Three essays on corporate finance

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THREE ESSAYS ON CORPORATE FINANCE

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NANYANG BUSINESS SCHOOL

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A thesis submitted to the Nanyang Technological University in partial fulfilment of the requirement for the degree of Doctor of Philosophy

2019

Statement of Originality

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Summary

This thesis consists of three essays on corporate finance.

Essay one investigates whether cyber risk, which is an emerging threat to many firms, affects firms' cost of debt. We find that bank loan spreads increase by an average of 36 basis points in the three years following a breach. This economically significant increase in loan spreads is more evident when breaches result in the loss of financial information or when the stock market's response to such incidents is negative, suggesting that banks take the magnitude of the adverse impacts of breaches on firms into account when pricing. The increase in loan spreads is also more pronounced when attacked firms are informationally more opaque, when they have poorer corporate governance, and when their managers have greater risk-shifting incentives. Thus, banks also consider breached firms' information environment and managerial incentives when reassessing loan pricing. Durable lending relationships prior to breaches mitigate the adverse impact of breaches on loan rates. We further find that breached firms replace short-term bank borrowing with long-term bonds after an incident, suggesting that they attempt to avoid frequent debt rollovers.

In Essay two, I investigate the impact of corporate income taxes on executive compensation. Using staggered changes in corporate income tax rates across U.S. states, I find that the sensitivities of executives' wealth to stock return (delta) and the sensitivities of executives' wealth to return volatility (vega) increase as the state tax increases. I also find that, after the taxes increase, managers receive more equity-intensive compensation. The tax impact on delta and vega is more pronounced with the executive's myopic characteristics and better corporate governance. Finally, I show that the adverse effect of taxes on firm risk tend to be mitigated after managers experience their delta and vega increases. Taken together, shareholders factor corporate income taxes into managerial compensation contracts.

Essay three investigates whether the introduction of equity options influences the cost of bank debt. I use the listing of exchange-traded equity options and examine the effect of option listing on the cost of bank debt. I find a significant decline in loan spreads after a firm is listed in option exchange. The declining effect is more pronounced for informationally opaque firms. I also find that listed firms receive better credit rating after listing. The results suggest that options improve the informational environment and reduce monitoring costs and credit risk faced by banks, in turn, reduce the cost of debt for the borrower.

Essay one

Data breach and cost of bank debt

Data Breach and Cost of Bank Debt

Abstract

We investigate whether cyber risk, which is an emerging threat to many firms, affects firms' cost of debt. We find that bank loan spreads increase by an average of 36 basis points in the three years following a breach. This economically significant increase in loan spreads is more evident when breaches result in the loss of financial information or when the stock market's response to such incidents is negative, suggesting that banks take the magnitude of the adverse impacts of breaches on firms into account when pricing. The increase in loan spreads is also more pronounced when attacked firms are informationally more opaque, when they have poorer corporate governance, and when their managers have greater risk-shifting incentives. Thus, banks also consider breached firms' information environment and managerial incentives when reassessing loan pricing. Durable lending relationships prior to breaches mitigate the adverse impact of breaches on loan rates. We further find that breached firms replace short-term bank borrowing with long-term bonds after an incident, suggesting that they attempt to avoid frequent debt rollovers.

Keywords**:** Cyber risk, Data breach, Cost of bank loan

JEL Classification: G21, G32, G34

"Cyberattacks and data breaches in the financial sector could impose substantial costs on the U.S. economy. If investors could no longer trust that traded securities were priced efficiently, financial assets would lose their attractiveness as investment vehicles........ As a result, the cost of capital would increase, reducing economic growth."

-A report by the Council of Economic Advisers, February 2018

I. Introduction

The number of data breach incidents and the costs associated with them have significantly increased in recent years.[1] For example, the estimated annual cost associated with cyberattacks is almost $600 billion worldwide in 2017 (McAfee (2018)).[2] While recent studies have examined the negative consequences of data breach events on shareholder wealth and corporate policy,[3] we know little about whether these events affect the wealth of other major stakeholders. Data breaches increase firm risk and weaken the trust of stakeholders in affected firms because they frequently result in a loss of privacy-sensitive stakeholder information and a change in the assessment of the loss distribution by customers or other stakeholders (Kamiya et al. (2018)). Thus, firm stakeholders, who suffer from the adverse impacts of incidents, are expected to take action to protect their claims on firm value.

In this paper, using a comprehensive sample of data breaches on U.S. publicly listed firms reported in the Privacy Rights Clearinghouse (PRC) from 2005 to 2014, we examine these unexplored issues by analyzing whether banks, which provide more than half of debt financing in the U.S. (Graham, Li, and Qiu (2008)), adjust their loan rates when their borrowing firms experience data breaches.

Due to the unknown, new nature of cyber risk, it is unclear a priori whether this type of risk would affect a bank's pricing decision. There are two competing views about how banks respond to cyber risk. On the one hand, cyber risk can result in the loss of confidential information on customers and businesses,

[1] A data breach is a confirmed incident in which sensitive, confidential or otherwise protected data have been accessed and/or disclosed in an unauthorized fashion (https://searchsecurity.techtarget.com/definition/data-breach).
[2] "Economic Impact of Cybercrime─No Slowing Down," (February 2018), https://www.mcafee.com/enterprise/en-us/solutions/lp/economics-cybercrime.html
[3] For previous studies that examine the effects of data breach on shareholder wealth and firm policies, see, for example, Campbell et al. (2003), Garg, Curtis, and Halper (2003a, 2003b), Hovav and D'Arcy (2003), Cavusoglu, Mishra, and Raghunathan (2004), Hovav and D'Arcy (2004), Ko and Dornates (2006), Gatzlaff and McCullough (2010), Hilary, Segal, and Zhang, (2016), and Kamiya et al. (2018).

which could increase the uncertainty of a firm's operation and its litigation risk (Romanosky, Hoffman, and Acquisti (2014), Kamiya et al. (2018)).[4] It can also reduce a firm's sales and cash flow by undermining customer confidence and firm reputation in the product market (Lending, Minnick, and Schorno (2017), Kamiya et al. (2018)). In addition, breached firms may have to allocate more resources for coping with the aftermath of the incidents and preventing their recurrence, which imposes large costs on firms and thus reduces cash flows available to debtholders. Finally, a lack of detail in disclosure requirements[5] associated with data breaches may lead managers to withhold information on such incidents (Amir, Levi, and Livne (2018)), resulting in increased information asymmetry between insiders and outside stakeholders. This makes it difficult for debtholders to assess the actual financial status of the affected borrower and the likelihood of the future recurrence of such an incident. Thus, in responding to these unanticipated changes in firm fundamentals, lenders are expected to adjust loan rates based on their reassessment of the affected firm's future cash flow, financial distress risk, and risk-taking incentives (the "*uncertainty increasing*" view).

On the other hand, it is also likely that banks do not adjust their post-breach pricing terms for affected firms because they possess an information advantage over other stakeholders and outside investors. While it is difficult for stakeholders to accurately assess the actual costs of data breaches and the ability of affected firms' managers to remediate the risk,[6] banks, as large inside debtholders, have access to private

[4] Customers are concerned about the leakage of their information due to data breaches and take legal actions against firms. For example, Romanosky, Hoffman, and Acquisti (2014) find that the odds of a firm being sued are 3.5 times greater when a data breach incident involves individuals' financial loss.

[5] State Security Breach Notification Laws and the Securities and Exchange Commission (SEC) Cybersecurity Disclosure Guidance require publicly listed firms to notify affected individuals and report breaches to government and regulatory agencies. In particular, the 2011 SEC rules require publicly traded firms in the U.S. to disclose "materially important" cyber risk and cyber incidents. However, this disclosure requirement is criticized by practitioners and lawyers because it is too vague and provides affected firms with discretion in reporting the severity of data breach incidents. On February 21, 2018, the SEC updated the 2011 disclosure guidance by requiring firms to disclose the board's role in overseeing cyber risk management and prohibiting insiders from trading on material nonpublic information relating to cybersecurity risk and incidents.

[6] The case of Sony illustrates the difficulty in estimating the true cost of a data breach. On November 25, 2014, Sony experienced a cyberattack that resulted in substantial leakage of personal information, such as e-mails exchanged among employees, information about executive salaries, and copies of then-unreleased Sony films. The analysts' initial estimated cost of the loss caused by the incident amounted to almost $1.25 billion, while Sony's own projected remediation cost in the year following the breach was only about $35 million. This large discrepancy in the assessment

information on their borrower (Fama (1985), Diamond (1984, 1991), James (1987)) and thus have comparative advantages in collecting firm-specific private information. These information advantages may allow banks to provide breached firms with increased monitoring and enable banks to influence breached firms to take more effective risk-mitigating actions and cyber risk management policies,[7] which helps reduce the potential adverse effects of data breaches on loan spreads (the "*effective monitoring*" view).

To distinguish these two competing views, we examine the impact of a data breach on the cost of bank loans by performing a difference-in-differences test using a propensity score-matched sample. Consistent with the uncertainty increasing view, we find that breached firms experience a significant increase in loan spreads of 35.55 basis points in the three years following a breach. Given the average loan spread for our sample firms is approximately 180.28 basis points, this increase accounts for almost 19.72% of the mean spread.

We next conduct a series of subgroup analyses to better understand the circumstances under which banks increase loan spreads. First, we examine whether banks' adjustment of loan rates in responding to data breaches differs depending on the magnitude of the potential loss caused by data breaches. We measure the severity of the event according to the loss of individuals' personal financial information, the market reaction to the announcement of the incident, and industries in which affected firms operate. We expect that the adverse effect of cyber risk on loan spreads is more pronounced when data breaches involve the loss of personal financial information because such breaches can hurt customer confidence and firm reputation more and thus result in a large decrease in firm sales and a large increase in firm risk. To the extent that the market's ex-ante valuation of data breaches reflects future changes in firms' cash flow and riskiness, we also expect an increase in loan spreads to be greater when the stock market reaction to data breaches is

of the cost associated with the incident suggests that it is very difficult for stakeholders to estimate the extent of the loss associated with data breaches, especially when breaches involve a loss of personal financial information.

[7] Recent studies show that outside stakeholders such as auditors and regulators (Li, No, and Boritz (2016), Rosati, Gogolin, and Lynn (2018)) increase their scrutiny for breached firms. For example, Li, No, and Boritz (2016) find a significant positive relation between audit fees and cyber incidents, and increased audit fees are especially significant for firms that experience repeated incidents or incidents that involve the loss of intellectual property. Rosati, Gogolin, and Lynn (2018) also find that breached firms receive increased scrutiny by regulators and external auditors in the post-breach period, which results in a higher probability of receiving SEC comment letters.

negative because a negative market reaction suggests that outside investors' ex-ante perception of firms' future prospects is dim. Next, we expect that the adverse effect of breaches on loan spreads is more pronounced for firms operating in consumer-focused industries because the scale of information losses and the damage in customer trust for firms' future prospects tend to be greater in these industries than in other industries. We also expect that the loan-rate-increasing effect of data breaches is more evident for firms operating in industries with low growth opportunities because the adverse impacts of data breaches on firm sales and operation can increase the probability of distress risk in such industries. The results of our subgroup analyses are consistent with the predictions that banks take the magnitude of the adverse impacts of data breaches into account when adjusting their pricing terms for affected firms.

Second, we examine whether banks' adjustment of loan pricing differs depending on firms' abilities and incentives to mitigate potential adverse impacts of incidents and revamp their risk mitigation actions, as measured by their financial reporting quality, managerial risk-taking incentives, and corporate governance. We focus on breached firms' information environment because banks may have to devote more effort to informationally opaque firms in assessing the extent of the potential loss associated with incidents and analyzing the costs required to develop follow-up measures to avoid the recurrence of such events. These firms may also experience greater difficulties in handling the aftermath of the incidents. Thus, we expect a lager increase in loan spreads for informationally opaque firms than informationally transparent firms.

Shareholder-debtholder conflicts and managerial risk-taking incentives may also affect firms' abilities and incentives to mitigate the negative impacts of data breaches and take risk mitigation actions. To the extent that data breaches increase firm-specific operational risk (Kamiya et al. (2018)), CEOs who have strong incentives to maximize shareholder wealth may engage in more risk-taking activities to offset shareholder value loss arising from data breaches at the expense of debtholders (Jensen and Meckling (1976), Lewellen, Loderer and Rosenfeld (1985), John and John (1993), Begley and Feltham (1999), Lafond and Roychowdhury (2008)). These CEOs may also do a poor job responding to risk-increasing events. Thus, lenders who rationally anticipate such risk-shifting incentives for breached firms may demand higher interest rates to compensate for their potential losses. We measure potential shareholder-debtholder conflicts using a firm's default risk and dividend policy, and we measure managerial risk-taking incentives using the proportion of the CEO's equity-based pay component to total pay, age, and tenure. We expect that banks charge higher interest rates to breached firms when firms have higher default risk and pay more dividends and when CEOs receive higher equity-based compensation, are younger, or have a shorter tenure.

Another important factor that affects firms' abilities and incentives to mitigate the negative impacts of data breaches and take risk mitigation actions is their internal and external corporate governance. Prior studies show that good corporate governance helps reduce the agency cost of debt by discouraging managerial shirking and improving financial reporting quality (Klein (2002), Anderson, Mansi, and Reeb (2004), Cornett, Marcus, and Tehranian (2008)). Since good corporate governance also provides managers with incentives to engage in action plans and internal controls more effectively, it may also influence managers to do a better job in handling the risk management of a breach. Thus, we expect the adverse effect of cyber risk on loan costs to be more pronounced for breached firms with weak corporate governance.

Consistent with all of these predictions, we find that breached firms' information environments, shareholder-debtholder conflicts, managerial risk-taking incentives, and firm-level corporate governance are important factors when banks reassess loan rates after a data breach incident.

We further examine whether prior lending relationships have a significant effect on the loan price changes of firms that experience a data breach. Durable bank relationships enable banks to be well informed about their clients. This information advantage allows banks to provide their clients with important monitoring and other intermediary functions (Fama (1985), Diamond (1984, 1991)) and thus effectively assess the change in firms' financial distress risk and losses caused by data breaches with relatively small costs. Moreover, banks that are large debtholders, such as lead banks, may have strong incentives to maintain close lending relationships with their existing clients by providing favorable loan contract terms (e.g. Bharath et al. (2011)) even after a data breach. Thus, we expect the adverse effect of data breaches on loan spreads to be less pronounced when breached borrowers have a strong lending relationship with their lead banks prior to such incidents. Consistent with our predictions, we find that the adverse effect of data breaches on loan spreads is evident only when lead banks have relatively a short history of lending relationships with affected borrowers or when they issue a relatively small number of loans to affected borrowers prior to the incidents.

Finally, we examine how firms respond to an increase in loan spreads for their post-breach financing. We find that breached firms replace short-term bank debt with long-term public bonds, possibly to avoid frequent debt rollovers and strict monitoring by banks. Thus, data breaches have a significant effect on breached firms' cost of debt as well as the choice of debt components.

