholder proposals in the sample, focusing on ES-related proposals used in our study from the RiskMetrics and SharkRepellent databases from 2005 to 2021. The initial sample includes 3,613 ES-related proposals, with the filtering process detailed in Section 3.1. Proposals are categorized by voting year in Panel A and by proposal type in Panel B. Please note that RiskMetrics does not provide sub-categories for proposal types.

Panel A: Summary statistics by year

Year	Shareholder proposals	Approved proposals	Approved proposals(%)	Average vote outcome(%)	SD vote outcome(%)	Vote outcome ±7.5%	Vote outcome ±10%	Vote outcome ±12.5%
2005	95	1	1.05	12.08	10.33	2	2	4
2006	103	1	0.97	15.63	13.25	2	3	5
2007	109	3	2.75	20.07	15.13	6	7	11
2008	106	1	0.94	17.76	13.00	2	2	8
2009	110	2	1.82	18.65	14.31	5	8	15
2010	111	2	1.80	21.62	14.37	6	13	17
2011	100	4	4.00	23.83	16.91	12	16	20
2012	109	1	0.92	21.81	14.32	9	12	17
2013	124	5	4.03	24.82	16.69	10	15	22
2014	128	5	3.91	25.47	15.19	15	21	31
2015	129	0	0.00	23.49	12.69	7	12	16
2016	126	9	7.14	26.65	16.31	14	20	24
2017	124	5	4.03	26.09	14.94	11	17	23
2018	106	10	9.43	29.81	15.69	18	23	31
2019	109	10	9.17	32.23	16.50	18	25	32
2020	99	17	17.17	33.57	17.37	25	30	35
2021	87	32	36.78	43.68	25.52	15	21	28
Total	1,875	108	5.76	24.43	16.94	177	247	339

Panel B: Summary statistics by type of ES proposal

Proposal	Shareholder proposals	Approved proposals	Approved proposals(%)	Average vote outcome(%)	SD vote outcome(%)	Vote outcome ±7.5%	Vote outcome ±10%	Vote outcome ±12.5%
Animal	91	2	2.20	7.62	14.22	0	0	0
Environmental	365	29	7.95	25.66	17.54	38	56	68
Health	114	3	2.63	14.15	13.36	4	6	7
Human Rights	147	4	2.72	18.22	15.54	8	8	13
Labor	214	15	7.01	24.78	17.56	23	30	43
Minority/Woman Board	44	10	22.73	30.48	24.41	5	8	9
Political	660	32	4.85	28.73	13.71	79	113	155
Social	49	1	2.04	13.41	16.69	1	1	2
Sustainability	135	8	5.93	29.22	16.43	17	22	35
Riskmetrics proposals	56	4	7.14	22.05	19.16	2	3	7

Table 2: Summary Statistics of Focal and Peer Firms

This table presents the summary statistics for our key outcome and control variables for both focal and peer firms. Based on 1,819 unique ES proposals that were voted on, the sample includes 102,198 peer-year and 1,833 focal firm-year observations from 2005 to 2021. All continuous variables have been winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix A.I.

Variable	No of Obs.	Mean	Std. Deviation	*p*25	Median	*p*75
Log (total assets):						
Focal firm	1,375	24.24	1.36	23.39	24.41	25.27
10 closest peers	11,418	22.09	1.62	20.90	22.02	23.21
20 closest peers	23,611	22.02	1.62	20.83	21.93	23.12
50 closest peers	58,250	21.98	1.60	20.82	21.90	23.03
Leverage:						
Focal firm	1,375	0.53	0.31	0.34	0.49	0.67
10 closest peers	11,418	0.51	0.36	0.29	0.45	0.67
20 closest peers	23,611	0.50	0.36	0.27	0.44	0.67
50 closest peers	58,250	0.50	0.37	0.27	0.45	0.68
ROA:						
Focal firm	1,374	0.13	0.09	0.08	0.12	0.17
10 closest peers	11,418	0.12	0.10	0.08	0.12	0.16
20 closest peers	23,611	0.12	0.10	0.08	0.12	0.16
50 closest peers	58,250	0.12	0.11	0.08	0.12	0.16
Market-to-book:						
Focal firm	1,375	1.86	1.14	1.18	1.46	2.06
10 closest peers	11,418	1.78	1.09	1.14	1.46	2.01
20 closest peers	23,611	1.79	1.13	1.14	1.46	2.03
50 closest peers	58,250	1.79	1.15	1.14	1.46	2.02
Age:						
Focal firm	1,366	40.51	25.05	19.00	38.00	69.00
10 closest peers	11,418	25.85	20.79	9.00	20.00	40.00
20 closest peers	23,611	25.33	20.40	9.00	20.00	38.00
50 closest peers	58,250	24.65	20.11	9.00	19.00	37.00
Log (1+aisd):						
Focal firm	1,375	4.60	0.75	4.33	4.71	5.02
10 closest peers	11,418	5.10	0.57	4.82	5.05	5.47
20 closest peers	23,611	5.10	0.58	4.80	5.05	5.47
50 closest peers	58,250	5.11	0.57	4.82	5.07	5.48
Aisd:						
Focal firm	1,375	140.67	106.10	77.94	112.50	156.88
10 closest peers	11,418	191.80	116.29	122.50	155.00	235.98
20 closest peers	23,611	190.53	114.51	120.37	155.00	236.50
50 closest peers	58,250	192.20	115.32	122.50	158.57	238.41

Table 3: Validity of the RD Design: Balance of Predetermined Covariates

This table presents the differences in observable characteristics between the 10 closest passing peer firms (those closest in lending relationship to the focal firm that passes an ES-related proposal) and the 10 closest vetoing peer firms in the year prior to the focal firm's vote on the ES-related proposal. The reported RD estimates are based on a first-order polynomial and the optimal bandwidth determined by the mserd method (Calonico et al., 2017). For further details, refer to the explanation on Page 14.