Our study contributes to several strands of the literature. First, it contributes to the emerging literature that examines how various stakeholders respond to cybersecurity risk. Recent studies show that cyber risk not only has negative consequences for shareholders (e.g., Garg, Curtis, and Halper (2003b), Cavusoglu, Mishra, and Raghunathan (2004), Gatzlaff and McCullough (2010), Kamiya et al. (2018)) but also increases board attention to risk management (Lending, Minnick, and Schorno (2017), Nordlund (2018), Kamiya et al. (2018)), auditor attention to audit fees and audit risk (Li, No, and Boritz (2016), Rosati, Gogolin, and Lynn (2018)), and regulator attention to information disclosure (Rosati, Gogolin, and Lynn (2018)). Whereas previous studies examine the consequences of cyber risk from the perspective of shareholders, board, auditor, and regulator, our study is the first to focus on how lenders, as one of important stakeholders, pay attention to cybersecurity risk and to provide new evidence on the adverse effect of cybersecurity on the borrowing costs.

Second, our study adds to the banking literature that examines firms' growth opportunities (Graham, Li, Qiu (2008), Bradley and Roberts (2015)), financial strength (Graham, Li, Qiu (2008), Bradley and Roberts (2015)), litigation risk (Deng, Willis, Xu (2014), Chu (2017)), information technology reputation (Kim, Song, and Stratopoulos (2017)), and industry competition (Valta (2012)) as important determinants of bank loan contracts. Our study extends this literature by showing that cyber risk, an emerging risk associated with firms' information technology, is an important factor when banks reassess loan rates for firms exposed to such risk.

Finally, our paper contributes to the literature on capital structure, which shows that a firm's debt choice is affected by the firm' bank relationships and growth opportunities (Houston and James (1996)), its credit quality (Denis and Mihov (2003), Rauh and Sufi (2010)), age, size (Hackbarth, Hennessy, and Leland (2007)), ownership structure (Lin et al. (2013)), and information environment (Li, Lin, and Zhan (2015)), and the market value of its collaterals (Lin (2016)). Our results show that data breaches affect borrowing firms' debt choice between bank debt and public debt, possibly due to the incentives to reduce their exposure to debt rollover risk.

The remainder of this paper proceeds as follows. In Section II, we discuss our sample and present the distribution of the sample events and the summary statistics of firm characteristics. In Section III, we analyze the effects of data breaches on loan spreads and examine the cross-sectional variations of these effects focusing on the severity of data breaches, industry characteristics, firms' information environments, asset substitution problems, and corporate governance. In Section IV, we test the relation between a firm's data breach and its choice of debt. We conclude the paper in Section V.

II. Data and Summary Statistics

We obtain data on data breach incidents from the PRC database over the period 2005 to 2014. We follow Kamiya et al. (2018) to construct our sample. Specifically, we first identify all data breach events in private and public firms from the PRC. Next, we manually match organization names reported in the PRC database with firm names listed in Compustat and the Center for Research in Securities Prices (CRSP). When attacked firms are unlisted subsidiaries of listed firms, we consider cyberattacks to have occurred in their listed parent firms. If we cannot match organization names recorded in the PRC database with firm names in Compustat and CRSP, we search Capital IQ corporate profiles and other sources, including company websites and *Factiva*, to ensure the accuracy of their names for proper matching. We restrict the sample to affected firms with financial and stock return data available in Compustat and CRSP, respectively. We exclude firms in the financial industry (Standard Industrial Classification (SIC) Codes 6000 – 6999). We require that firms have a share code of 10 or 11, which screens out real estate investment trusts, mutual funds, or closed-end funds. We next merge the list of firms identified by this procedure with the list of firms covered in the Loan Pricing Corporation's DealScan database. We match firms in DealScan and those in Compustat using a DealScan-Compustat link file created by Chava and Roberts (2008). Following procedures similar to those of Graham, Li, and Qiu (2008), who examine the effects of financial restatements on bank loan contracting, we keep only the first breach event if a firm experiences multiple data breaches during our sample period to avoid the potential overlap between the pre- and post-breach periods of these multiple data breaches, which could confound our results. We require that breached firms have at least one loan issued in the three years before and three years after the data breach. These procedures result in a final sample of 118 data breaches.

Table I presents the distribution of the 118 data breaches in our sample firms by industry and year. We find that data breaches are concentrated in service industries (31.36%), wholesale trade and retail trade industries (26.27%), and manufacturing industries (25.42%). Thus, firms in which customers' personal information is important in doing businesses are more likely to experience a data breach. The numbers of data breaches used in our analysis is highest in 2006 (20), followed by 2007 (19) and 2012 (18), and lowest in 2008 and 2009, possibly due to a decrease in bank loan financing during the financial crisis.

Panel A of Table II describes the summary statistics for our sample of 118 nonfinancial breached firms and other nonfinancial firms that do not experience a data breach in a given year (6,595 firm-year observations) covered in Compustat, CRSP, and DealScan over the period 2005 to 2014. We winsorize all continuous variables at the 1st and 99th percentiles. We find that compared to non-breached firms, breached firms are larger, older, more profitable (i.e., higher ROA), and less risky (i.e., lower cash flow volatility and stock return volatility). They also have higher growth opportunities (i.e., higher Tobin's *q*), lower asset tangibility, a higher frequency of having a credit rating, and a lower frequency of having an institutional blockholder. Using firms covered in the BoardEx database, we find that the board size for breached firms is larger than that for non-breached firms and the proportion of outside directors is not significantly different between the two groups. Using data from the ExecuComp database, we also compare CEO characteristics between breached and non-breached firms and find that CEO-chair duality, CEO age, CEO tenure, and the proportion of equity-based CEO pay are not significantly different between them. These results are similar to those of Kamiya et al. (2018), who show that cyberattacks are more likely to occur at more visible firms such as larger firms, firms included on the *Fortune* 500 list, and more highly valued firms.[8] In Panel B, which presents loan characteristics of breached firms (982 firm-loan observations) and non-breached firms (8,627 firm-loan observations), we find that loan spreads for breached firms are lower than those for non-breached firms. We also find that loans issued to breached firms are larger, and have a shorter maturity and a greater number of lenders compared to loans issued to non-breached firms. Detailed definitions of these variables are provided in the appendix.

III. Impacts of Data Breaches on Loan Spreads

A. Difference-in-differences test

To test the two competing views on the effect of cyber risk on loan spreads, we perform a difference-in-differences test using loans issued to the propensity score-matched firms during the three years before and three years after the breach. For each treatment firm that experiences a data breach, we match a control firm that does not experience a data breach using propensity score matching, in which we estimate the logit regression of *Data breach* (an indicator that takes the value of one if a firm experiences a data breach and zero otherwise) on log (market value of equity), leverage, Tobin's q, ROA, stock return, tangibility, cash flow volatility, and institutional blockholder (indicator). We require treatment and matching firms to be in

[8] In untabulated tests, we examine which firms are more likely to experience data breaches. Using a sample of all 6,713 firm-year observations covered in Compustat, CRSP, and DealScan from 2005 to 2014, we estimate a probit regression in which the dependent variable is an indicator that equals one if a firm experiences a data breach in a given year and zero otherwise. A priori, it is unclear which firms are more likely to be breached. On the one hand, a data breach is more likely to occur in visible firms (e.g., large firms), well-performing firms, or safe firms since these firms tend to have more valuable information to be breached. On the other hand, to the extent that these firms are aware of their substantial losses when they are breached and thus invest more in data security and risk management, it is possible that the likelihood of data breaches is higher for firms whose defenses are easier to breach, such as small firms and risky firms. We find that firms are more likely to become targets of data breaches when they are larger. Firms are also more likely to experience data breaches when they have a higher leverage ratio, lower future growth opportunities, lower asset tangibility, and no credit rating. With respect to corporate governance, we find that the existence of an institutional blockholder and board size are, respectively, negatively and positively associated with the likelihood of experiencing a data breach. These results are consistent with previous studies showing that corporate governance is associated with the occurrence of a data breach (Lending, Minnick, and Schorno (2017)).

the same industry (the same two-digit SIC codes) and in the same fiscal year. We also require that treatment and matching firms have at least one loan issued during both the three years before and three years after the data breach.[9] We then match a treatment firm with the control firm that has the closest propensity score without replacement.

Using these treatment and control firms, we then estimate the following ordinary least squares (OLS) regression model:

$$Log(AISD_{it}) = \alpha + \beta_1 Post \times Data\ breach + \beta_2 X_{t-1} + \gamma_t + \omega_i + \varphi_p + \varepsilon_{it} \qquad (1)$$

where $AISD_{it}$ is all-in-spread-drawn, which is a rate a borrower pays in basis points over LIBOR or the LIBOR equivalent; *Post* is an indicator that takes the value of one for the post-breach period (year *t*, year *t*+1, and year *t*+2) and zero for the pre-breach period (year *t*-1, year *t*-2, and year *t*-3), where year *t* is a year after the data breach announcement date; *Data breach* is an indicator that takes the value of one if a firm experiences a data breach and zero otherwise; X_{t-1} is firm characteristics used in propensity score matching that are measured one year prior to the loan start date and loan characteristics that may affect the cost of debt, including loan size, maturity, term loan dummy, and the number of lenders; and φ_p, γ_t, and ω_i are loan purpose fixed effects, year fixed effects, and firm fixed effects, respectively. Standard errors are adjusted for heteroskedasticity and clustered by firm to address the potential issue that high correlation among loans borrowed by the same firm could lead to biased standard errors.

B. Impacts of data breaches on loan spreads

[9] This requirement could create a sample selection issue if firms that experience a data breach would not want to borrow from the bank because of a large increase in loan spreads or the bank is not willing to issue loans because of the firm's weak fundamentals in the post-period. Although these potential explanations would bias us against finding results, we check the distribution of our sample firms before we construct a propensity score matched sample. We classify both treatment and matching firms into four subgroups according to the loan issuance and delisting status: 1) firms with loans issued by their lead bank of the pre-breach period (25 (2,000) treatment firms (control firms)), 2) firms with loans issued by a new lead bank (98 (4,595) treatment firms (control firms)), 3) delisted firms (3 treatment firms (268 control firms)), 4) firms that are listed but do not issue any loans in the post-breach period (53 treatment firms (2,334 control firms)). We conduct the Kolmogorov-Smirnov test to see whether the distribution of each sub-group between treatment and control firms is equal and find no evidence that the distribution of any subgroup firms is concentrated on either treatment or control firms.

Panel A of Table III presents descriptive statistics for a sample of 212 propensity score-matched firms (106 treatment firms and 106 control firms). None of the firm-level characteristics show significant differences between the two groups, indicating that our propensity score matching approach identifies control firms that are highly similar to treatment firms.

Panel B of Table III shows the results from the OLS regression in which the dependent variable is log (*AISD*). In column (1), we include only the interaction term between *Post* and *Data breach*, firm fixed effects, year fixed effects, and loan purpose fixed effects and find that the coefficient on the interaction term between *Post* and *Data* is positive and significant at the 5% level. This result supports the uncertainty increasing view that banks charge higher interest rates for firms that experience a data breach to protect their claim on firm value.

In column (2), we find that the magnitude of the coefficient on the interaction term becomes slightly greater when firm and loan characteristics are added to the regression. Its coefficient estimate of 0.18 suggests that banks increase the loan spread for breached firms by 19.72% ($e^{0.180} - 1$). Given that the average loan spread for the propensity score-matched sample is 180.29 basis points, this number indicates an increase in the average loan spread by 35.55 basis points after the data breach. With the sample average loan size of $976 million, an increase in loan spreads by 35.55 basis points corresponds to additional interest payments of $3.47 million (=$976 million × 0.003555).

In column (3), we break down *Post (indicator)* into three subperiod indicators, $Year_t$, $Year_{t+1}$, and $Year_{t+2}$, and interact each subperiod indicator with *Data breach*. We find that the coefficient on the interaction term involving $Year_t$ is positive and insignificant and that the coefficient on the interaction term involving $Year_{t+1}$ is positive and significant at the 5% level. The magnitude of the coefficient on the interaction term involving $Year_{t+2}$ is reduced significantly and it loses its significance. These findings suggest that the adverse effect of data breaches on the cost of bank loans is particularly pronounced two years after the data breach.

Overall, these results support the uncertainty increasing view that data breaches intensify lenders' concerns about breached firms' future prospects and their ability to mitigate such incidents; thus, lenders increase their monitoring costs, which then translate into higher loan rates for these firms.

C. Loan spread changes and severity of data breaches

In this subsection, we investigate whether the adverse effect of data breaches on loan spreads varies depending on the severity of the incidents and borrowers' industry characteristics. To measure the severity of the incidents, we first classify incidents into data breaches involving the loss of personal financial information (e.g., customers' bank accounts and credit card information) and those without such a loss. To the extent that this privacy-sensitive information is used for other cybercrimes (e.g., hacking, skimming, and phishing schemes), the expected loss from the potential misuse of such information can be a serious threat to a firm and its stakeholders. We also classify data breaches according to the market reaction around the breach announcement date, as prior studies suggest that the market reaction is more negative when remediation costs borne by shareholders are greater (e.g., Campbell et al. (2003), Kamiya et al. (2018)).

The results are reported in Panel A of Table IV. We find that only the coefficients on the interaction terms involving indicators for data breaches resulting in more severe loss (i.e., data breaches with financial information loss and those with negative CAR (-1,+1)) are significant. However, the difference between the coefficients on the interaction terms involving indicators for more severe incidents and those involving indicators for less severe incidents is not statistically significant.

As a further test, we examine whether banks' adjustment of loan rates for breached firms differs depending on the industry to which an attacked firm belongs. The results are reported in Panel B of Table IV. In columns (1) and (2), we divide the sample firms into firms operating in consumer-focused industries and those operating in other industries (Lev, Petrovits, and Radhakrishnan (2010)). We expect that firms operating in consumer-focused industries experience a larger increase in loan spreads after a breach because the potential loss arising from their tarnished reputation and the costs of restoring consumer confidence borne by shareholders are greater in these industries than in other industries. Consistent with this prediction, we find a significant increase in loan spreads for firms operating in consumer-focused industries and no significant increase in loan spreads for firms operating in other industries. In columns (3) and (4), we divide the sample firms according to industry growth opportunity (Tobin's *q*). We expect the loan-spread increasing effect of data breaches to be more pronounced for firms operating in industries with lower growth opportunities because low growth in firms' assets and sales in these industries may further increase their distress risk caused by data breaches. We calculate the sample median Tobin's *q* in the same two-digit SIC code industries and divide the firms according this median. As shown in columns (3) and (4), the increase in loan spreads is larger and significant when firms operate in industries with lower growth opportunities.

Overall, the results in this subsection show that banks consider the severity of expected losses associated with data breaches and borrowers' industry characteristics when they reassess a loan pricing term for breached firms: the treatment effect is more pronounced for firms whose expected losses from breaches are larger and for firms operating in consumer-focused industries or industries with lower future growth opportunities.

D. Loan spread changes and information environment

In this subsection, we investigate whether a bank's decision to change loan spreads for breached firms is affected by their informational environment. Prior studies show that the information environment is an important determinant of lenders' loan pricing decisions (Sengupta (1998), Bharath et al. (2011), Lin et al. (2011)). For example, Sengupta (1998) hypothesizes that a timely and detailed disclosure reduces lenders' perception of default risk and find that firms with high disclosure ratings from financial analysts enjoy a lower cost of issuing debt. Since firms' opaque information environment makes it more difficult for lenders to investigate and estimate the impact of a data breach on firm operation and sales, we expect the loan-spread-increasing effect of data breaches in the post-breach period to be more pronounced when borrowers face less transparent information environments.

Specifically, we use analysts' earnings forecast dispersion (Imhoff and Lobo (1992), Barron et al. (1998)), analyst following (Lang and Lundholm (1996), Walther (1997), Balakrishnan et al. (2014)), R&D

intensity (Aboody and Lev (2000)), and bid-ask spreads as the measures of firms' information transparency. Previous studies use the dispersion of analyst earnings forecasts as a proxy for the information environment (e.g., Imhoff and Lobo (1992), Lang and Lundholm (1996), Barron et al. (1998)) because higher dispersion implies analysts' uncertainty about a firm's performance. Thus, we expect that banks increase loan spreads more for firms with higher forecast dispersion than for those with lower forecast dispersion. Previous studies also show that low analyst coverage increases information asymmetry (Lang and Lundholm (1996), Walther (1997)) because analyst following is positively associated with the informativeness of a firm's disclosure policy. Thus, we expect that our results are more prominent in subgroups with a low analyst following. Aboody and Lev (2000) show that insiders take advantage of information on planned changes in R&D budgets and conclude that R&D expenses are a major contributor to information asymmetry and insiders' gains. This finding suggests that banks require higher loan rates for breached firms with higher R&D intensity. Finally, bid-ask spread can be an important measure of a firm's information asymmetry. Since market makers face uncertainties when they trade against informed investors, in response, they protect themselves by asking for a high spread. Thus, a higher bid-ask spread suggests greater informational opacity of a firm. This argument indicates that our results should be more pronounced for firms with higher bid-ask spreads.

Table V presents the results. Consistent with our predictions, the coefficients on the interaction term between *Post* and *Data breach* are positive and significant in subgroups of firms with higher analyst forecast dispersion, lower analyst coverage, higher R&D intensity, and higher bid-ask spreads. The impacts of information opacity are also economically large and significant. For example, the coefficient on the interaction term between *Post* and *Data breach* in the subgroup of firms with higher analyst forecast dispersion is 0.204, which corresponds to a 22.63% increase in loan spreads. Overall, these results suggest that the adverse effect of a data breach on the cost of bank debt is affected by the breached firm's information environment.

E. Loan spread changes and asset substitution problems

Managers who work for the interests of shareholders have strong incentives to invest in risky projects that benefit shareholders at the expense of debtholders wealth (Jensen and Meckling (1976)). Debtholders who rationally anticipate these managerial risk-shifting (i.e., asset substitution) incentives internalize the agency costs associated with such incentives into their loan contracts by charging higher interest rates. In this subsection, we examine whether breached firms' asset substitution problems affect a bank's reassessment of their loan rates. We use a firm's default risk, dividend policy, and CEO compensation structure to measure the extent of its asset substitution problems.

First, we predict that banks increase loan rates more for firms with high default risk because data breaches amplify a firm's uncertainty risk, and thus, shareholders' incentives to exploit debtholders are likely to increase. To test this prediction, we use cash flow volatility, leverage, and a credit rating score as measures of a firm's default risk. Following Lin et al. (2011), we convert letter ratings into numbers ranging between 1 (highest rating) and 7 (no rating). We then divide our sample firms into two subgroups according to the sample median default risk and reestimate the regressions separately for each subgroup. Panel A of Table VI presents the results. Consistent with our prediction, we find that the coefficients on the interaction term between *Post* and *Data breach* are positive and significant only in subgroups of firms with higher default risk (i.e., firms with higher cash flow volatility, higher leverage, and a lower credit rating score).

Second, we examine whether the effect of a data breach on loan spreads differs across firms' dividend policies. Previous studies show that lenders view excessive payments of dividends as a potential source of wealth transfer to shareholders (Maxwell and Stephens (2003), Chava, Livdan, and Purnanandam (2009)). To the extent that breached firms experience financing deficit after data breaches (Kamiya et al. (2018)) and firms' dividend policies are sticky and cannot be easily reversed, high dividend policies of breached firms could result in wealth transfer from debtholders to shareholders. Therefore, we expect that a post-increase in loan rates for breached firms is higher when they pay higher dividends. To investigate this issue, we first divide our sample firms into dividend payers and nonpayers. The results are reported in Panel B of Table VI. The coefficient on the interaction term between *Post* and *Data breach* is positive and significant at the 10% level in a subgroup of dividend payers (column (1)), while it is insignificant in a subgroup of nonpayers (column (2)).

Next, following Chava, Livdan, and Purnanandam (2009), we use state payout statutes and divide our sample firms into firms incorporated in nimble dividend statue states where managers have more flexibility in paying dividends and those incorporated in other states. If managers have greater flexibility in paying dividends, creditors will be exposed to higher expropriation risk. Thus, we expect conflicts of interest between shareholders and debtholders to be greater for borrowers that are incorporated in nimble dividend states, and we expect the adverse effect of data breaches on loan spreads to be more evident for such borrowers. Columns (3) and (4) show results that are consistent with this prediction: the coefficient on the interaction term between *Post* and *Data breach* is positive and significant at the 5% level in subgroups of firms incorporated in nimble dividend states, while the corresponding coefficient is insignificant in subgroups of firms incorporated in other states.

In sum, the results in Panel B of Table VI suggest that banks charge higher loan spreads for breached borrowers for which they have a concern about excessive payout to shareholders.

Finally, we use CEO compensation structure and CEO characteristics (i.e., age and tenure) as measures of managerial risk-taking incentives. Equity-linked compensation helps align the interests of managers and shareholders. Supporting this view, the compensation literature documents a positive relation between equity-based compensation and corporate risk-taking (e.g. DeFusco, Johnson, and Zorn (1990), Guay (1999), Coles, Daniel, and Naveen (2006), Chava and Purnanandam (2010)). Thus, we expect that banks charge higher loan rates for breached firms that pay CEOs a higher equity-linked compensation because of their concern that a high proportion of CEO equity-linked compensation to CEO total pay induces CEOs to take more risk. Columns (1) and (2) of Panel C Table VI present results for the subgroup analysis using the ratio of CEO equity-linked pay to CEO total pay. Consistent with our prediction, we find that the increase in loan spreads is concentrated only among firms with a higher proportion of equity-based compensation to total pay. The effect is also economically large: after data breaches, firms with an above-sample median proportion of CEO equity-linked pay to CEO total pay experience an increase in loan spreads of 24.37%

$(e^{0.221} - 1)$ compared to other firms that do not experience a data breach (column (1)). This is equivalent to an increase in loan rates of 38 basis points after a data breach relative to the subsample average loan rate of 157 basis points.

CEO age and tenure can also affect risk-taking preferences and incentives. Previous studies show that younger CEOs invest more aggressively and take greater risks to signal their superior ability (Prendergast and Stole (1996)). Serfling (2014) provides empirical evidence that CEOs' risk-taking behavior decreases as they become older. Therefore, banks are likely to charge higher loan spreads for breached borrowers led by young CEOs than for breached borrowers led by old CEOs. Similarly, we predict that the loan-increasing effect of data breaches is more pronounced for firms with short-tenured CEOs because CEOs have strong incentives to show off their talent by pursuing riskier corporate policies that help increase shareholder wealth early in their term in office. Moreover, since CEOs who have held the position longer tend to be more myopic, long-tenured CEOs have greater incentives to forgo risky but positive NPV projects when firms experience unexpected cyber incidents. Consistent with these predictions, we find that breached firms with younger (shorter-tenured) CEOs experience a significant increase in loan spreads at the 1% level in the three-year post-breach period, while those with older (longer-tenured) CEOs experience an insignificant decrease in loan spreads during the same period.

In sum, the results in Table VI suggests that reflecting banks' concerns about wealth transfer from debtholders to shareholders after data breaches, the adverse impact of data breaches on loan pricing is more pronounced when breached firms' risk-shifting incentives are higher.

F. Loan spread changes and corporate governance

In this subsection, we examine how breached firms' internal and external governance affects banks' adjustment of loan rates for these firms. We first use the proportion of outside directors on the board as the measure of the quality of a firm's internal governance. Kamiya et al. (2018) show that victims of a cyberattack are more likely to increase board oversight of firm risk, suggesting that that the board reassesses the risks the firm is exposed to after an attack and the costs of these risks. To the extent that this increase in board oversight of firm risk after a breach is more likely to occur in well-governed firms and these firms also do a better job in responding to the aftermath of data breaches, we expect that banks' concern about their potential losses associated with data breaches is greater for poorly governed firms, and thus, banks charge higher loan rates for such firms.

We use the proportion of outside directors on the board and CEO-chair duality to measure the quality of firms' internal governance. Weisbach (1988) finds that CEO turnover is more sensitive to firm performance when independent directors represent a majority of the board, suggesting that independent directors perform an important monitoring role. Previous literature also shows that CEO-chair duality is associated with higher CEO compensation (Core, Holthausen, and Larcker (1999)) and lower sensitivity of CEO turnover to firm performance (Goyal and Park (2002)), suggesting that firms with CEO-chair duality are poorly governed.

Columns (1) and (2) (columns (3) and (4)) of Table VII report the results for subsamples of firms classified according to the sample median proportion of outside directors on the board (CEO-chair duality). We find that the coefficients on the interaction term between *Post* and *Data breach* are positive and significant for firms with a lower proportion of outside directors on the board and those with CEO-chair duality, while the corresponding coefficients are insignificant for firms with a higher proportion of outside directors on the board and those without CEO-chair duality. These results are consistent with our expectation that banks' concern about the aftermath of their borrowers' data breaches is more severe when firms are poorly governed.

Next, we focus on external corporate governance to examine the effect of data breaches on borrowers' loan rates. Specifically, we investigate whether such an effect varies across the level of product market competition in the industry in which firms operate. Since previous studies show that product market competition has a disciplinary effect on managerial behavior (Hart (1983), Shleifer and Vishny (1997)), we expect that firms facing intense product market competition act more promptly to contain the negative consequences of an incident on their operation and market position. Thus, we predict that the increase in loan spreads following data breaches is more pronounced for firms operating in less competitive industries.

We use the Herfindahl index, calculated as the sum of squared market shares of firms' sales in the two-digit SIC industries in a given year, as the measure of product market competition, and we split our sample firms into two subgroups according to the sample median Herfindahl index. The results are reported in columns (5) and (6) of Table VII. Supporting our expectation, the coefficient on the interaction term between *Post* and *Data breach* is positive and significant in the high Herfindahl index group (i.e., low product market competition), but it is insignificant in the low Herfindahl index group (i.e., high product market competition).

G. Loan spread changes and lending relationship

In this subsection, we investigate whether a change in loan rates for data breached firms is influenced by their prior lending relationships with lead banks. There are at least two reasons why lending relationships affect banks' loan pricing for breached firms. First, lead lenders who have prior long-term lending relationships with borrowers may have implicit long-term incentives to maintain these relationships by providing necessary assistance when their borrowers experience financing difficulties. Second, durable lending relationships lower the information asymmetry between lenders and borrowers and thus decrease loan spreads and covenant tightness, particularly for borrowers that are less transparent (Bharath et al. (2011), Prilmeier (2017)). Since durable lending relationships allow banks to provide their clients with important monitoring and other intermediary functions (Fama (1985), Diamond (1984, 1991)) and thus assess the change in firm fundamentals caused by data breaches with relatively small costs, we expect breached firms with such relationships to experience no or a smaller increase in loan spreads after breaches.

We construct two measures of durable lending relationships for each loan facility, focusing on prior lending relationships with lead banks.[10] The first measure is the number of months from the issuance of a loan arranged by a lead bank to the month immediately prior to the breach announcement date (*Lending*

[10] We define lead banks as lenders that belong to one of the following three cases: 1) lenders for which the field "lead arranger credit" is marked "Yes" in DealScan, 2) lenders of all sole lender loans, or 3) lenders for which the field "lender roles" are "agent," "administrative agent," "arranger," or "lead bank.". If a loan contains multiple lead banks, we choose as the lead bank a bank that has issued more loans in past five years.

duration). The second measure is the ratio of the number of loans arranged by a lead bank to a borrower in the five years prior to the data breach to the total number of loans that the firm borrowed in the five years prior to the data breach (*Number of lead bank loans*). We then divide our sample into two subgroups according to each of the sample median durable lending relationship measures and reestimate the regressions separately for each subgroup.

Table VIII presents the results. We find that the coefficient on the interaction term between *Post* and *Data breach* is positive and significant only for a subgroup of firms with a shorter lending relationship (column (1)) and for a subgroup of firms with a smaller number of loans arranged by lead banks in the five years prior to a data breach (column (2)). These results suggest that prior durable lending relationships mitigate the adverse impact of a data breach on loan rates and support our predictions.[11]

IV. Impact of Data Breaches on Firms' Debt Structure

Our results thus far show that banks consider an increase in firm uncertainty caused by cyber risk when reassessing loan prices for their breached borrowers. This raises an important question about whether data breaches also influence affected firms' choice of debt structure in responding to the lender's decision to raise the loan rate. To address this issue, we examine how breached firms adjust their borrowing from private lenders (i.e., bank borrowing) and public lenders (i.e., bond borrowing). On the one hand, we expect

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[11] In untabulated tests, we examine whether data breaches affect the covenants in bank debt contracts. Specifically, using the propensity score-matched sample above, we estimate the regressions in which the dependent variables are the number of financial covenants, the number of general covenants, and the sum of financial covenants and general covenants. General covenants contain following five covenants indicators: loan security, dividend restriction, asset sale sweep, debt sale sweep, and equity sale sweep. We control for the firm and loan characteristics used in Panel B of Table IV. We find little evidence that data breaches affect loan covenant contract terms. The coefficients on the interaction term between *Post* and *Data breach* are positive but insignificant across all specifications. When we investigate the effect by year, we find that only the number of general covenants increases significantly in *year t+1* at the 10% level. Since previous studies show that loan covenants play an important role in mitigating incentives conflicts between mangers (shareholders) and debtholders (Jensen and Meckling (1976), Myers (1977), Smith and Warner (1979), Bradley and Roberts (2015)), as a further test, we divide the sample firms according to the sample median of various firm characteristics that measure such incentive conflicts (i.e. firm size, leverage, cash flow volatility, the proportion of tangible assets to total assets, analyst forecast dispersion, analyst coverage, and Tobin's *q*) and reestimate the regressions separately for these subgroups. We do not find any evidence that firms with greater incentive conflict problems experience an increase in the number of loan covenants: the effect of data breaches on the number of loan covenants is insignificant for firms with a smaller size, higher leverage, more volatile cash flows, higher tangible assets, higher information opacity, and higher growth opportunities.

breached firms to switch from bank debt to public debt financing as their effort to avoid rising costs of bank debt and strict monitoring. On the other hand, breached firms may borrow more from banks to send a signal to the market that they still maintain relationships with banks with superior monitoring ability (Fama (1985), Diamond (1991)). Therefore, it is an empirical question whether a breached firm increases public debt over bank debt.

We obtain data on debt structure for our propensity score-matched sample firms from S&P Capital IQ (Lin et al. (2013)). Our final sample consists of 987 firm-year observations for the 2005 to 2014 period. Table IX presents the results from OLS regressions in which the dependent variables are the ratio of bank debt (the sum of revolving credit and term loans) to total debt (columns (1) and (2)) and the ratio of public debt (the sum of bonds, notes, and commercial paper) to total debt (columns (3) and (4)). In column (1), we find that the coefficient on the interaction term between *Post* and *Data breach* is negative and significant at the 5% level, suggesting that the proportion of bank loans to total debt decreases after a firm experiences a data breach. Given that the average ratio of bank debt to total debt for our sample firms is 0.305, the coefficient estimate of -0.077 suggests that breached firms on average experience a decline in their bank loans of 25.2% (0.077 / 0.305) in the three years after a breach. In column (2), we find the decrease in affected firms' bank loans persists across all three years after the breach.