Variable	RD Estimate	*p*-value	Effective no. of obs
log aisd	0.05	0.49	1098
log total asset	-0.05	0.78	1365
log market value	0.03	0.87	1665
ROA	0.01	0.16	2592
Market to Book	0.05	0.70	1547
Debt over Capital	0.01	0.85	2137
Leverage	0.01	0.56	3881
Cash over Asset	-0.00	0.78	1574
ES score	-0.01	0.73	2043

Table 4: RD Estimates on Focal Firms' Bank Loan Costs

This table presents the RD estimates for focal firms' loan rates at $t+1$. The estimation follows a similar approach to Equation (2) (applied to focal firms). The estimates are calculated using three different optimal bandwidths, as proposed by Calonico et al. (2017). Control variables include firm size, market-to-book ratio, leverage, ROA, and age, with variable definitions provided in Appendix A.I. Standard errors are clustered at the firm level and reported in parentheses. Statistical significance is indicated by *, **, and ***, corresponding to the 10%, 5%, and 1% significance levels, respectively. See further explanation on Page 15.

	(1) mserd	(2) msesum	(3) msecomb2
RD Estimate	-0.337***	-0.231***	-0.344***
	(0.103)	(0.087)	(0.100)
Observations	3,340	3,340	3,340
Effective Obs.	385	433	423

Table 5: Spillover Effects on Peers' Bank Loan Costs

This table presents the RD estimates for peer firms' loan rates following the passage of focal firms' ES proposals, corresponding to the estimate of β in Eq (2). The dependent variable is the natural logarithm of 1 plus the adjusted interest rate spread $ln(1+\text{aisd})$ on peer firm's new or amended bank loans in Year $t+1$, weighted by loan amounts. Columns 1 – 3 provide estimates based on optimal bandwidths as recommended by Calonico et al. (2017), while Columns 5 and 6 show estimates using 50% and 150% of the optimal bandwidth, respectively. Control variables include firm size, market-to-book, leverage, ROA, and firm age, with definitions provided in Appendix A.I. Standard errors are clustered at the firm level and shown in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Further details are explained on Page 15.

	(1) mserd	(2) msesum	(3) msecomb2	(4) 50% of mserd	(5) 150% of mserd
Panel A: Closest 10 peers					
RD Estimate	0.197**	0.199**	0.196**	0.287**	0.212***
	(0.087)	(0.085)	(0.086)	(0.139)	(0.072)
Observations	11,418	11,418	11,418	11,418	11,418
Effective Obs.	3,773	4,057	4,057	1,477	6,484
Panel B: Closest 20 peers					
RD Estimate	0.156**	0.157**	0.157**	0.266***	0.139***
	(0.063)	(0.063)	(0.063)	(0.093)	(0.052)
Observations	23,611	23,611	23,611	23,611	23,611
Effective Obs.	7,819	7,994	7,994	2,985	13,108
Panel C: Closest 50 peers					
RD Estimate	0.117***	0.109***	0.113***	0.223***	0.102***
	(0.038)	(0.035)	(0.037)	(0.055)	(0.032)
Observations	58,250	58,250	58,250	58,250	58,250
Effective Obs.	19,044	25,428	22,964	7,209	32,114

Table 6: Spillover Heterogeneity: Sources of Information

This table presents RD estimates on spillover heterogeneity. The dependent variable is a peer firm's loan cost, measured as $ln(1+\text{aisd})$ on its new or amended bank loans. Panel A divides the sample based on whether the focal and peer firms are located within the same state. Panel B splits the sample by period (2005–2013 vs. 2014–2021), and Panel C categorizes ES proposals based on the level of disagreement between the proponent's and opponent's statements related to the ES proposal. The column headings indicate the exact MSE-optimal bandwidth selectors for the RD estimators, as proposed by Calonico et al. (2017). Control variables include firm size, market-to-book ratio, leverage, and ROA, with definitions provided in Appendix A.I. Standard errors are clustered at the firm level and shown in parentheses. Statistical significance is indicated by *, **, and ***, corresponding to the 10%, 5%, and 1% significance levels, respectively. Further details are explained on Page 18.

	(1) mserd	(2) msesum	(3) msecomb2	(4) mserd	(5) msesum	(6) msecomb2
Panel A: Spillover by geographic locations betwen focal and peer firms						
Closest 10 peers	Same state ($N = 5,060$)			Different state ($N = 6,606$)		
RD Estimate	0.279*	0.293*	0.287*	0.060	0.062	0.061
	(0.150)	(0.154)	(0.152)	(0.123)	(0.122)	(0.123)
Closest 20 peers	Same state ($N = 10,329$)			Different state ($N = 13,919$)		
RD Estimate	0.208**	0.209**	0.206**	0.065	0.066	0.066
	(0.099)	(0.103)	(0.100)	(0.081)	(0.080)	(0.081)
Closest 50 peers	Same state ($N = 25,256$)			Different state ($N = 34,437$)		
RD Estimate	0.129**	0.130**	0.129**	0.080	0.082*	0.080
	(0.059)	(0.059)	(0.059)	(0.049)	(0.049)	(0.049)
Panel B: Spillover by time period						
Closest 10 peers	Earlier-half sample ($N = 5,710$)			Later-half sample ($N = 5,708$)		
RD Estimate	0.674**	0.686**	0.686**	0.052	0.076	0.069
	(0.274)	(0.275)	(0.275)	(0.086)	(0.081)	(0.083)
Closest 20 peers	Earlier-half sample ($N = 12,114$)			Later-half sample ($N = 11,497$)		
RD Estimate	0.331**	0.338**	0.331**	0.079	0.081	0.089
	(0.141)	(0.131)	(0.141)	(0.067)	(0.062)	(0.065)
Closest 50 peers	Earlier-half sample ($N = 29,924$)			Later-half sample ($N = 28,326$)		
RD Estimate	0.421***	0.428***	0.419***	0.000	-0.001	0.003
Panel C: Spillover by proposal disagreement						
Closest 10 peers	Low disagreement ($N = 6,035$)			High disagreement ($N = 5,024$)		
RD Estimate	0.002	0.025	0.011	0.240**	0.314***	0.317***
	(0.150)	(0.178)	(0.173)	(0.101)	(0.115)	(0.113)
Closest 20 peers	Low disagreement ($N = 12,385$)			High disagreement ($N = 10,492$)		
RD Estimate	-0.057	-0.057	-0.057	0.233***	0.260***	0.238***
	(0.128)	(0.129)	(0.128)	(0.074)	(0.079)	(0.075)
Closest 50 peers	Low disagreement ($N = 30,884$)			High disagreement ($N = 25,602$)		
RD Estimate	-0.034	-0.035	-0.034	0.151***	0.162***	0.162***
	(0.074)	(0.074)	(0.074)	(0.042)	(0.044)	(0.044)

Table 7: Spillover Heterogeneity: Peer Firm Characteristics

This table is similar to Table 5, but it presents results from sub-sample analyses based on firm characteristics. Panel A, B, C, and D divide the sample into two halves based on peer firms' sizes, ages, cash flow volatility, and financial constraints, respectively. Control variables include firm size, market-to-book ratio, leverage, and ROA, with definitions provided in Appendix A.I. Standard errors are clustered at the firm level and reported in parentheses. Statistical significance is denoted by *, **, and ***, corresponding to the 10%, 5%, and 1% levels, respectively. Further details are explained on Page 19.