Turning to the use of public debt, in column (3), we find that the coefficient on the interaction term between *Post* and *Data breach* is positive and significant at the 10% level. The coefficient estimate of 0.062 indicates that the ratio of public debt to total debt for breached firms increases by 9.8% (0.062/0.632) after a breach. In column (4), we find that breached firms increase public debt in $Year_t$ and $Year_{t+1}$.

Overall, these results suggest that affected firms significantly increase their reliance on public debt after a breach but significantly decrease their reliance on bank loans during the same period, suggesting that breached firms adjust their debt structure by replacing bank debt with public debt.

V. Conclusion

In this paper, we investigate whether a data breach affects a firm's borrowing cost. We develop two competing views about how banks respond to cyber risk. On the one hand, in responding to unanticipated changes in firm fundamentals, lenders may increase their loan rates after data breaches because they need to reassess the affected firm's uncertain future prospects, and it is difficult for outside stakeholders to assess the actual costs of data breaches (the "uncertainty increasing view"). On the other hand, banks have better access to private information about their breached borrowers than other investors. This information advantage allows banks to provide effective monitoring and influence firms to take risk mitigating actions, which would offset any adverse effects of cyber risk on loan spreads (the "effective monitoring view").

Consistent with the uncertainty increasing view, we find that data breaches significantly increase the cost of bank debt. This adverse effect of data breaches on the cost of bank debt is particularly pronounced when the magnitude of the potential loss associated with data breaches is greater (i.e., data breaches involving the loss of customers' financial information and those in which the market reaction to the announcement of data breaches is negative). We also find that the adverse effect of data breaches on the cost of bank debt is concentrated on firms operating in industries in which the expected loss caused by a data breach is greater (i.e., consumer-focused industries and industries with lower growth opportunities).

In further analyses, we find that firms with higher informational opacity, firms with higher risk-shifting incentives, firms with poorer governance, and firms without prior durable lending relationships experience a larger increase in loan rates subsequent to a data breach. These subgroup analysis results suggest that banks not only consider the extent of the loss caused by an incident but also consider firms' abilities and incentives to mitigate the adverse impacts of incidents by taking risk mitigation actions.

Finally, we find that breached firms reduce their reliance on bank debt and increase their reliance on public debt in the post-breach period, possibly to avoid frequent debt rollovers, rising costs of bank debt, or more intense bank monitoring following an incident.

Overall, our study provides strong evidence that lenders factor cyber risk, a newly emerging operational risk, into their loan pricing decision and that this decision is affected by several industry-, firm-, and breach-specific characteristics. Our study highlights the importance of cyber risk as a new factor that determines the cost of capital, and it provides evidence on a new channel through which cyber risk adversely affects firm value.

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Table I
Distribution of Data Breaches by Year and Industry

The table presents the chronological distribution of 118 data breaches against 118 distinct firms covered in Compustat, CRSP, and DealScan over the period 2005 to 2014 by calendar year and industry (Standard Industrial Classification (SIC) two-digit codes). We exclude firms that operate in the financial industry (SIC codes between 6000 and 6999). The number in parentheses is the percentage of data breaches that occurred in a given industry each year, and the number in brackets is the percentage of data breaches that occurred in each industry during the sample period.

Calendar year	Agriculture, forestry, fisheries (01-09)	Mineral, construction (10-17)	Manufacturing (20-39)	Transport, communications (40-48)	Electric, gas, and sanitary services (49)	Wholesale trade and retail trade (50-59)	Service industries (70-89)	Public administration (90-99)	Total
2005	0	0	6	2	0	5	2	0	15
	(0.00)	(0.00)	(20.00)	(18.18)	(0.00)	(16.13)	(5.41)	(0.00)	(12.71)
2006	0	0	4	4	1	6	4	1	20
	(0.00)	(0.00)	(13.33)	(36.36)	(16.67)	(19.35)	(10.81)	(100.00)	(16.95)
2007	0	1	7	2	0	3	6	0	19
	(0.00)	(50.00)	(23.33)	(18.18)	(0.00)	(9.68)	(16.22)	(0.00)	(16.10)
2008	0	0	1	0	0	1	2	0	4
	(0.00)	(0.00)	(3.33)	(0.00)	(0.00)	(3.23)	(5.41)	(0.00)	(3.39)
2009	0	1	0	1	1	0	2	0	5
	(0.00)	(50.00)	(0.00)	(9.09)	(16.67)	(0.00)	(5.41)	(0.00)	(4.24)
2010	0	0	3	0	0	3	2	0	8
	(0.00)	(0.00)	(10.00)	(0.00)	(0.00)	(9.68)	(5.41)	(0.00)	(6.78)
2011	0	0	2	1	2	3	5	0	13
	(0.00)	(0.00)	(6.67)	(9.09)	(33.33)	(9.68)	(13.51)	(0.00)	(11.02)
2012	0	0	3	1	1	6	7	0	18
	(0.00)	(0.00)	(10.00)	(9.09)	(16.67)	(19.35)	(18.92)	(0.00)	(15.25)
2013	0	0	3	0	1	3	5	0	12
	(0.00)	(0.00)	(10.00)	(0.00)	(16.67)	(9.68)	(13.51)	(0.00)	(10.17)
2014	0	0	1	0	0	1	2	0	4
	(0.00)	(0.00)	(3.33)	(0.00)	(0.00)	(3.23)	(5.41)	(0.00)	(3.39)
Total	0	2	30	11	6	31	37	1	118
	(0.00)	(100.00)	(100.00)	(100.00)	(100.00)	(100.00)	(100.00)	(100.00)	
	[0.00]	[1.69]	[25.42]	[9.32]	[5.08]	[26.27]	[31.36]	[0.85]	[100.00]

Table II
Summary Statistics

Panel A presents summary statistics for a sample of 118 firms that experience a data breach in the following fiscal year and 1,533 firms (6,595 firm-year observations) that do not experience a data breach covered in Compustat, CRSP, and DealScan over the period 2005 to 2014. Panel B shows summary statistics for a sample of 118 firms (982 firm-loan observations) that experience a data breach in the following fiscal year and 1,533 firms (8,627 firm-loan observations) that do not experience a data breach over the period 2005 to 2014. We exclude firms that operate in the financial industry (SIC codes between 6000 and 6999). The appendix provides detailed descriptions of the construction of the variables. ***, **, and * denote that the mean and median differences in firm and industry characteristics between affected and non-affected firms are significant at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm characteristics

	Firm-years followed by data breach (N=118): A		Firm-years without data breach (N=6,595): B		Test of difference (A-B)	
Variable	Mean	Median	Mean	Median	Mean	Median
Market value ($ billion)	14.343	6.848	3.788	1.282	10.555***	5.566***
Leverage	0.295	0.266	0.275	0.260	0.021	0.006
Tobin's *q*	1.770	1.513	1.642	1.402	0.128*	0.111***
ROA	0.149	0.138	0.134	0.126	0.015**	0.012*
Stock return	0.165	0.132	0.148	0.099	0.017	0.033
Tangibility	0.270	0.199	0.333	0.250	-0.063***	-0.051***
Cash flow volatility	0.009	0.005	0.012	0.008	-0.003***	-0.003***
Stock return volatility	0.082	0.070	0.113	0.097	-0.031***	-0.027***
Credit rating (indicator)	0.729	1.000	0.554	1.000	0.175***	0.000***
R&D	0.012	0.000	0.014	0.000	-0.002	0.000
Firm age	31.822	22.000	24.859	18.000	6.964***	4.000***
Sale growth	0.112	0.079	0.121	0.085	-0.010	-0.006
Institutional blockholder (indicator)	0.644	1.000	0.778	1.000	-0.134***	0.000***
Board size	10.577	11.000	9.048	9.000	1.529***	2.000***
Fraction of outside directors	0.845	0.875	0.838	0.875	0.006	0.000
CEO-Chair duality	0.542	1.000	0.533	1.000	0.009	0.000
CEO age	57.130	57.000	56.042	56.000	1.088	1.000
CEO tenure	7.019	5.500	7.098	5.000	-0.079	0.500
Proportion equity-based pay	0.466	0.523	0.443	0.487	0.023	0.036

Panel B: Loan characteristics

	Loans issued to breached firms (N=982): a		Loans issued to non-breached firms (N=8,627): b		Test of difference (a - b)	
Variable	Mean	Median	Mean	Median	Mean	Median
Loan spread (basis point)	178.42	150.00	217.33	175.00	-38.91***	-25.00***
Loan amount ($ millions)	1,345	663.00	484.00	250.00	861***	413.00***
Loan maturity (months)	51.22	60.00	53.28	60.00	-2.07***	0.00*
Number of lenders	12.03	9.00	8.42	7.00	3.60***	2.00***
Term loan (indicator)	0.26	0.00	0.28	0.00	-0.02	0.00

Table III
Effects of Data Breaches on Loan Spreads

This table presents descriptive statistics for treatment firms that experience a data breach over the period 2005 to 2014 and control firms that do not experience a data breach over the same period (Panel A) and estimates of ordinary least squares (OLS) regressions in which the dependent variable is the natural logarithm of all-in-spread-drawn (AISD), a rate a borrower pays in basis points over LIBOR or the LIBOR equivalent (Panel B). The sample consists of 1,160 loans issued by 212 firms (106 treatment firms that experience a data breach over the period 2005 to 2014 and 106 control firms that do not experience a data breach over the same period). We exclude firms that operate in the financial industry (SIC codes between 6000 and 6999). The propensity score is calculated using the logit regression of *Data breach* (an indicator that takes the value of one if a firm experiences a data breach and zero otherwise) on log (market value), leverage, Tobin's *q*, ROA, tangibility, stock return, cash flow volatility, and institutional blockholder (indicator). We require treatment and matching firms to be in the same industry (the same two-digit standard industrial classification (SIC) codes) and in the same fiscal year. We require that treatment and matching firms have at least one loan issued during both the periods three years before and three years after the data breach. *Post* takes the value of one for the post-breach period (year *t*, year *t*+1, and year *t+2*) and zero for the pre-breach period (year *t*-1, year *t*-2, and year *t*-3), where year *t* is defined as the first one-year period starting one day after the data breach announcement. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Descriptive statistics for propensity score-matched sample firms

	Treatment firms with a data breach (N=106): a		Control firms without a data breach (N=106): b		Test of difference (a –b): *p*-value	
Variable	Mean	Median	Mean	Median	*t*-test	Wilcoxon *z*-test
Log (market value)	8.561	8.671	8.402	8.641	0.78	0.75
Leverage	0.283	0.266	0.282	0.248	0.04	0.00
Tobin's *q*	1.758	1.488	1.744	1.622	0.14	0.76
ROA	0.149	0.134	0.145	0.143	0.36	0.30
Stock return	0.166	0.140	0.182	0.108	0.29	0.17
Tangibility	0.260	0.181	0.279	0.197	0.62	0.66
Cash flow volatility	0.009	0.005	0.009	0.005	0.09	0.55
Institutional blockholder (indicator)	0.679	1.000	0.745	1.000	1.06	1.06

Panel B. Effects of data breaches on loan spreads

	Log (loan spread)		
Independent variable	(1)	(2)	(3)
Post (indicator) × Data breach (indicator)	0.173**	0.180**	
	(0.041)	(0.041)	
Year *t*			0.154
			(0.110)
Year *t*+1			0.184**
			(0.039)
Year *t*+2			0.064
			(0.532)
Log (market value)		-0.177**	-0.087
		(0.012)	(0.231)
Book leverage		-0.783**	-0.225
		(0.010)	(0.409)
Tobin's *q*		0.042	-0.027
		(0.589)	(0.748)
ROA		-2.639***	-2.925***
		(0.001)	(0.000)
Stock return		0.035	-0.003

		(0.568)	(0.957)
Tangibility		-0.922	-1.013**
		(0.134)	(0.048)
Cash flow volatility		5.513	9.914
		(0.448)	(0.124)
Institutional blockholder (indicator)		0.082	0.066
		(0.266)	(0.266)
Log (loan size)		-0.019	-0.040
		(0.583)	(0.251)
Log (loan maturity)		0.033	0.062
		(0.522)	(0.183)
Term loan (indicator)		0.189***	0.140***
		(0.000)	(0.000)
Number of lenders		-0.009*	-0.009*
		(0.089)	(0.070)
Firm fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
Loan purpose fixed effects	Y	Y	Y
Observations	1,160	1,160	1,160
Adj. R^2	0.816	0.813	0.836

Table IV
Effects of Data Breaches on Loan Spreads by the Magnitude of Expected Loss

The table presents estimates of OLS regressions in which the dependent variable is the natural logarithm of all-in-spread-drawn (AISD), a rate a borrower pays in basis points over LIBOR or the LIBOR equivalent. The sample consists of 1,160 loans issued by 212 firms (106 treatment firms that experience a data breach over the period 2005 to 2014 and 106 control firms that do not experience a data breach over the same period). We exclude firms that operate in the financial industry (SIC codes between 6000 and 6999). The propensity score is calculated using the logit regression of *Data breach* (an indicator that takes the value of one if a firm experiences a data breach and zero otherwise) on log (market value), leverage, Tobin's *q*, ROA, tangibility, stock return, cash flow volatility, and institutional blockholder (indicator). We require treatment and matching firms to be in the same industry (the same two-digit standard industrial classification (SIC) codes) and in the same fiscal year. We require that treatment and matching firms have at least one loan issued in both the periods three years before and three years after the data breach. *Post* takes the value of one for the post-breach period (year *t*, year *t*+1, and year *t+2*) and zero for the pre-breach period (year *t*-1, year *t*-2, and year *t*-3), where year *t* is defined as the first one-year period starting one day after the data breach announcement. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Subsample analyses according to data breach characteristics

	Log (loan spread)	
Independent variable	(1)	(2)
Post (indicator) × Data breach with financial information loss (indicator)	0.153*	
	(0.065)	
Post (indicator) × Data breach without financial information loss (indicator)	0.183	
	(0.179)	
Post (indicator) × Data breach with negative CAR (-1, 1) (indicator)		0.166**
		(0.048)
Post (indicator) × Data breach with positive CAR (-1, 1) (indicator)		0.134
		(0.308)
Test of coefficient differences (*p*-value)	(0.836)	(0.811)
Control variables (same as in Panel B of Table III)	Y	Y
Firm fixed effects	Y	Y
Year fixed effects	Y	Y
Loan purpose fixed effects	Y	Y
Observations	1,160	1,160
Adj. R^2	0.836	0.836

Panel B. Subsample analyses according to industry characteristics

	Log (loan spread)			
	Consumer focused industries	Other industries	High industry Tobin's *q*	Low industry Tobin's *q*
Independent variable	(1)	(2)	(3)	(4)
Post (indicator) × Data breach (indicator)	0.253**	0.073	0.035	0.319**
	(0.028)	(0.460)	(0.695)	(0.030)
Control variables (same as in Panel B of Table III)	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Loan purpose fixed effects	Y	Y	Y	Y
Observations	712	448	641	519
Adj. R^2	0.833	0.847	0.859	0.804

Table V
Effects of Data Breaches on Loan Spreads: Subgroup Analyses According to Information Environment

The table presents estimates of OLS regressions in which the dependent variable is the natural logarithm of all-in-spread-drawn (AISD), a rate a borrower pays in basis points over LIBOR or the LIBOR equivalent. The sample consists of 1,160 loans issued by 212 firms (106 treatment firms that experience a data breach over the period 2005 to 2014 and 106 control firms that do not experience a data breach over the same period). We exclude firms that operate in the financial industry (SIC codes between 6000 and 6999). The propensity score is calculated using the logit regression of *Data breach* (an indicator that takes the value of one if a firm experiences a data breach and zero otherwise) on log (market value), leverage, Tobin's *q*, ROA, tangibility, stock return, cash flow volatility, and institutional blockholder (indicator). We require treatment and matching firms to be in the same industry (the same two-digit standard industrial classification (SIC) codes) and in the same fiscal year. We require that treatment and matching firms have at least one loan issued in both the periods three years before and three years after the data breach. *Post* takes the value of one for the post-breach period (year *t*, year *t*+1, and year *t+2*) and zero for the pre-breach period (year *t*-1, year *t*-2, and year *t*-3), where year *t* is defined as the first one-year period starting one day after the data breach announcement. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Log (loan spread)							
	Analyst forecast dispersion		Analyst following		R&D intensity		Bid-ask spread	
	High	Low	High	Low	High	Low	High	Low
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post (indicator) × Data breach (indicator)	0.204*	-0.069	0.042	0.336**	0.422**	0.038	0.295**	0.121
	(0.091)	(0.526)	(0.674)	(0.034)	(0.034)	(0.585)	(0.032)	(0.395)
Control variables (same as in Panel B of Table III)	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Loan purpose fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Observations	428	383	546	425	317	843	540	611
Adj. R^2	0.824	0.881	0.828	0.863	0.815	0.822	0.837	0.865

Table VI
Effects of Data Breaches on Loan Spreads: Subgroup Analyses according to Asset Substitution Problem

The table presents estimates of OLS regressions in which the dependent variable is the natural logarithm of all-in-spread-drawn (AISD), a rate a borrower pays in basis points over LIBOR or the LIBOR equivalent. The sample consists of 1,160 loans issued by 212 firms (106 treatment firms that experience a data breach over the period 2005 to 2014 and 106 control firms that do not experience a data breach over the same period). We exclude firms that operate in the financial industry (SIC codes between 6000 and 6999). The propensity score is calculated using the logit regression of *Data breach* (an indicator that takes the value of one if a firm experiences a data breach and zero otherwise) on log (market value), leverage, Tobin's *q*, ROA, tangibility, stock return, cash flow volatility, and institutional blockholder (indicator). We require treatment and matching firms to be in the same industry (the same two-digit standard industrial classification (SIC) codes) and in the same fiscal year. We require that treatment and matching firms have at least one loan issued in both the periods three years before and three years after the data breach. *Post* takes the value of one for the post-breach period (year t, year t+1, and year t+2) and zero for the pre-breach period (year t-1, year t-2, and year t-3), where year t is defined as the first one-year period starting one day after the data breach announcement. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Subsample analyses according to firm risk

	Log (loan spread)					
	Cash flow volatility		Book leverage		Credit rating score	
	High	Low	High	Low	High	Low
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
Post (indicator) × Data breach (indicator)	0.226**	0.085	0.242**	0.062	0.122	0.244**
	(0.045)	(0.540)	(0.037)	(0.601)	(0.487)	(0.022)
Control variables (same as those in Panel B of Table III)	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Loan purpose fixed effects	Y	Y	Y	Y	Y	Y
Observations	521	639	641	519	248	912
Adj. R^2	0.853	0.860	0.888	0.790	0.760	0.763

Panel B. Subsample analyses according to variables measuring shareholder-creditor conflicts

	Log (loan spread)			
	Dividend payers (indicator)	No payers	Nimble dividend states (indicator)	Other states
Independent variable	(1)	(2)	(3)	(4)
Post (indicator) × Data breach (indicator)	0.190*	0.071	0.185**	0.100
	(0.092)	(0.532)	(0.020)	(0.582)
Control variables (same as those in Panel B of Table III)	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Loan purpose fixed effects	Y	Y	Y	Y
Observations	642	518	826	298
Adj. R^2	0.837	0.787	0.855	0.810

Panel C. Subsample analyses according to CEO compensation and CEO characteristics

	Log (loan spread)					
	Proportion of equity-based pay to CEO pay		CEO age		CEO tenure	
	High	Low	High	Low	High	Low
Independent variable	(1)	(2)	(5)	(6)	(7)	(8)
Post (indicator) × Data breach (indicator)	0.221*	0.176	-0.133	0.412***	-0.108	0.369***
	(0.095)	(0.293)	(0.128)	(0.009)	(0.238)	(0.007)
Control variables (same as those in Panel B of Table III)	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Loan purpose fixed effects	Y	Y	Y	Y	Y	Y
Observations	486	510	467	522	465	524
Adj. R^2	0.864	0.865	0.874	0.872	0.893	0.844

Table VII
Effects of Data Breaches on Loan Spreads: Subgroup Analyses According to Internal / External Governance

The table presents estimates of OLS regressions in which the dependent variable is the natural logarithm of all-in-spread-drawn (AISD), a rate a borrower pays in basis points over LIBOR or the LIBOR equivalent. The sample consists of 1,160 loans issued by 212 firms (106 treatment firms that experience a data breach over the period 2005 to 2014 and 106 control firms that do not experience a data breach over the same period). We exclude firms that operate in the financial industry (SIC codes between 6000 and 6999). The propensity score is calculated using the logit regression of *Data breach* (an indicator that takes the value of one if a firm experiences a data breach and zero otherwise) on log (market value), leverage, Tobin's *q*, ROA, tangibility, stock return, cash flow volatility, and institutional blockholder (indicator). We require treatment and matching firms to be in the same industry (the same two-digit standard industrial classification (SIC) codes) and in the same fiscal year. We require that treatment and matching firms have at least one loan issued in both the periods three years before and three years after the data breach. *Post* takes the value of one for the post-breach period (year *t*, year *t*+1, and year *t+2*) and zero for the pre-breach period (year *t*-1, year *t*-2, and year *t*-3), where year *t* is defined as the first one-year period starting one day after the data breach announcement. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Log (loan spread)					
	Proportion of outside directors to the board		CEO-Chair duality (indicator)		Herfindahl index	
	High	Low	Yes	No	High	Low
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
Post (indicator) × Data breach (indicator)	-0.066	0.245*	0.281*	0.132	0.219*	0.070
	(0.540)	(0.069)	(0.062)	(0.170)	(0.076)	(0.484)
Control variables (same as in Panel B of Table III)	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Loan purpose fixed effects	Y	Y	Y	Y	Y	Y
Observations	474	620	598	562	549	611
Adj. R^2	0.857	0.858	0.840	0.845	0.878	0.819

Table VIII
Effects of Data Breaches on Loan Spreads: Subgroup Analyses According to Durable Lending Relationship

The table presents estimates of OLS regressions in which the dependent variable is the natural logarithm of all-in-spread-drawn (AISD), a rate a borrower pays in basis points over LIBOR or the LIBOR equivalent. The sample consists of 1,160 loans issued by 212 firms (106 treatment firms that experience a data breach over the period 2005 to 2014 and 106 control firms that do not experience a data breach over the same period). We exclude firms that operate in the financial industry (SIC codes between 6000 and 6999). The propensity score is calculated using the logit regression of *Data breach* (an indicator that takes the value of one if a firm experiences a data breach and zero otherwise) on log (market value), leverage, Tobin's *q*, ROA, tangibility, stock return, cash flow volatility, and institutional blockholder (indicator). We require treatment and matching firms to be in the same industry (the same two-digit standard industrial classification (SIC) codes) and in the same fiscal year. We require that treatment and matching firms have at least one loan issued in both the periods three years before and three years after the data breach. *Lending duration* is the number of months that have elapsed since the issuance of loans arranged by the same lead bank. *Number of lead bank loans* is the ratio of a borrower's number of loans arranged by a lead bank in the past five years to the borrower's total number of loans made in the past five years. *Post* takes the value of one for the post-breach period (year *t*, year *t*+1, and year *t+2*) and zero for the pre-breach period (year *t*-1, year *t*-2, and year *t*-3), where year *t* is defined as the first one-year period starting one day after the data breach announcement. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Log (loan spread)			
	Lending duration		Number of lead bank loans	
	Long	Short	High	Low
Independent variable	(1)	(2)	(3)	(4)
Post (indicator) × Data breach (indicator)	0.143	0.416**	0.098	0.222**
	(0.108)	(0.013)	(0.314)	(0.040)
Control variables (same as in Panel B of Table IV)	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Loan purpose fixed effects	Y	Y	Y	Y
Observations	576	584	521	609
Adj. R^2	0.872	0.853	0.890	0.846

Table IX
Effects of Data Breaches on Firms' Debt Structure

This table presents estimates of ordinary least squares (OLS) regressions in which the dependent variable is the ratio of bank debt (sum of revolving credit and term loans) to total debt (Column 1 and 2) and the ratio of public debt (sum of bonds and notes and commercial paper) to total debt (Column 3 and 4). The sample consists of 987 firm-year observations of 212 unique firms (106 treatment firms that experience a data breach over the period 2005 to 2014 and 106 control firms that do not experience a data breach over the same period). We exclude firms that operate in the financial industry (SIC codes between 6000 and 6999). The propensity score is calculated using the logit regression of *Data breach* (an indicator that takes the value of one if a firm experiences a data breach and zero otherwise) on log (market value), leverage, Tobin's *q*, ROA, stock return, tangibility, cash flow volatility, and institutional blockholder (indicator). We require treatment and matching firms to be in the same industry (the same two-digit standard industrial classification (SIC) codes) and in the same fiscal year. We require that treatment and matching firms have at least one loan issued in both the periods three years before and three years after the data breach. *Post* takes the value of one for the post-breach period (year *t*, year *t*+1, and year *t+2*) and zero for the pre-breach period (year *t*-1, year *t*-2, and year *t*-3), where year *t* is defined as the first one-year period starting one day after the data breach announcement. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Bank debt / total debt		Public debt / total debt	
Independent variable	(1)	(2)	(3)	(4)
Post (indicator) × Data breach (indicator)	-0.077**		0.062*	
	(0.035)		(0.060)	
Year *t*		-0.065*		0.059**
		(0.056)		(0.049)
Year *t*+1		-0.075*		0.070*
		(0.052)		(0.090)
Year *t*+2		-0.112**		0.060
		(0.041)		(0.235)
Control variables (same as those in Panel B of Table III)	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Observations	971	971	938	938
Adj. R^2	0.737	0.737	0.661	0.660

Appendix

The appendix provides detailed descriptions of all variables used in the tables.

Variable	Definition	Source
Firm characteristics		
Analyst following	Number of analysts following. We divide high and low groups based on whether a firm is covered by three or fewer analysts (Balakrishnan et al. (2014)). The number of analyst forecasts is based on the annual (fourth quarter) earnings and forecasts made in the 90 days prior to the earnings announcement date.	IBES
Analyst forecast dispersion	Standard deviation of analyst forecasts. We use the annual (fourth quarter) earnings and forecasts made in the 90 days prior to the earnings announcement date.	IBES
Bid-ask spread	Annual average of a firm's daily bid-ask spread. We use daily closing bid and ask data to calculate the spread (100 × (ask − bid) / [(ask + bid) / 2]). We then compute the average value of these daily bid-ask spreads over the fiscal year.	CRSP
Board size	Number of directors on the board	BoardEx
Book leverage	(*dltt* + *dlc*) / *at*	Compustat
Cash flow volatility	Standard deviation of quarterly operating income before depreciation (*oibdp*) over the past 2 years divided by total assets (*at*)	Compustat
CEO-chair duality (indicator)	One if the CEO is also the chair of the board and zero otherwise	ExecuComp
Consumer-focused industry (indicator)	Industries with SIC codes of 0100–0999, 2000–2399, 2500–2599, 2700–2799, 2830–2869, 3000–3219, 3420–3429, 3523, 3600–3669, 3700–3719, 3751, 3850–3879, 3880–3999, 4813, 4830–4899, 5000–5079, 5090–5099, 5130–5159, 5220–5999, 7000–7299, or 7400–9999 (Lev, Petrovits, and Radhakrishnan (2010))	Compustat
Credit score	Scale numbers of alphabetical symbols of S&P domestic long-term issuer credit ratings (*splticrm*) 1=AAA,2=AA,3=A,4=BBB,5=BB, 6=B or worse, and 7=no rating (Lin et al. (2011))	Compustat
Data breach with financial information loss (indicator)	One if a firm experiences a data breach involving the loss of social security numbers or credit card information and zero otherwise	PRC
Data breach with negative CAR (-1,1) (indicator)	One if a stock market reaction to a data breach announcement is negative and zero otherwise, in which the daily abnormal stock returns are cumulated to obtain the cumulative abnormal return (CAR) from day t1 before the breach announcement date to day t1 after the breach announcement date, using the CRSP value-weighted return as a proxy for the market return.	CRSP, Factiva
Data breach with positive CAR (-1,1) (indicator)	One if a stock market reaction to a data breach announcement is positive and zero otherwise, in which the daily abnormal stock returns are cumulated to obtain the cumulative abnormal return (CAR) from day t1 before the breach announcement date to day t1 after the breach announcement date, using the CRSP value-weighted return as a proxy for the market return.	CRSP, Factiva
Delaware (indicator)	One if a firm is incorporated in Delaware and zero otherwise	Compustat
Dividend payer (indicator)	One if the sum of common shares (*dvc*) and dividend for preferred shares (*dvp*) is positive and zero otherwise	Compustat
Fraction of outside directors to board	Proportion of outside directors on the board	BoardEx
Industry Herfindahl index	Sum of squared market shares of firms' sales at the two-digit SIC industries in a given year. Negative or missing sales are excluded when computing the index.	Compustat
Industry Tobin's *q*	Median Tobin's *q* of all sample firms in the same two-digit SIC code industries	Compustat

Institutional blockholder (indicator)	One if a firm has at least one institutional block shareholder and zero otherwise. Institutional block shareholders are defined as institutional shareholders that own more than 5% of a firm's equity scaled by the total number of shares outstanding.	Institutional (13f) Holdings
Market value	Market value of equity (prcc_f × *csho*)	Compustat
Nimble dividend policy (indicator)	One if a firm is incorporated in nimble dividend policy states (Arizona, Delaware, Kansas, Louisiana, Maine, New Hampshire, Nevada, Oklahoma, Rhode Island, and Vermont)	Compustat
Proportion of equity-based pay	Value of option awards (ExecuComp: the value of stock options (*option_awards_blk_value* before FAS 123R and *options_awards_fv* afterward) plus the value of stock grants (ExecuComp: *rstkgrnt* before FAS 123R and *stock_awards_fv* afterward) scaled by the amount of total compensation (ExecuComp: *tdc1*)	ExecuComp
R&D	R&D expenditure. Missing values are coded zero Max (0, *xrd*) / *at*	Compustat
ROA	Operating income before depreciation divided by total assets *oibdp* /*at*	Compustat
Stock return	Buy-and-hold monthly stock returns during a fiscal year	CRSP
Stock return volatility	Standard deviation of monthly stock returns during a fiscal year	CRSP
Tangibility	Net property, plant, and equipment (*ppent*) divided by total assets (*at*)	Compustat
Tobin's *q*	Market value of assets divided by book value of assets (*at* - *ceq* + *prcc_f* × *csho*) / *at*	Compustat
CEO characteristics		
CEO age	CEO age	ExecuComp
CEO tenure	Number of years the CEO has held the office	ExecuComp
CEO-chair duality (indicator)	One if the CEO is also the chair of the board and zero otherwise	BoardEx
Loan characteristics		
Lending duration	To determine lending relationship for a given loan, we focus on the loan's lead bank. We designate lenders that are retained in any of three roles as lead banks: 1) any lenders for which the field "lead arranger credit" is marked "Yes" in DealScan, 2) the lenders of all sole lender loans, and 3) any lenders for which the field "lender roles" are "agent", "administrative agent", "arranger", or "lead bank." If a loan contains multiple lead banks, we choose a bank that has issued more loans in the previous five years as a lead bank. Lending relation is calculated as the number of months that have elapsed since the borrower obtained a loan arranged by the same lead bank.	DealScan
Loan purpose dummies	Indicator variables for loan purposes, including debt repayment, general corporate purposes (corporate purposes, working capital), acquisitions (takeover, acquisition line, LBO), CP backup, and others	DealScan
Log (loan maturity)	Natural logarithm of loan maturity (months)	DealScan
Log (loan size)	Natural logarithm of facility amount ($ million)	DealScan
Number of lead bank loans	Number of loans arranged by a lead bank to a borrower in the past five years divided by the total number of loans that the borrower issued in the past five years	DealScan
Number of lenders	Total number of lenders in a loan	DealScan
Term loan (indicator)	One if a loan is term loan and zero otherwise	DealScan

Essay two

Corporate income tax and executive compensation

Corporate Income Tax and Executive Compensation

Abstract

We investigate the impact of corporate income taxes on executive compensation. Using staggered changes in corporate income tax rates across U.S. states, we find that the sensitivities of executives' wealth to stock return (delta) and the sensitivities of executives' wealth to return volatility (vega) increase as the state tax increases. We also find that the value of firm-specific wealth increases in response to the tax increases. We further find that after the tax increase, managers receive more equity-intensive compensation and the impact of tax changes on delta and vega is more pronounced with the executive's myopic characteristics and better corporate governance. Finally, we show that the adverse effect of taxes on firm risk tend to be mitigated after managers experience increases in delta and vega. Taken together, shareholders factor corporate income taxes into managerial compensation contracts.