	(1)	(2)	(3)	(4)	(5)	(6)
	mserd	msesum	msecomb2	mserd	msesum	msecomb2
Panel A: Spillover by peer size						
Closest 10 peers	Small ($N = 5,875$)			Big ($N = 5,543$)		
RD Estimate	0.356***	0.359***	0.354***	0.050	0.052	0.051
	(0.135)	(0.136)	(0.135)	(0.113)	(0.112)	(0.112)
Closest 20 peers	Small ($N = 12,204$)			Big ($N = 11,407$)		
RD Estimate	0.176**	0.170*	0.174**	0.130	0.131	0.131
	(0.086)	(0.087)	(0.086)	(0.087)	(0.088)	(0.087)
Closest 50 peers	Small ($N = 30,118$)			Big ($N = 28,132$)		
RD Estimate	0.153***	0.145***	0.140**	0.086*	0.086*	0.085*
	(0.057)	(0.053)	(0.055)	(0.047)	(0.048)	(0.047)
Panel C: Spillover by peer cash flow volatility						
Closest 10 peers	Low volatility ($N = 5,729$)			High volatility ($N = 5,659$)		
RD Estimate	0.114	0.115	0.114	0.296**	0.295**	0.296**
	(0.104)	(0.104)	(0.104)	(0.137)	(0.137)	(0.137)
Closest 20 peers	Low volatility ($N = 11,816$)			High volatility ($N = 11,700$)		
RD Estimate	0.035	0.032	0.032	0.292***	0.287***	0.294***
	(0.069)	(0.072)	(0.072)	(0.098)	(0.095)	(0.097)
Closest 50 peers	Low volatility ($N = 29,093$)			High volatility ($N = 28,939$)		
RD Estimate	0.043	0.039	0.034	0.185***	0.168***	0.165***
	(0.047)	(0.048)	(0.048)	(0.056)	(0.052)	(0.054)

	(1)	(2)	(3)	(4)	(5)	(6)
	mserd	msesum	msecomb2	mserd	msesum	msecomb2
Panel B: Spillover by peer age						
Closest 10 peers	Younger ($N = 5,414$)			Older ($N = 6,004$)		
RD_Estimate	0.217*	0.222*	0.220*	0.177	0.177	0.177
	(0.123)	(0.122)	(0.123)	(0.128)	(0.128)	(0.128)
Closest 20 peers	Younger ($N = 11,465$)			Older ($N = 12,146$)		
RD Estimate	0.222***	0.223***	0.222***	0.074	0.085	0.086
	(0.084)	(0.084)	(0.084)	(0.090)	(0.081)	(0.087)
Closest 50 peers	Younger ($N = 28,548$)			Older ($N = 29,702$)		
RD Estimate	0.146***	0.154***	0.147***	0.088	0.087	0.087
	(0.051)	(0.052)	(0.051)	(0.052)	(0.053)	(0.053)
Panel D: Spillover by peer financial constraints						
Closest 10 peers	Less constrained ($N = 6,015$)			More constrained ($N = 5,403$)		
RD Estimate	0.020	0.023	0.015	0.381***	0.381***	0.381***
	(0.107)	(0.112)	(0.108)	(0.138)	(0.138)	(0.138)
Closest 20 peers	Less constrained ($N = 12,395$)			More constrained ($N = 11,216$)		
RD Estimate	0.019	0.018	0.019	0.292***	0.296***	0.296***
	(0.079)	(0.080)	(0.079)	(0.094)	(0.094)	(0.094)
Closest 50 peers	Less constrained ($N = 30,663$)			More constrained ($N = 27,587$)		
RD Estimate	0.057	0.050	0.040	0.176***	0.176***	0.176***
	(0.049)	(0.053)	(0.051)	(0.059)	(0.058)	(0.058)

Table 8: Spillover Heterogeneity: Peer Firm ES Score or Industry

This table is similar to Table 5, but it presents results from sub-sample analyses based on peer firms' ES scores from Refinitiv (Panel A) or whether the peer firm operates in a polluting or non-polluting industry (Panel B). The dependent variable is the peer firm's loan cost, measured as $ln(1+\text{aisd})$ on its new or amended bank loans. The column headings display the exact MSE-optimal bandwidth selectors for the RD estimators, as proposed by Calonico et al. (2017). Control variables include firm size, market-to-book ratio, leverage, and ROA, with definitions provided in Appendix A.I. Statistical significance is indicated by *, **, and ***, corresponding to the 10%, 5%, and 1% significance levels, respectively. Further details are explained on Page 21.

	(1) mserd	(2) msesum	(3) msecomb2	(4) mserd	(5) msesum	(6) msecomb2
Panel A: spillover by peer firms' ex-ante ES scores						
Closest 10 peers	Low ES Score ($N=4,690$)			High ES Score ($N=4,704$)		
RD Estimate	0.310**	0.315**	0.315**	-0.110	-0.110	-0.109
	(0.126)	(0.124)	(0.125)	(0.121)	(0.120)	(0.120)
Closest 20 peers	Low ES Score ($N=8,820$)			High ES Score ($N=8,962$)		
RD Estimate	0.272***	0.272***	0.273***	-0.060	-0.060	-0.060
	(0.107)	(0.105)	(0.106)	(0.083)	(0.083)	(0.083)
Closest 50 peers	Low ES Score ($N=14,960$)			High ES Score ($N=15,075$)		
RD Estimate	0.231***	0.209***	0.229***	0.023	0.021	0.022
	(0.083)	(0.077)	(0.081)	(0.055)	(0.054)	(0.054)
Panel B: spillover by peer firms' industries						
Closest 10 peers	Polluting industries ($N=6,316$)			Non-polluting industries ($N=5,102$)		
RD Estimate	0.252**	0.260**	0.260**	0.167	0.156	0.164
	(0.122)	(0.120)	(0.120)	(0.114)	(0.112)	(0.114)
Closest 20 peers	Polluting industries ($N=10,110$)			Non-polluting industries ($N=13,501$)		
RD Estimate	0.243***	0.246***	0.248***	0.103	0.081	0.101
	(0.093)	(0.092)	(0.092)	(0.083)	(0.081)	(0.083)
Closest 50 peers	Polluting industries ($N=24,210$)			Non-polluting industries ($N=34,040$)		
RD Estimate	0.166***	0.150***	0.151***	0.078	0.068	0.075
	(0.057)	(0.055)	(0.056)	(0.049)	(0.048)	(0.049)

Table 9: Spillover Heterogeneity: Loan Types

This table is similar to Table 5, but it presents results from sub-sample analyses based on loan types (term loans vs. credit lines). The dependent variable is a peer firm's loan cost $ln(1+\text{aisd})$, on its new or amended bank loans in Year $t+1$, weighted by loan amounts. The column headings display the exact MSE-optimal bandwidth selectors for the RD estimators, as proposed by Calonico et al. (2017). Control variables include firm size, market-to-book ratio, leverage, and ROA, with variable definitions provided in Appendix A.I. Standard errors are clustered at the firm level and reported in parentheses. Statistical significance is indicated by *, **, and ***, representing the 10%, 5%, and 1% significance levels, respectively. Further details can be found on Page 20.

	(1) mserd	(2) msesum	(3) msecomb2	(4) mserd	(5) msesum	(6) msecomb2
Closest 10 peers	Term loans ($N=4,965$)			Lines of credits ($N=9,835$)		
RD Estimate	0.011 (0.137)	0.014 (0.136)	0.013 (0.136)	0.209** (0.083)	0.205** (0.085)	0.206** (0.084)
Closest 20 peers	Term loans ($N=10,022$)			Lines of credits ($N=20,443$)		
RD Estimate	0.069 (0.087)	0.067 (0.092)	0.072 (0.089)	0.107* (0.058)	0.107* (0.057)	0.108* (0.057)
Closest 50 peers	Term loans ($N=25,146$)			Lines of credits ($N=50,677$)		
RD Estimate	0.054 (0.060)	0.055 (0.060)	0.056 (0.060)	0.081** (0.036)	0.080** (0.034)	0.080** (0.035)

Table 10: The Spillover Channel: Loan Repricing by Non-ES Banks

This table presents the estimate of β in Eq (2), similar to Table 5. Panel A provides the RD estimates of bank loan costs for the focal firms. In Panel B, loans are separated into those led by ES banks (banks with positive ES scores according to Refinitiv) versus non-ES banks. Panel C splits loans into those led by banks adopting Equator Principals (EP banks) versus non-EP banks. The column headings display the exact MSE-optimal bandwidth selectors for the RD estimators, proposed by Calonico et al. (2017). Control variables include firm size, market-to-book ratio, leverage, and ROA, with definitions provided in Appendix A.I. Standard errors are clustered at the firm level and reported in parentheses. Statistical significance is denoted by *, **, and ***, corresponding to the 10%, 5%, and 1% significance levels, respectively. Further details can be found on Page 21.

bandwidth	(1) mserd	(2) msesum	(3) msecomb2	(4) mserd	(5) msesum	(6) msecomb2
Panel A: ES banks vs non-ES Banks [focal firms]						
Focal firms	ES Bank ($N = 1,310$)			Non-ES Bank ($N = 2,030$)		
RD Estimate	-0.519***	-0.426***	-0.470***	0.061	-0.102	-0.109
	(0.097)	(0.069)	(0.070)	(0.132)	(0.177)	(0.168)
Panel B: ES banks vs. non-ES banks [peer firms]						
Closest 10 peers	ES Bank ($N = 2,646$)			Non-ES Bank ($N = 8,772$)		
RD Estimate	0.141	0.144	0.144	0.253**	0.254**	0.258**
	(0.157)	(0.158)	(0.158)	(0.109)	(0.106)	(0.108)
Closest 20 peers	ES Bank ($N = 5,198$)			Non-ES Bank ($N = 18,413$)		
RD Estimate	0.092	0.092	0.093	0.177**	0.179**	0.179**
	(0.090)	(0.090)	(0.090)	(0.080)	(0.079)	(0.080)
Closest 50 peers	ES Bank ($N = 12,228$)			Non-ES Bank ($N = 46,022$)		
RD Estimate	0.007	0.007	0.008	0.169***	0.166***	0.164***
	(0.057)	(0.058)	(0.058)	(0.049)	(0.048)	(0.048)
Panel C: EP banks vs. non-EP banks [peer firms]						
Closest 10 peers	EP Bank ($N = 3,543$)			Non-EP Bank ($N = 7,875$)		
RD Estimate	0.124	0.119	0.111	0.218**	0.214*	0.207*
	(0.163)	(0.166)	(0.165)	(0.110)	(0.116)	(0.112)
Closest 20 peers	ES Bank ($N = 6,925$)			Non-ES Bank ($N = 16,686$)		
RD Estimate	-0.022	-0.022	-0.020	0.203***	0.202***	0.203***
	(0.127)	(0.129)	(0.129)	(0.075)	(0.076)	(0.075)
Closest 50 peers	ES Bank ($N = 15,980$)			Non-ES Bank ($N = 42,270$)		
RD Estimate	-0.058	-0.071	-0.067	0.157***	0.164***	0.154***
	(0.071)	(0.074)	(0.074)	(0.044)	(0.046)	(0.044)