Keywords: Corporate taxes, Executive compensation, Risk taking

JEL Classification: G32, G38, M12

I. Introduction

Taxation has played an important role in corporate behavior. Taxes affect firms' capital structure, organizational form and restructuring, payout, risk management, and compensation policies.[1] Corporate income taxes also play a role in firm risk. Since they are asymmetrically levied on the firm's profits, not losses, higher taxes reduce the variance of firm performance.[2] Consistent with this premise, empirical evidence suggests that firms reduce risk in response to a tax increase (Ljungqvist, Zhang, and Zuo (2017)). Besides, corporate taxes also affect future innovation. Mukherjee, Singh, and Žaldokas (2017) find that taxes affect patenting, R&D investment and new product launchings, suggesting that higher corporate taxes reduce innovators incentives and discourage risk taking. These results suggest that corporate taxes lead to a significant reduction in firm risk and managerial risk bearing, which increases agency conflicts between manager and shareholder. An interesting question then is how shareholders react to the decline in managerial risk-taking that is driven by the change in corporate income taxes.

The classic literature on principal-agent studies suggests that providing high-powered incentives is a solution for a shareholder to ensure managers to take optimal actions. There is ample evidence that highlights a positive relation between high-powered incentives and firm risk (e.g., Jensen and Meckling (1976), Guay (1999), Coles, Daniel, and Naveen (2006), Low (2009), Chava and Purnanandam (2010), Gormley, Matsa, and Milbourn (2013), Ellul, Wang, and Zhang (2017)). The link between the impact of taxes on firm risk and the role of equity-based compensation in risk related agency problem suggests that shareholders may motivate a manager by providing more high-power incentives. However, there is only modest evidence that taxes are a driving factor affecting corporate or employee compensation decisions (Graham (2013)). In this paper, we empirically examine whether taxes on corporate income affect managerial compensation contracts.

[1] Graham (2013) provides an excellent review of corporate and personal taxation research.
[2] Suppose there is a project with two equally likely outcomes, "good" and "bad". The project yields a profit of \$100 in a good state of economy but a loss of \$20 in a bad state of economy. The variance of this project falls from 3600 to 1600 when the tax rate increases from zero to 40 %.

The key prediction of the principal-agent model of executive compensation is that the executive's pay-performance sensitivity is decreasing in the variance of the firm's performance (e.g., Holmström and Milgrom (1987)). Aggarwal and Samwick (1999) find strong empirical evidence that the pay-performance sensitivities of both CEOs and other executives are decreasing in the variance of their firms' stock returns. These studies clearly show that the performance-related component of managerial compensation is decreasing in the variance of firm performance from both theoretical and empirical perspectives. We extend the shareholder-manager contracting model where corporate tax exists. Since corporate tax distorts manager's incentive to exert effort and reduces the firm's performance variance, shareholders provide more equity-based compensation contracts to maximize their net-of-wage firm value when corporate tax increases. Therefore, we predict that corporate taxes increase the executive's equity-based compensation.

Alternatively, it is possible that shareholders do not increase CEO's compensation in response to corporate tax increases. First, using the higher level of equity-based compensation after tax increases may amplify the reduction in the shareholders' residual claim. Thus firms would reduce the use of equity-based compensation after corporate tax increases. Second, in practice, firms would minimize the effect of tax increases by employing a variety of tax strategies. For example, the reduction in cash flows due to corporate taxes could be written off by using tax code that permits offset of losses. Also, firms strategically use tax credits to dilute the adverse impact of taxes. Thus shareholders have weak incentives to provide high-powered compensation contracts to managers in the existence of tactical and sophisticated tax planning.

Overall, the arguments above suggest that the effect of corporate income taxes on managerial compensation is empirically ambiguous.

As compared to the impact of tax increases, the effect of tax cuts on executive compensation is expected to be relatively modest. Our model predicts symmetric responses to tax increases or tax cuts, but, in practice, the effect of tax cuts on executive compensation might be attenuated reflecting the weak relation between risk-taking and tax cuts. For example, Ljungqvist, Zhang, and Zuo (2017) argue that the effect of a tax cut on risk-taking is likely limited because of the presence of creditors who are afraid of a risk-shifting problem. Mukherjee, Singh, and Žaldokas (2017) further present evidence that firms change their incentives to innovate with regards to tax increases but not tax cuts, suggesting that investments are less responsive to tax cuts. Given the level of equity-based compensation is decreasing in the variance of firm performance, the weak and insignificant relationship between tax cuts and risk-taking behavior might diminish the effect of tax cuts on managerial equity compensation in the real world.

To examine the effect of corporate income taxes on the compensation contracts of managers, we exploit staggered changes in state-level corporate tax rates and perform a difference-in-differences test. This empirical approach is appealing to identify corporate tax effects because we can show that the impact of tax changes is similar across different time periods and different states. We use a sample of 68,703 executive-year observations of U.S. firms from 1992 to 2015. Following compensation literature, two risk-taking incentives: the sensitivity of executive wealth to stock volatility (executive vega) and the sensitivity of executive wealth to stock price (executive delta).[3] Consistent with our prediction, our results show that firms increase executive delta and vega following tax increases. On average, treated firms increase executive delta and vega by 5.59% and 5.39% for one percentage-point tax increase, relative to other firms headquartered in states that are not subject to a tax change. We do not find any significant change in executive delta and vega following tax cuts.

To address econometric concerns that arise from our difference-in-differences approach, we perform several additional tests. First, we present dynamic tests whether the parallel-trends assumption is violated. We find no significant change in executive delta or vega before the increase in tax rates, supporting the

[3] We use delta as a measure of pay-performance sensitivity. Delta, the dollar change in wealth associated with a 1 % change in the firm's stock price, is seen as alignment the incentives of managers with the interests of shareholders (Coles, Daniel, and Naveen (2006)). On the other hand, it is possible that the higher level delta also expose managers to firm-specific risk and they might forgo some risky but positive NPV projects from the perspective of shareholders. Several empirical studies report the negative impact of delta on corporate risk-taking policies. For example, Coles, Daniel, and Naveen (2006) find that, in contrast to the results on vega, delta provides strong incentives to decrease R&D expenditures, increase capital expenditures, and decrease leverage. Low (2009) find that it is CEO vega, not CEO delta, that helps encourage risk-taking. Chava and Purnanandam (2010) find that CEOs' delta (vega) are associated with lower (higher) leverage and higher (lower) cash balances. They also find that CFO's delta (vega) are associated with safer (riskier) debt maturity choices and higher (lower) earnings-smoothing through accounting accruals. To the extent that shareholders understand the nature of managerial risk aversion and the negative consequences of providing higher delta, shareholders compensate managers with stock options rather than shares, and thus delta is less likely to change in response to tax changes. Therefore, even though our model predicts the increasing relation between corporate tax and executive delta, it is empirically unclear whether corporate income taxes affect executive delta.

parallel-trends assumption. Second, we restrict controls firms to those located in neighboring states. To the extent that firms located in neighboring states face different state tax rates from treat firms located in the states that change tax rates but share similar local economic conditions, this test alleviates the concern that unobserved local economic conditions lead the spurious relation between tax and managerial compensation. We find that the positive effect of an increase in tax rate on executive delta and vega is robust to restricting controls firms to those located in neighboring states. Finally, to ensure that our results are not driven by anticipated future changes in state income taxes, we exclude firms in states whose tax rate changes are likely to be anticipated. We first examine the effect of tax increases by eliminating firms headquartered in New England (i.e., Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island or Vermont), following Ljungqvist and Smolyansky (2016). In an additional test, we exclude firms headquartered in Colorado, Connecticut, Minnesota, or New York, (Heider and Ljungqvist (2015)). The results are robust to controlling for the anticipation effects.

We find that the increase in delta and vega is similar for both CEOs and non-CEO executives. After the increase in state corporate tax rate, CEO delta and vega increase by 4.45% and 4.35%, respectively. Non-CEO executives also experience a significant increase in their delta and vega after tax rises. While tax cuts do not affect delta, we find symmetric responses to tax changes for the CEO group. Firms reduce CEO vega by 2.16% in response to tax cuts, while the effect of tax cuts on vega is small and statistically insignificant for non-CEO executives, suggesting that shareholders take account of tax cuts when they design CEOs' risk-taking incentives.

We also examine executives' the value of equity portfolio related to firms. Consistent with executive delta and vega results, we find that equity portfolio increases after corporate tax increases. Again, tax cuts continue to have an insignificant effect on firm-related wealth. These results provide further evidence that shareholders use equity-based compensation to maximize their net-of-wage firm value.

To further test whether corporate income taxes affect managerial compensation, we examine how taxes affect the composition of total compensation. Specifically, we estimate regressions in which the dependent variables are the proportion of equity-based compensation (e.g., option awards or stock awards) to total pay. We find that firms provide more equity-intensive compensation to executives when a state increases its corporate income tax rate.

We explore cross-sectional heterogeneity to understand under which the tax impact on executive vega is more pronounced. We investigate whether the tax effect on executive delta and vega varies across executives' characteristics. First, since managerial ownership plays an essential role in aligning the managerial incentives with those of shareholders and provides managers with strong incentives to exert their effort, the impact of tax increases on executive compensation is more likely to be stronger when the executives have lower ownership in the firms. Consistent with the prediction, we find that the impacts of tax increases are higher for firms whose executive has low ownership. Second, we examine whether the effect of taxes on compensation varies across executive age. We expect the corporate tax effect on vega to be more pronounced when managers are older. Since old managers are more likely to keep the status quo and less likely to engage in long-term commitments, shareholders offer them more higher-power incentives in response to tax increases. Consistent with our prediction, we find that the effect of tax increases is more pronounced for old executives. Next, we examine the cross-sectional variation in the corporate tax effect according to corporate governance. If shareholders have better ability in monitoring managerial behavior, they are more likely to react to the role of higher corporate taxes in discoursing effort. We expect the effect of corporate income tax on executive delta and vega is more pronounced when firms are better governed. Consistent with prediction, we find that the impact of corporate taxes on executive delta and vega is stronger for firms with higher institution ownership and dedicated institution ownership.

Finally, we investigate whether the changes in compensation caused by tax increases induce managerial risk taking. We find that the adverse effect of taxes on firm risk is reduced for firms whose executives experience the increase in their delta and vega. Besides, we also find that operating performance improves following tax increases when firms increase executive delta and vega. These results suggest that firms achieve the desired outcomes by adjusting managerial compensation contracts.

Our study contributes to literature at least in two important ways. First, our paper contributes to the literature on the effects of corporate taxes on compensation. Prior literature focuses on deferred tax deduction and stock options and reports the weak relation between tax rate and executive options. Hall and Liebman (2000) find a moderate tax advantage to option pay. Core and Guay (2001) find that firms grant fewer options to non-executive employees when they face a higher tax rate. Klassen and Mawani (2000) investigate Canadian firms and find that the use of option compensation decreases with the marginal tax rate. In contrast, by using staggered changes in corporate income tax rates across U.S. states, our study enables to identify the effect of corporate taxes and provides new evidence on a causal effect of corporate income taxes on the executive compensation. Besides, we can identify important channels (i.e., managerial effort and the variance of firm performance) through which the corporate income taxes affect executive compensation.

Second, we extend the literature on the effect of corporate income taxes on corporate decisions. Previous studies show that corporate income taxes affect investment (Asker, Farre-Mensa, and Ljungqvist (2015)), leverage (Heider and Ljungqvist (2015)), debt and equity issuance (Farre-Mensa and Ljungqvist (2016)), risk-taking (Ljungqvist, Zhang, and Zuo (2017)), and innovation policies (Mukherjee, Singh, Žaldokas (2017)). Our results complement the literature by showing that corporate taxes affect firms' compensation decisions.

The remainder of the paper proceeds as follows. Section II describes the sample and empirical methodology. In Section III, we examine the effects of corporate income tax changes on executive vega, conduct additional analyses to strengthen the causal effect of taxes on executive vega. Section IV investigates the relation between changes in corporate tax rates and executive compensation structure. Section V tests the heterogeneity of treatment effect to executives' characteristics. We conduct additional cross-sectional tests according to corporate governance in Section VI. Section VII examines the risk and performance consequences of changes in compensation induced by changes in corporate income taxes. In Section VIII, we report robustness tests and conclude in Section IX.

II. Data and Empirical Methodology

A. Sample and summary statistics

Our sample begins with firm-year observations covered in ExecuComp, Compstat, and CRSP databases for the period 1992-2015. We exclude firms in regulated industries (SIC codes 4900-4999 and 6000-6999). We also exclude firms headquartered outside the United States. We obtain compensation details for up to five top executives in each firm.[4] After deleting nonmissing data for compensation and control variables and their lagged values, our final sample consists of 68,703 executive-year observations for 2,112 unique firms.

We obtain state-level tax changes from Heider and Ljungqvist (2015). Since the list of tax changes cover up to the fiscal year 2012, we extend the tax change list to the fiscal year 2015 by using Book of States, Tax Foundation Special Reports, and regional Federal Reserve Reports.