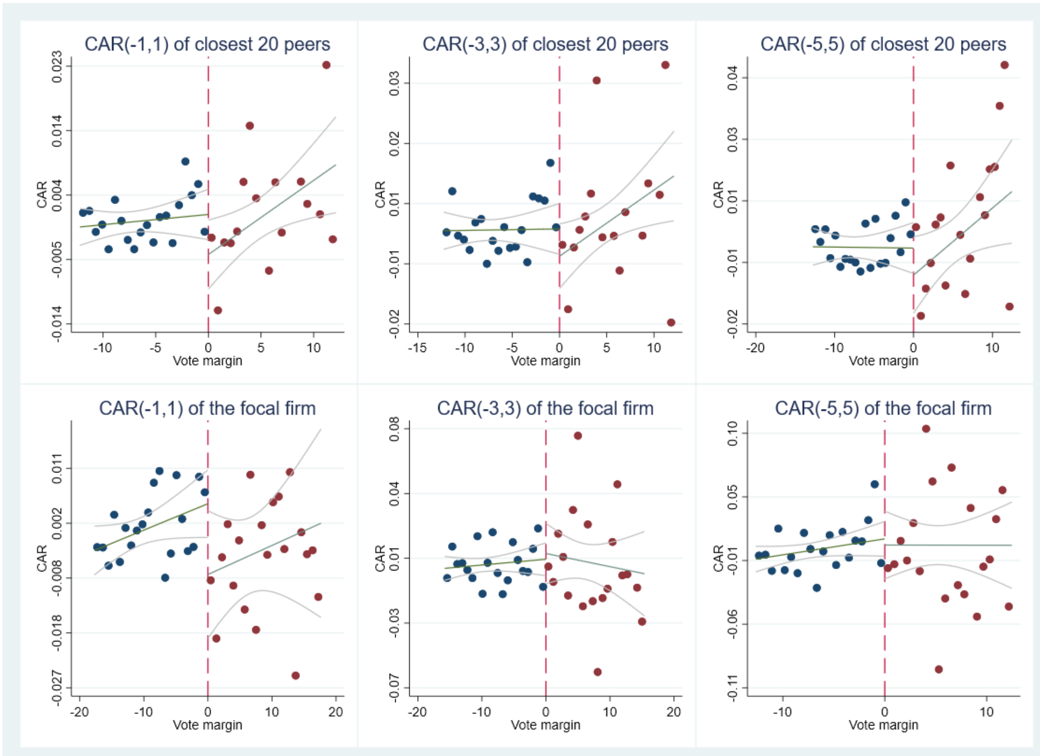
Table 11: The Spillover Channel: Non-ES Banks Reprice Low ES Profile Firms

This table is similar to Table 5, but it presents results from sub-sample analyses based on both the bank status (in panels) and peer ES profiles. Panel A focuses on loans led by ES banks, while Panel B covers loans led by non-ES banks. Within each panel, peer firms are split into two sub-samples based on whether their ES score is below or above the sample median. The column headings display the exact MSE-optimal bandwidth selectors for the RD estimators, proposed by Calonico et al. (2017). Control variables include firm size, market-to-book ratio, leverage, and ROA, with definitions provided in Appendix A.I. Standard errors are clustered at the firm level and reported in parentheses. Statistical significance is denoted by *, **, and ***, corresponding to the 10%, 5%, and 1% significance levels, respectively. Further details can be found on Page 22.

	(1) mserd	(2) msesum	(3) msecomb2	(4) mserd	(5) msesum	(6) msecomb2
Panel A: Loans from non-ES banks						
Closest 10 peers	Low ES-score peers ($N = 1,665$)			High ES-score peers($N = 1,654$)		
RD Estimate	0.355*	0.324*	0.319*	0.017	0.012	0.012
	(0.191)	(0.187)	(0.188)	(0.236)	(0.233)	(0.233)
Closest 20 peers	Low ES-score peers ($N = 3,378$)			High ES-score peers ($N = 3,368$)		
RD Estimate	0.351**	0.343**	0.344**	-0.075	-0.090	-0.074
	(0.143)	(0.134)	(0.135)	(0.134)	(0.123)	(0.126)
Closest 50 peers	Low ES-score peers ($N = 8,159$)			High ES-score peers ($N = 8,306$)		
RD Estimate	0.254**	0.253**	0.253**	-0.069	-0.067	-0.067
	(0.106)	(0.106)	(0.106)	(0.076)	(0.075)	(0.075)
Panel B: Loans from ES banks						
Closest 10 peers	Low ES-score peers ($N = 525$)			High ES-score peers($N = 547$)		
RD Estimate	0.217	0.197	0.220	0.122	0.320	0.130
	(0.323)	(0.316)	(0.321)	(0.421)	(0.591)	(0.427)
Closest 20 peers	Low ES-score peers ($N = 1,050$)			High ES-score peers ($N = 1,042$)		
RD Estimate	0.299	0.292	0.299	-0.008	0.011	0.023
	(0.213)	(0.213)	(0.214)	(0.292)	(0.224)	(0.246)
Closest 50 peers	Low ES-score peers ($N = 2,449$)			High ES-score peers ($N = 2,481$)		
RD Estimate	0.009	0.010	0.010	-0.179	-0.179	-0.179
	(0.132)	(0.132)	(0.132)	(0.122)	(0.122)	(0.122)

Appendix

Figure A.I: RD Effect on Cumulative Abnormal Returns (CAR)



This figure presents a visualization of the RD plot for cumulative abnormal returns (CAR) for focal and peer firms during a few days surrounding the proposal date. The top panel consists of three subplots showing the RD effect on the equal-weighted CARs of the closest 20 peer firms over the periods representing day -1 to 1, day -3 to 3, and day -5 to 5 around the proposal date, respectively. The bottom panel contains three corresponding subplots for the focal firms. Abnormal returns are calculated using the Fama-French five-factor model (Fama and French, 2015).