Table 1 presents descriptive statistics for state tax changes and our sample firms. During our sample period, there are 22 cases of corporate income tax increases and 64 cases of tax cuts. In comparison, Heider and Ljungqvist (2015) report 43 cases of tax increases and 78 cases of tax cuts in fiscal years 1989–2011. The difference in the number of tax changes between our and their studies is that we restrict the sample firms to those covered in ExecuComp and the sample period to after 1991 since ExecuComp provides executives compensation data only from 1992.[5] The average rate of tax increases is 1.036%, while the average rate of tax cuts is 0.540%. The magnitude of tax changes is similar to that in prior studies (Heider and Ljungqvist (2015), Mukherjee, Singh, and Žaldokas (2017)).

Turning to firm characteristics, we find that average executive delta and vega are $381,000 and $63,000, respectively. Average executive total compensation is $2,645,000, of which the average option award is $812,000, the average stock award is $648,000, and the average salary and bonus compensation is $700,000. Average and median firm characteristics, including total assets, book leverage, cash holdings,

[4] Execucomp provides executive compensation data collected directly from each company's annual proxy (DEF14A SEC form). Some firms list less than five executives in their proxy statement.
[5] Note that ExecuComp's data collection on the S&P 1500 began in 1994. The data from 1992 to 1993 mostly cover the S&P 500, not the entire S&P 1500.

return on assets (ROA), Tobin's q, stock return, stock return volatility, R&D (R&D expenses/total assets), and capital expenditure, are consistent with those in prior compensation literature (Hayes, Lemmon, and Qiu (2012), Humphery-Jenner, Lisic, Nanda, and Silveri (2016)). The median firm in our sample has 31.415% of the marginal tax rate.[6]

B. Empirical methodology

Testing the relation between managerial compensation and firms' tax rate has potential endogeneity issues. For example, a high level of equity-based compensation induces managers to undertake risky investment projects, which, in turn, affects its tax status or tax credits. To address the potential endogeneity concern, we adopt staggered changes in corporate income tax rates across U.S. states (e.g., Asker, Farre-Mensa, and Ljungqvist (2015), Heider and Ljungqvist (2015), Farre-Mensa and Ljungqvist (2016), Ljungqvist, Zhang, and Zuo (2017), Mukherjee, Singh, and Žaldokas (2017)) and perform a difference-in-differences test. This approach has several advantages. First, since changes in state-level corporate income tax are staggered at different points in time, we can show that the effect of tax changes is similar across different time periods. Second, we can alleviate a concern that omitted factors (e.g. a change in local economic conditions) drive the treatment effect because tax changes occur in various states.

Specifically, we exploit a difference-in-differences test using the following regression specification:

$$\Delta Compensation_{i,j,s,t} = \beta\ Tax\ increase\ (\%)_{s,t-1} + \gamma\ Tax\ decrease\ (\%)_{s,t-1} + \delta\ \Delta X_{i,t-1} + \theta \Delta Z_{s,t-1} + \alpha_{j,t} + \varepsilon_{i,j,s,t} \quad (1)$$

where i, j, s, and t, index firm, industry, state, and year, respectively. Δ is the first difference operator. $\Delta Compensation_{i,j,s,t}$ is the change in the change in the sensitivity of CEO wealth to stock price movements (delta) and sensitivity of CEO wealth to return volatility (vega) from year *t-1* to year *t*. Delta is defined as

[6] Simulated marginal tax rate data come from John Graham's website (http://faculty.fuqua.duke.edu/~jgraham/taxform.html). Graham (1996a), Graham (1996b), and Graham, Lemmon, and Schallheim (1998) provide a detail about stimulated marginal tax rate.

the change in the dollar value of CEO wealth for a 1% change in stock price. Vega is the change in the dollar value of CEO wealth for a 0.01 change in the annualized standard deviation of stock returns. The calculation of delta and vega follows Core and Guay (2002) and Coles, Daniel, and Naveen (2006). To alleviate the concerns that arise from the skewness of delta and vega, we use *Log (1+Delta)* and *Log (1+Vega)* as the measure of $\Delta Compensation_{i,j,s,t}$. We add one dollar to delta and vega to ensure that zero-value observations are not lost in our analysis. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. $\Delta X_{i,t-1}$ is the change in firm characteristics.

We use a variety of firm characteristics used in compensation literature. We include the natural logarithm of total assets to control for firm size. We also control for leverage, cash holdings, ROA, growth opportunity (Tobin's q), investment policies (R&D and capital expenditure), past stock performance (buy-and-hold stock returns over the fiscal year), and risk (volatility of stock returns stock over the fiscal year). We also control for a firm's exposure to tax changes by using simulated marginal tax rates.

We control for several state-level variables ($\Delta Z_{s,t-1}$). We include state-level GDP growth rate and the state unemployment rate to control for local economic conditions. We obtain state-level GDP data from the U.S. Bureau of Economic Analysis and state-level unemployment data from the U.S. Bureau of Labor Statistics. We also control for state wage and capital gains taxes since they can affect managerial wealth and compensation contracts. We thus include the maximum state tax rate on wage income and the maximum state tax rate on long-term capital gains. The data on state wage and capital gains taxes come from Feenberg and Coutts (1993). Managerial risk-taking incentives could be moderated by tax loss-offset provisions (Domar and Musgrave (1944)). To control for the loss-offset rules on managerial risk-taking compensation, we include changes in loss carryback and loss carryforward rules across U.S. states. The data on state tax loss carryback and carryforward come from Ljungqvist, Zhang, and Zuo (2107). We extend the loss-offset rule changes to the fiscal year 2015 by using Book of States, Tax Foundation Reports. $\alpha_{j,t}$ is two-digit SIC(standard industry classification) industry-year fixed effects to control for unobserved, time-varying differences across industries. Standard errors are adjusted for heteroskedasticity and clustered by state. All variables are defined in Appendix. We use first differencing in our regression because it removes unobserved firm fixed effects. Besides, first differencing alleviates the common concerns rising from repeated treatments and treatment reversals.

III. Empirical results

A. Baseline results: Effects of corporate income taxes on executive delta and vega

In this section, we examine how corporate tax changes affect managerial compensation. Table II reports the results from ordinary least squares (OLS) regressions of *Equation* (1) in which the dependent variable is the change in executive delta and vega. In column (1) and (2), we include *Tax Increase (%)*, *Tax Decrease (%)*, firm characteristics and industry-year fixed effects. We find that the coefficients on *Tax Increase (%)* are positive and significant at below 5% level. For tax cuts, the coefficients on *Tax Decrease (%)* are statistically insignificant. Our findings are in line with the previous evidence that firms' asymmetric responses to tax changes: firms reduce risk-taking only for a tax increase but not for a tax decrease. Therefore, firms adjust their managers' compensation in response to tax increases because of reduced firm's risk-taking.

In column (3) and (4), we additionally control for state-level characteristics. We include the change in GDP growth, unemployment rate, wage tax rate, and capital gain tax rate. We also include four dummy variables if a state increases (decreases) in the length of tax loss carryback (carryforward) period. In column (3) and (4), we find that the coefficients on *Tax increase (%)* are 0.053 and 0.052, respectively, and they continue to be positive and significant when we control for state-level characteristics. In terms of economic magnitude, given the mean unconditional tax rate increase for our sample treated firms is 103.6 basis points, this magnitude suggests that treated firms increase executive delta by 5.59% (= 1.036 × 0.053) for one percentage point tax increase. With the sample mean delta of $ 381,000, this 5.59% increase corresponds to $21,315 increase in the executive delta. Similarly, executive vega increases by 5.39% translating into $3,394 increase in executive vega.

In sum, our results show that firms increase executives' pay sensitivity and convexity to firm value when corporate income tax rates increase. Consistent with the asymmetric impact of taxes on firm risk-taking, we find that a firm significantly raises executives' equity-based compensation in response to a tax increase but does not respond to a tax cut. Our results suggest that shareholders use high-powered incentives according to the corporate taxes to induce managers to act in the best interests of shareholders.

B. Pre-treatment trends

Our difference-in-differences regression results in Table II show that, following tax changes, firms adjust executive delta and vega relative to other firms that are not subject to tax changes. One concern with these results is that the treatment effect is due to the pre-treatment differences between treated and control firms, thus violating parallel-trend and no-treatment reversal assumptions. To address this concern, we examine the dynamics of vega in the years around corporate tax rate changes. We define *Tax increase (%) t* (*Tax decrease (%) t*) is the magnitude of top marginal corporate income tax increase (decrease) in a firm's headquartered state in a given fiscal year *t*, which are the same as *Tax increase (%)* and *Tax decrease (%)* in Table II. We then include two lag and three lead terms of *Tax increase (%) t* (*Tax decrease (%) t)* in *Equation (1)*. *Tax increase (%) t-2* indicates the two-year lagged value of *Tax increase (%) t*; *Tax increase (%) t-1* is the one-year lagged value of *Tax increase (%) t*; *Tax increase (%) t+1* is the one-year forward value of *Tax increase (%) t*; *Tax increase (%) t+2* is the two-year forward value of *Tax increase (%) t; Tax increase (%) t+3* is the three-year forward value of *Tax increase (%) t*. The two lag and three lead terms of *Tax decrease (%) t* are defined as the same as *Tax increase (%) t*.

Our variables of interest for testing pre-treatment differences are *Tax increase (%) t+1*, *Tax increase (%) t+2* and *Tax increase (%) t+3*. If the coefficients on *Tax increase (%) t+1* and *Tax increase (%) t+2* are positively or negatively significant, these results indicate that there is any difference in compensation contracts between the treatment firms and control firm prior to the corporate income tax changes. For example, suppose the coefficient on *Tax increase (%) t+2* (e.g., the change in corporate tax rate occurs in the fiscal year 2000) is positive and significant in which the compensation dependent variable is measured at *t+1* (e.g., vega measured at the fiscal year 1999). These results show that firms adjust their compensation prior to tax changes, which violates the parallel trend assumption of difference-in-differences tests. Therefore, we expect that the coefficients on *Tax increase (%) t+1, Tax increase (%) t+2,* and *Tax increase (%) t+3* are statistically insignificant if there exist no pre-treatment trends.

Table III reports the results of examining the pre-treatment trends. Consistent with our prediction, we find that *Tax increase (%) t+1, Tax increase (%) t+2* and *Tax increase (%) t+3* are statistically insignificant across all specifications. These results suggest that there is no difference in executive delta and vega between treated firms and control firms prior to the change in corporate income tax rates. In vega analysis (column (2)), we find pre-treatment trends for tax decreases. The coefficients on lead terms of *Tax decrease (%)* variables are positive at 10 % level, which violates parallel trend assumption. Overall, the findings in Table III suggest that the absence of pre-treatment trends prior to tax increases enables the causal interpretation of our results.

C. Alternative explanations

C.1. Local economic conditions

Thus far, we have shown that firms increase managers' exposure to stock price movements in response to a corporate tax rise. However, there are several alternative explanations for this finding. For example, local economic conditions simultaneously affect changes in state tax rates and managers’ compensation structure. Although we control for state-level GDP growth and unemployment rates in regressions to alleviate this concern, we further address this possibility by restricting control firms to those located in neighboring states of states in which the changes in corporate income taxes occur. Neighboring states are likely to share similar local economic conditions as the treated states, but their tax policies are different from those of treated states. Thus, by restricting control firms to those located on neighboring states and comparing our treated firms to these control firms, we can alleviate the concern that local economic conditions drive our baseline results. There are 1,689 unique control firms (29,399 executive-year observations) located in non-neighboring states. After excluding these non-neighboring state firms, we are left with 34,959 executive-year observations, which accounts for 51% of our total sample.

The results are reported in Table IV. Our results show that firms raise executive delta and vega after tax increases, suggesting that the effect of tax increases on CEO compensation is robust to controlling local economic factors. In column (1), *Tax increase (%)* is positive and significant at 5 % level, and its magnitude is similar to that of the coefficient in Table II. Vega analysis in column (2) shows that the magnitude of the coefficient on *Tax Increase (%)* slightly drop, but it remains statistically significant at 10 % level. The tax decreases do not affect the changes in delta and vega in both columns. Our results in Table IV reassure the role of corporate income taxes in managerial risk-taking incentives and suggest that the local conditions are unlikely to contaminate our baseline results.

C.2. Anticipation effects

As discussed in Section 3. B., the absence of pre-treatment trends implies that firms do not change managerial compensation in expectation of future tax increases. In this subsection, we provide additional evidence that the anticipation of future tax changes does not drive our previous results by excluding firms whose tax rates are likely to be anticipated. Specifically, we follow prior studies on state tax rates (e.g. Ljungqvist and Smolyansky (2016), Ljungqvist, Zhang, and Zuo (2017)) and exclude firms headquartered in New England (i.e., Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont) and reestimate the regressions in Table II.

The results are presents in column (1) and (2) of Table V. We find that the coefficients on *Tax increase (%)* are positive and significant in all regressions. The coefficients on *Tax decrease (%)* continue to remain insignificant. These results suggest that the treatment effect of corporate income taxes on executive vega holds after controlling for anticipation effects.

As an additional test, we exclude firms headquartered in Colorado, Connecticut, Minnesota, and New York in which states experienced the political economy events surrounding all state tax changes and at least 100 firms are affected by tax changes (Heider and Ljungqvist, (2015)). The results are reported in column (3) and (4) of Table V. Again, we find that the coefficient on *Tax increase (%)* are positive and significant at below 5 % level and their magnitude becomes slightly higher. The coefficients on *Tax decrease (%)* are not significantly different from zero. The findings in Table V suggest that our main results are not driven by the anticipation of the changes in state taxes, providing further evidence on the causal effects of tax rate rise on executive delta and vega.

D. Effect of corporate income tax on CEOs' and Non-CEO executives' delta and vega

In this subsection, we repeat the baseline analysis using CEO and non-CEO subsamples to examine whether the effect of corporate tax on executive varies across executives' positions. Since corporate taxes discourage both CEOs' and non-CEO executives' risk-taking incentives similarly, we expect that the results in CEO subsample are qualitatively similar to those in non-CEO executive subsample.

Table VI presents the results from OLS regressions in which the dependent variables are the change in delta and vega of CEO (column (1) and (2)) and the change in delta and vega of non-CEO executive (column (3) and (4)). In column (1) and (2), we find an increase in CEO delta and vega after tax increases. The coefficients on *Tax Increase (%)* are positive and significant at 5 % level across all specification in CEO subsamples. In terms of magnitude, the coefficient estimate of 0.043 (0.042) in column (1) (column (2)) indicates that firms increase their CEO delta (vega) by 4.45 % (4.35 %) for every one percent tax rate increase. We also find that firms reduce CEO vega in response to tax cuts. The coefficients on *Tax decrease (%)* are negative and significant at below 5% level. The coefficient estimate of -0.040 in column (2) translates into the 2.16 % reduction in CEO vega. Symmetric changes in CEO vega according to tax increases and cuts implies that, given a wealthy empirical evidence focusing on a positive relationship between the CEO vega and various risk-taking policies (e.g. Guay (1999), Coles, Daniel, and Naveen (2006), Low (2009), Gormley, Matsa, and Milbourn (2013)), shareholders might pay close attention to the risk-taking incentives of CEO for tax increases as well as tax cuts.

Turning to the non-CEO executive analysis in column (3) and (4), we find that non-CEO executives experience an increase in their delta and vega after tax increase. The coefficients on *Tax increase (%)* are positive and significant across all columns, similar to the results in CEO subsamples. In term of economic magnitudes, non-CEO executive delta and vega increases by 5.8%, which are larger than those for CEO.[7] The effect of tax cuts is insignificant to non-CEO executives. The coefficients on *Tax decrease (%)* are not statistically different from zero. In sum, the results in Table VI indicate that tax increases affect both CEOs' and non-CEO executives' risk-taking incentives in a similar manner.