Table A.I: Variable Definitions

Variables	Definitions
Panel A: Dependent variable (source: Dealscan)	
Log(1+aisd)	Interest rate spread on a loan (over LIBOR) plus any associated fees in originating the loan.
Loan growth	The change in the total loan from t to $t+1$.
Loan maturity	The change in the average loan maturity loan from t to $t+1$.
Panel B: The RD running variable (source: SharkRepellent, Rismetrics)	
Voting outcome	Total percentage votes for each ES proposal
Panel C: Firm, bank, or proposal characteristics (source: Compustat, CRSP, Refinitiv)	
Size	Logarithm of firm's total assets.
Market value	Logarithm of common shares outstanding multiplied by the end-of-period share price.
Leverage	Long-term debt scaled by total assets.
ROA	Earnings before interest, taxes, depreciation, and amortization (EBITDA) scaled by total assets.
Operating expenses	Sum of cost of goods sold (COGS) and selling, general and administrative expenses (XSGA).
Revenue over asset	Gross income received from all divisions of the company over total assets.
Cash	Cash and short-term investments.
EBIT	Earnings before interest and taxes.
Current liabilities	Liabilities due within one year, including the current portion of long-term debt.
Market-to-book	Market value relative to book value of equity.
Age	Years since the first time the firm appears on the CRSP.
Industry concentration	The Herndahl-Hirschman index computed as a sum of the squared market shares based on net sales within the firm's 3-digit SIC industry
Cash flow volatility	The standard deviation of [operating cash flows/total assets] for firm i over the four year period from year $t-4$ to t
High (low) ES score	An indicator variable which takes the value of 1 if peer firm's ES score on Refinitiv is above (below) median value and 0 otherwise. The ES score is calculated based the yearly average environmental and social scores.
ES (non-ES) bank	ES banks are banks with positive ES scores based on Refinitiv in that year. All other banks are non-ES banks.
EP (non-EP) bank	EP banks are bank signatories to the Equator Principles in that year. All other banks are non-EP banks.

Table A.II: Close call ES proposals

Panel A: Examples of ES proposals that were marginally rejected

Example 1	
Company	Valero Energy
Meeting date	April 30th, 2009
Proposal type	Political issue
Proposal description	The shareholders of Valero Energy Corporation hereby request that the Company provide a report, updated semi-annually, disclosing Valero's: 1. Policies and procedures for political contributions and expenditures (both direct and indirect) made with corporate funds. 2. Monetary and non-monetary political contributions and expenditures not deductible under section 162 (e)(1)(B) of the Internal Revenue Code, including but not limited to contributions to or expenditures on behalf of political candidates, political parties, political committees and other political entities organized and operating under 26 USC Sec. 527 of the Internal Revenue Code.
Statement in opposition	Valero's policy does not use corporate funds to make contributions to political candidates, political parties, political committees, or other political entities organized and operating under Section 527 of the Internal Revenue Code. Instead, Valero's political activities consist primarily of its sponsorship of the Valero Energy Corporation Political Action Committee, known as VALPAC. The political contributions cited by the proponent were made by VALPAC, and did not involve Valero corporate funds. The Board believes that disclosure of dues paid by Valero to trade associations who may engage in political activity could misrepresent Valero's political activity. Valero joins trade associations principally for the business, technical, and industry expertise these organizations provide, not for political purposes. Valero carefully monitors the appropriateness and effectiveness of the political activities that the most significant trade associations to which it belongs undertake, and Valero does not agree with all positions taken by trade associations on political issues. The proposal's reporting requirements are unnecessary. In addition, the Board believes that the legally required disclosures regarding VALPAC's contributions under the current comprehensive system of reporting and accountability for political contributions provide sufficient public information, as evidenced by the proponent's citation of political contribution figures.
Voting results	47.42%
Source	SharkRepellent

Example 2	
Company	Dominion Resources
Meeting date	May 10th, 2017
Proposal type	Environmental issue
Proposal description	Shareholders request that Dominion Resources, with board oversight, publish an assessment (at reasonable cost and omitting proprietary information) of the long term impacts on the company's portfolio, of public policies and technological advances that are consistent with limiting global warming to no more than two degrees Celsius over pre-industrial levels.

Statement in opposition	The Board believes that the company, through its current integrated environmental strategy and robust corporate planning, is already taking steps to address the complex, multifaceted challenge climate change may have on its businesses and operations. This includes an ongoing assessment of potential long-term impacts of technological and regulatory changes on our business and assets, including those consistent with limiting global warming in line with the Paris Agreement. Preparing a separate report as requested by the proponents, analyzing the potential public policy and technological changes that may occur over the next 25 years relating to a global initiative, would not only take additional time and corporate resources and substantially duplicate existing efforts, but result in some speculation with little value. We do not believe that this would be in the best interests of our shareholders.
Voting results	47.84%
Source	SharkRepellent
Panel B: Examples of ES proposals that were marginally approved	
Example 3	
Company	WPX Energy
Meeting date	May 19th, 2016
Proposal type	Environmental issue
Proposal description	Shareholders request that the Board of Directors issue a report describing how the company is monitoring and managing the level of methane emissions from its operations. The requested report should include a company-wide review of the policies, practices, and metrics related to WPX Energy's methane emissions risk management strategy. The report should be prepared at reasonable cost, omitting proprietary information, and made available to shareholders by December 31, 2016.
Proponent statement	According to the Environmental Protection Agency (EPA), the oil and gas sector in the United States is the largest industrial source of methane pollution and leaks more than 7 million metric tons of methane emissions each year, enough to meet the cooking and heating needs of over 5 million American homes. Methane gas emissions are a significant contributor to climate change Shareholders request that the Board of Directors issue a report describing how the company is monitoring and managing the level of methane emissions from its operations. The requested report should include a company-wide review of the policies, practices, and metrics related to WPX Energy's methane emissions risk management strategy. The report should be prepared at reasonable cost, omitting proprietary information, and made available to shareholders by December 31, 2016.
Voting results	50.82%
Source	SharkRepellent
Example 4	
Company	Newell Brands
Meeting date	May 7th, 2019
Proposal type	Add Minorities/Women to Board (board diversity)

Proposal description	Shareholders request that the Board of Directors prepare a report (at a reasonable cost, in a reasonable time, and omitting confidential information) providing its assessment of the current state of its executive leadership team diversity and its plan to make the company's executive leadership team more diverse in terms of race, ethnicity, and gender.
Proponent statement	Currently, Newell Brands has limited racial/ethnic diversity on its Management Committee and Extended Leadership Committee. Two of the eighteen committee members are women. To address the lack of diversity in senior roles we believe the Board and senior leadership must set clear policies to attract, retain and promote women, including establishing and reporting on gender pay equity, formalizing mentor and sponsorship programs, and establishing gender-neutral family support programs. Further, we believe that linking diversity performance metrics to senior executive compensation packages can sharpen management's ability to manage human capital management risks, increase accountability and successfully reach inclusion and diversity goals. RESOLVED: Shareholders request that the Board of Directors prepare a report (at a reasonable cost, in a reasonable time, and omitting confidential information) providing its assessment of the current state of its executive leadership team diversity and its plan to make the company's executive leadership team more diverse in terms of race, ethnicity, and gender.
Voting results	56.59 %
Source	SharkRepellent

Table A.III: Robustness: RD estimation with alternative specifications

This table presents robustness tests for Table 5, using the same regression specifications, except bias-corrected estimators in Panel A and a first-order polynomial in Panel B.

	(1) msesrd	(2) msesum	(3) msecomb2	(4) 50% of mserd	(5) 150% of mserd
Panel A: Bias-corrected estimators					
Closest 10 peers					
RD Estimate	0.192**	0.194**	0.191**	0.245	0.203**
	(0.095)	(0.092)	(0.094)	(0.186)	(0.091)
Observations	11,418	11,418	11,418	11,418	11,418
Effective Obs.	3,773	4,057	4,057	1,477	6,484
Closest 20 peers					
RD Estimate	0.166**	0.169**	0.169**	0.295**	0.166**
	(0.069)	(0.069)	(0.069)	(0.121)	(0.066)
Observations	23,611	23,611	23,611	23,611	23,611
Effective Obs.	7,819	7,994	7,994	2,985	13,108
Closest 50 peers					
RD Estimate	0.126***	0.125***	0.117***	0.305***	0.113***
	(0.041)	(0.038)	(0.040)	(0.071)	(0.039)
Observations	58,250	58,250	58,250	58,250	58,250
Effective Obs.	19,044	25,428	22,964	7,209	32,114
Panel B: RD estimates with polynomial or oder 1					
Closest 10 peers					
RD Estimate	0.150**	0.141**	0.142**	0.221**	0.122**
	(0.070)	(0.066)	(0.068)	(0.108)	(0.054)
Observations	11,418	11,418	11,418	11,418	11,418
Effective Obs.	1,023	1,105	1,087	442	1,692
Closest 20 peers					
RD Estimate	0.098**	0.085**	0.096**	0.195***	0.079**
	(0.047)	(0.041)	(0.045)	(0.071)	(0.038)
Observations	23,611	23,611	23,611	23,611	23,611
Effective Obs.	2,304	3,235	3,162	1,075	3,907
Closest 50 peers					
RD Estimate	0.078**	0.059**	0.061**	0.186***	0.065**
	(0.031)	(0.024)	(0.027)	(0.044)	(0.025)
Observations	58,250	58,250	58,250	58,250	58,250
Effective Obs.	5,168	9,563	9,199	2,365	8,732

Table A.IV: Robustness: RD Using Artificial Cutoffs on Voting Outcomes

This table presents placebo tests for Table 5, using the same regression specifications but with artificial thresholds for majority voting, as indicated by the panel headings.

	Closest 10 peers			Closest 20 peers			Closest 50 peers		
	mserd	msesum	msecomb2	mserd	msesum	msecomb2	mserd	msesum	msecomb2
Panel A. Cutoff = 25%									
RD Estimate	-0.060	-0.049	-0.045	-0.020	-0.019	-0.020	-0.005	-0.001	-0.005
	(0.051)	(0.052)	(0.051)	(0.035)	(0.034)	(0.035)	(0.024)	(0.025)	(0.025)
Panel B. Cutoff = 30%									
RD Estimate	0.032	0.032	0.033	0.051	0.051	0.050	0.025	0.026	0.025
	(0.044)	(0.044)	(0.044)	(0.033)	(0.034)	(0.033)	(0.024)	(0.024)	(0.024)
Panel C. Cutoff = 60%									
RD Estimate	0.016	0.018	0.017	-0.021	-0.016	-0.015	0.022	0.034	0.034
	(0.096)	(0.096)	(0.096)	(0.064)	(0.065)	(0.064)	(0.046)	(0.046)	(0.046)
Panel D. Cutoff = 65%									
RD Estimate	-0.121	-0.129	-0.132	-0.158	-0.157	-0.159	-0.119	-0.116	-0.005
	(0.194)	(0.192)	(0.192)	(0.122)	(0.123)	(0.122)	(0.079)	(0.079)	(0.025)

Table A.V: Robustness: Excluding Meetings with Mixed Outcomes

This table presents robustness tests for Table 5, using the same regression specifications, but exclusing instances where a focal firm has both passed and vetoed ES proposals on the same meeting date.

	(1) mserd	(2) msesum	(3) msecomb2	(4) 50% of mserd	(5) 150% of mserd
Closest 10 peers					
RD Estimate	0.218**	0.218**	0.216**	0.244**	0.132**
	(0.091)	(0.090)	(0.090)	(0.111)	(0.060)
Observations	11282	11282	11282	11282	11282
Closest 20 peers					
RD Estimate	0.191***	0.191***	0.195***	0.245***	0.092**
	(0.069)	(0.066)	(0.067)	(0.084)	(0.044)
Observations	23332	23332	23332	23332	23332
Closest 50 peers					
RD Estimate	0.136***	0.139***	0.136***	0.169***	0.079***
	(0.041)	(0.042)	(0.041)	(0.051)	(0.028)
Observations	57612	57612	57612	57612	57612

Table A.VI: Robustness: Difference-in-Differences Analysis of the Spillover

This table presents the difference-in-differences (DID) analysis on the sample of passing and vetoing peers, along with their corresponding matched firms. Panel A displays the means of firm characteristics for both peers and their matched firms. Panel B presents the estimation results for DID regressions, where the dependent variable is $ln(1+\text{aisd})$. The regressions include peers of successful and failed proposals and their control firms, covering the period from two years before to two years after the meeting year. The variable "Passing (vetoing) peer" is a dummy that indicates whether the firm is a peer of a passing (vetoing) proposal, with control firms set to 0. "Post voting" is a dummy variable equal to 1 for event years between 0 and +2, and 0 otherwise. All regressions include calendar year and firm fixed effects. Standard errors are clustered at the firm level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Matching Summary Statistics

Variables	Successful peer	Control firms	Successful vs. control firms	Failed peer	Control firms	Failed vs. control firms
Log(1+aisd)	5.06	5.08	-0.03	5.09	5.11	-0.02
Total assets	22.09	22.03	0.06	21.95	21.94	0.01
Operating income	0.04	0.04	-0.01	0.03	0.04	0.00
ROA	0.12	0.13	0.00	0.12	0.12	0.00
R&D expenditures	0.01	0.01	0.00	0.01	0.01	0.00
Capital expenditures	0.05	0.05	0.00	0.05	0.05	0.00
Leverage	0.30	0.30	0.00	0.29	0.29	0.00
Cash holding	0.09	0.09	0.00	0.09	0.09	0.00
Industry concentration	0.27	0.27	0.00	0.26	0.26	0.01
Propensity score	0.16	0.16	0.00	0.21	0.21	0.00

Panel B: Difference-in-Differences Regressions

	Log(1+aisd)	Log(1+aisd)	Log(1+aisd)	Log(1+aisd)
Post voting	-0.003	0.001	-0.010*	-0.002
	(0.010)	(0.010)	(0.005)	(0.005)
Passing peers × post voting	0.028**	0.023**		
	(0.010)	(0.011)		
Vetoing peers × post voting			0.018	0.009
			(0.012)	(0.012)
Size		-0.041***		-0.048***
		(0.012)		(0.011)
ROA		-0.121**		-0.127**
		(0.057)		(0.049)
Market-to-book		-0.022***		-0.020***
		(0.005)		(0.004)
Leverage		0.058**		0.071***
		(0.022)		(0.017)
Age		-0.014		-0.020**
		(0.015)		(0.008)
Firm-fixed effect	Yes	Yes	Yes	Yes
Year-fixed effect	Yes	Yes	Yes	Yes
Observations	32,173	26,845	84,480	76,043
R^2	0.881	0.892	0.878	0.881

Table A.VII: Further Analysis: Alternative Loan Terms

This table is similar to Table 5, except the dependent variable is a peer firm's loan growth in Columns 1–3 and loan maturity in Columns 4–6, following the passage of a focal firm's ES proposal.

	(1) mserd	(2) msesum	(3) msecomb2	(4) mserd	(5) msesum	(6) msecomb2
Closest 10 peers	Loan growth ($N = 11,418$)			Loan maturity ($N = 11,397$)		
RD Estimate	0.133	0.160	0.134	0.061	0.068	0.045
	(0.397)	(0.398)	(0.391)	(0.110)	(0.119)	(0.115)
Closest 20 peers	Loan growth ($N = 23,611$)			Loan maturity($N = 23,548$)		
RD Estimate	-0.194	-0.195	-0.192	0.024	0.017	0.024
	(0.237)	(0.239)	(0.238)	(0.066)	(0.062)	(0.066)
Closest 50 peers	Loan growth ($N = 58,250$)			Loan maturity ($N = 58,112$)		
RD Estimate	-0.174	-0.173	-0.173	0.064	0.063	0.064
	(0.196)	(0.195)	(0.195)	(0.043)	(0.043)	(0.043)

Table A.VIII: Further Analysis: Spillover in Year $t+2$

This table is similar to Table 5, except the dependent variable is a peer firm's average $ln(1+\text{aisd})$ on newly issued or amended loans in Year $t+2$, using loan amount as the weight.

	(1) mserd	(2) msesum	(3) msecomb2	(4) 50% of mserd	(5) 150% of mserd
Closest 10 peers					
RD Estimate	0.248**	0.249**	0.249**	0.263	0.245***
	(0.125)	(0.124)	(0.124)	(0.173)	(0.087)
Observations	8596	8596	8596	8596	8596
Closest 20 peers					
RD Estimate	0.127	0.129*	0.130*	0.122	0.123**
	(0.083)	(0.076)	(0.078)	(0.106)	(0.054)
Observations	17865	17865	17865	17865	17865
Closest 50 peers					
RD Estimate	0.080	0.080	0.080	0.072	0.043
	(0.050)	(0.050)	(0.050)	(0.061)	(0.037)
Observations	44311	44311	44311	44311	44311

Table A.IX: Further Analysis: Loan Costs from Common Lender vs. Non-Common Lender

This table is similar to Table 5, except it presents results from sub-sample analyses based on whether the loan rates are constructed using a common lender between focal and peer firms (common lender aisd) or not (non-common lender aisd).

	(1) mserd	(2) msesum	(3) msecomb2	(4) mserd	(5) msesum	(6) msecomb2
Closest 10 peers	Common lender aisd ($N = 9,392$)			Non-common lender aisd ($N = 3,434$)		
RD Estimate	0.209***	0.208**	0.207**	0.078	0.120	0.098
	(0.090)	(0.089)	(0.089)	(0.138)	(0.127)	(0.130)
Closest 20 peers	Common lender aisd ($N = 9,392$)			Non-common lender aisd ($N = 6,999$)		
RD Estimate	0.149**	0.150**	0.151**	0.063	0.063	0.063
	(0.063)	(0.061)	(0.062)	(0.097)	(0.097)	(0.097)
Closest 50 peers	Common lender aisd ($N = 48,162$)			Non-common lender aisd ($N = 17,859$)		
RD Estimate	0.109***	0.104***	0.104***	-0.017	-0.017	-0.019
	(0.038)	(0.036)	(0.037)	(0.056)	(0.053)	(0.054)

Table A.X: Spillover Heterogeneity: Peer Firm's Environmental Score

This table is the same as Panel A of Table 8, except we split peer firms based on whether their environmental scores are below or above the sample median.

	(1) mserd	(2) msesum	(3) msecomb2	(4) mserd	(5) msesum	(6) msecomb2
Closest 10 peers	Low E Score ($N = 3,637$)			High E Score ($N = 3,836$)		
RD Estimate	0.448***	0.448***	0.447***	-0.022	-0.023	-0.021
	(0.159)	(0.159)	(0.159)	(0.121)	(0.119)	(0.119)
Closest 20 peers	Low E Score ($N = 7,371$)			High E Score ($N = 7,645$)		
RD Estimate	0.214**	0.215**	0.215**	-0.000	-0.024	-0.009
	(0.108)	(0.107)	(0.107)	(0.086)	(0.079)	(0.082)
Closest 50 peers	Low E Score ($N = 18,037$)			High E Score ($N = 18,435$)		
RD Estimate	0.141**	0.140**	0.140**	-0.032	-0.030	-0.032
	(0.070)	(0.070)	(0.070)	(0.049)	(0.050)	(0.049)