E. Effects of corporate income taxes on executive delta and firm-specific wealth

We further examine whether state corporate taxes affect executives' firm-specific wealth. To the extent that shareholders provide more equity-based pay with managers in response to the tax increases, we expect that the value of stock and option portfolio held by an executive to increase following a tax increase. To test this prediction, we calculate executives' portfolio of stock and options using the methodology from Core and Guay (2002) and Coles, Daniel, and Naveen (2006). The results are presented in Table VII. Consistent of our expectation, we find that the impact of state tax increases on executives' firm-specific wealth is positive and significant. With the sample average equity portfolio of top five executives ($33.915 million), the coefficient estimate on *Tax increase (%)*, 0.046, corresponds to an increase in executives'

[7] In an untabulated test, we repeat the analysis using only CFOs. CFOs experience an increase in their vega by 8.33 % after a tax increase.

equity portfolio value of $1.616 million.[8] Our findings suggest that firms provide more equity-based compensation with managers, and as a result increase in the firm-specific wealth.

IV. Effects of corporate income taxes on compensation structure

In this section, we examine whether corporate income taxes affect managerial compensation structure. If shareholders change risk-taking incentives following the tax increases, we expect the proportion of equity-based compensation to total compensation to increase after tax increases.

To test these predictions, we estimate the following regression model:

$$\Delta(Compensation\ Structure)_{i,s,t-1+k} = \beta_1\ Tax\ increase\ (\%)_{st-1} + \beta_2 Tax\ decrease\ (\%)_{st-1} + \beta_3 \Delta X_{it-1} + \Delta Z_{st-1} + \alpha_{jt-1} + \varepsilon_{i,s,t-1+k} \quad (2)$$

where *i*, *s*, *j*, and *t* indicate firm, state, industry, and year ($k = 0$ to 1), respectively. Compared to *Equation* (1), which examines the effect of the change in tax rates on delta and vega a year ahead, we test whether firms contemporaneously change executive compensation structure in *Equation* (2) when $k = 0$. We use three compensation structure variables as the dependent variables: the ratio of option grants to total pay, the ratio of the sum of option grants and stock grants to total pay, and the ratio of option grants to stock grants. We include the same control variables as those used in *Equation (1)*.

Table VIII presents the results. We find that the coefficients on *Tax increase (%)* are all positive and significant in the year when the change in tax rate becomes effective (i.e., *k* = 0) These results suggests that firms immediately use more equity-based compensation in response to tax rises, consistent with our prediction. However, we find the effect of tax increases on the compensation structure is not significant in the year after tax increases (i.e., *k* =1). These results indicate that shareholders quickly change managers' compensation structure, but the adjustment is not persistent until the next year. We do not find any

[8] In an untabulated test, we decompose the executives' equity portfolio into stock and option portfolios and reestimate the regressions separately for each portfolio. The coefficient on Tax increase (%) is significant in option portfolio analysis, not in stock portfolio, suggesting that the change in option portfolio value dominates the overall change in equity portfolio.

significant evidence on tax cuts for both k = 0 and k = 1. The coefficients on *Tax decrease (%)* are insignificant across all columns.[9]

Our findings in Table VIII show that the corporate income taxes affect the composition of compensation package: the proportion of total compensation from that comes from equity grants contemporaneously increase with the tax rate. Together with our previous findings that tax increases lead to the increases in executive delta and vega in the next year, the results suggest that an immediate change in the proportion of option and stock grants to total pay results in the long run changes in vega and delta of managers' accumulated equity portfolios.

V. Impact of tax changes according to executive characteristics

To provide further evidence that the firms use high-powered compensation contracts to mitigate the negative effect of tax increases on firm performance and managerial risk-taking, in this section, we examine cross-sectional heterogeneity in treatment effects. Specifically, we investigate how the impact of tax changes on executive delta and vega differs across executives' characteristics. We use executives' stock holdings and executive age as proxies for managerial myopic traits.

We first focus on stock ownership. Jensen and Meckling (1976) and Holmström (1979) argue that managerial ownership reduces agency problems between shareholders and managers and high managerial ownership leads managers to act in the best interests of shareholders. Jensen and Meckling (1976), John and John (1993), and Begley and Feltham (1999) further argue that managers who act in the interests of shareholders have strong incentives to invest in high-risk, high-return projects that benefit shareholders. These arguments suggest that the impacts of changes in tax rates on executive compensation are more likely to be pronounced when managers have lower ownership in the firms. Second, we assess whether the relation between corporate income tax changes and risk-taking incentives is associated with the executive's age.

[9] To check whether our results are driven by the change in total compensation, in an untabulated test, we examine whether tax increases lead to the change in total compensation. We do not find that the dollar value of CEO total compensation changes after tax increases or tax decreases.

There are two different views on how the effect of taxes on vega varies across executive age. On the one hand, old age aggravates manager's myopic behaviors because old managers are more likely to keep the status quo and avoid risk-taking. On the other hand, old managers could pursue riskier corporate policies compared to younger managers because their career is less likely to be damaged in case of the failure of risky corporate decisions. Therefore, our prediction on the effect of tax increases with respect to executive age is unclear.

To examine our conjecture, we divide our sample into three subgroups according to executives' characteristics (i.e., stock holdings and age) measured in one year before a tax change. Stock holding is defined as the number of shares that an executive holds divided by the number of shares outstanding. After splitting the sample into three subgroups, we run separate regressions for each subgroup and report results for top and bottom tercile subsamples only.

Panel A of Table IX reports subsample analyses according to stock holdings. We find that the increases in executive delta and vega are more pronounced among firms with low managerial ownership. In column (3) and (4), the coefficients on *Tax increase (%)* are positive and significant at below 5 % level, while those in high stock holdings (column (1) and (2)) are statistically and economically insignificant. The differences in the effect of *Tax increase (%)* on delta and vega are statistically significant. These results are consistent with our prediction that after tax increases, firms provide more risk-taking compensation to managers who are less tied with firm value and risk. Panel B of Table IX reports cross-sectional results using executive age. While the impact of corporate taxes on delta become insignificant in both high and low executive age group, we find that the effect of tax increases on executive vega is stronger when a firm's executive is old. The magnitude of coefficient estimate in high subsample is three times greater than that in low subsample suggesting that shareholders provide old executives with high power incentives to mitigate the adverse effect of tax increases on managerial risk-taking. However, we caution that the results are suggestive. The test of differences between the two coefficients is statistically insignificant.[10]

Collectively, the results in Table IX suggests that shareholder pay attention to managers' myopic characteristics that amplify the risk-averse managerial behavior in response to tax increases. Therefore, the impact of tax increases on the delta and vega is more evident when managers have lower stock holdings, and when they are old.

VI. Impact of tax changes according to corporate governance

If shareholders and boards efficiently monitor and discipline managerial behavior, they are more likely to react and adjust managers' incentives in response to tax changes. In this section, we examine whether the tax effect on executive compensation varies with corporate governance. Hartzel and Stark (2003) find that institutional investors are positively related to the performance sensitivity of managerial compensation, suggesting that institutional monitoring mitigates the agency problem between shareholders and managers. We expect that firms with higher institutional investor ownership experience a larger increase in managers' risk-taking incentives after the tax rate rises. To test this prediction, we split our sample into three subgroups according to institutional investor ownership, measured at the end of previous fiscal year end date. We reestimate *Equation (1)* separately for each subsample. Panel A of Table X reports the estimates of regressions for top and bottom tercile subsamples. We find that the coefficients on *Tax increase (%)* are positive and significant at below 5 % level for the high institutional investor ownership group, while the coefficients are insignificant at the conventional level for the low institutional investor ownership group. The difference in the estimated coefficients between the two subgroups is significant at the 10 % level only for vega analysis. Consistent with our prediction, the results indicate that the effect of corporate income tax on executive delta and vega is more pronounced when institutional investor ownership is higher.

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[10] Unreported test, we conduct cross-sectional tests with CEO tenure. The results are similar to executive age analysis. We find that the treatment effect is more pronounced for firms whose CEO has longer tenure. This analysis is confined to CEO sample because we can only get tenure information for CEOs in Execucomp.

Bushee (1998) classifies institutional investors into three groups – dedicated, quasi-indexer, and transient and finds that firms with higher level of dedicated institutions ownership are less likely to cut R&D to meet short-term earnings goals because they alleviate pressures for myopic investment behavior due to their large and long-term holdings. To the extent that dedicated institutions' large stake and long-term perspective lead to higher incentives to monitor managers, we expect that the treatment effect is more pronounced when the ownership of dedicated institutions is higher. To test this prediction, we follow Bushee (1998), calculate the ownership of dedicated institutions for each firm and divide our sample into three subgroups based the dedicated institution ownership. We estimate our baseline regression in Table II separately for the three subsamples and report the results of top and bottom tercile group in Panel B of Table X. We find that the tax effect on executive delta and vega is more evident with firms with higher dedicated institution ownership. The coefficients on *Tax increase (%)* in column (1) and (2) are positive and significant at 1 % level for high dedicated institution ownership subsample and their coefficient are insignificant for low dedicated institution ownership subsample. *P*-values in column (5) and (6) show that the differences of *Tax Increase (%)* between the top and bottom terciles are statistically significant.

Next, we use the proportion of independent directors on the board to measure the effective board monitoring. Previous studies show that the independence of board plays a role in monitoring managers (e.g., Weisbach (1988)). We expect the impact of corporate income tax is more pronounced when the ratio of independent directors on the board is higher because a well-governed board may better react and change managerial compensation aftermath of tax increases. To test how board monitoring affects the tax effect on executive vega, we split our sample into three subgroups based the proportion of independent director on the board and estimate *Equation (1)* for each subsample. Panel C of Table X. Our results show suggestive evidence that independent boards strengthen the relation between corporate tax and executive compensation. We find that the effect of *Tax increase (%)* on the executive delta is more pronounced in high independent director ratio subsample, and its impact on executive vega, in terms of magnitude, is higher for high subgroup, although the difference across subsamples is not statistically significant at the conventional levels.

VII. Effects of tax changes and executive delta and vega on firm risk and performance

The important implications of our arguments are that corporate taxes decrease firms' cash flow and managerial risk-taking behavior, which incentivizes shareholders (boards) to increase equity-based incentives. In this section, we test these implications by examining subsequent changes in firm risk and performance after the tax changes.

We first measure firm risk and performance in year $t + 1$ using several measures total risk, idiosyncratic risk, ROA and pre-tax income. We split the sample into two subgroups according to whether the change in both executive delta and vega are increased in year t, where year t is defined as the next one fiscal year after a tax change.[11] Next, for each subsample, we perform a test by regressing firm risk and performance measured in year *t+1* on tax changes in year *t-1*. This empirical approach presumes that tax changes affect a subsequent managerial compensation structure, which in turn leads to a subsequent change in firm risk and performance. Given the adverse effect of corporate taxes on firm risk and investments (e.g., Ljungqvist, Zhang, and Zuo (2017), Mukherjee, Singh, and Žaldokas (2017)), we expect the negative impacts of the tax rise on firm risk and performance to be mitigated for firms that provide higher risk-taking incentives.

Table XI presents the results. In column (1) of Panel A and B, the results from full sample analysis suggest that the declining effect of corporate income taxes on firm risk persists over two years: the coefficient on *Tax increase (%)* is negative and significant at below 5% level. Next, we examine whether the negative impact of corporate income taxes on firm risk depends on whether firms raise their executive delta and vega. Subsample analyses show that the coefficients on *Tax increase (%)* are insignificant for firms that experience the increases in both executive delta and vega in year t, while those on *Tax increase (%)* are negative and significant for subsamples of firms in which either delta or vega are decreased in year

[11] We take the average of executives' delta and vega in each firm-year. Using total pay weighted average produces the similar results.

t. These results suggest that increased equity-based compensation plays a role in mitigating the adverse effect of tax increases on firm risk documented in the prior literature (Ljungqvist, Zhang, and Zuo (2017)).

In Panel C and D, we also examine whether the change in managerial compensation is associated with the relationship between corporate taxes and operating performance. Existing tax studies have focused on the role of tax benefits of debt in firm value and its impact on firm value is inconclusive. Masulis (1980) uses exchange offers and finds that leverage-increasing exchange offers increases equity value. Fama and French (1998) regress firm value with debt on debt interest and the coefficient on debt interest is either insignificant or negative suggesting that debt tax benefits are not the first-order effect on firm value. Graham (2000) estimates that the tax benefit of debt equals approximately 9%-10% of firm value. Given that corporate taxes affects firm value through not only capital structure but also various corporate policies such as cash holdings and investments, the overall effect of taxes on firm performance is ambiguous.

We use our staggered state-level changes and examine whether the state corporate income taxes cause a significant subsequent change in profitability. Our empirical results show that the corporate income taxes does not change operating performance. The coefficients on *Tax increase (%)* in column (1) of Panel C and Panel D are not significantly different from zero. In column (2) and (3), we find that firms experience performance improvements following tax increases only when they increase executive delta and vega. The coefficients on *Tax increase (%)* are positive and significant in a subsample of firms in which both delta and vega increase in year t, while they are insignificant in a subsample of firms with the decrease in executive delta and vega in year t.

Overall, our findings in this section show that the increased executive vega after tax increases mitigates the negative impacts of tax increases on risk and improves firms' subsequent cash flows. These results confirm the positive relation between equity-based compensation and managerial risk-taking shown in prior studies (Guay (1999), Coles, Daniel, and Naveen (2006), Low (2009), Chava and Purnanandam, (2010), Gormley, Matsa, and Milbourn (2013), Ellul, Wang, and Zhang (2017)). Also, we provide suggestive evidence that managers put their effort to maximize the size of the total pie (i.e., profitability) when they receive more risk-taking compensation after tax increase, which is desirable outcomes for shareholders.

VIII. Robustness tests

To check the robustness of our results, we conduct several additional tests.

First, some states reduce corporate income taxes soon after tax increases. To ensure that subsequent tax cuts do not drive our results, we restrict our sample in which tax increases with no tax cuts in the 2 to 3 years after. Table XII reports the results for regressing the change in *Log (1+Delta)* and *Log (1+Vega)* on tax increases with no subsequent tax cuts and control variables in column (3) of Table II. We restrict tax increases with no tax cuts in the two years in column (1) and (2). We find that the coefficients on *Tax increase (%)* are statistically significant at or below 5% level and the magnitude of coefficients is similar to the results in Table II. We additionally test tax increase without tax cuts in the three years in column (3) and (4). We find qualitatively similar results. These results suggest that the impact of tax increases is robust to the subsequent tax cuts.

Second, we investigate whether the million-dollar rule, which limited the corporate deductibility of non-performance-related executive compensation to $1 million, lead to our results. Murphy (2013) argue that although the objective of the new million-dollar rule (IRS rule Section 162(m)) was to reduce excessive CEO pay levels by limiting deductibility, the result was a significant increase in CEO pay because Section 162(m) encouraged companies to grant more stock options. Perry and Zenner (2001) find that the pay for performance sensitivity has increased for firms likely to be affected by the million-dollar rule. To alleviate this concern that our results are driven by executives who receive more than one million dollar salaries, we divide our sample into two subgroups based on whether executives' salary is above or below one million dollars. The results are presented in Table XIII. We find that executives in both subsamples experience the increase in their vega after tax increases. We also find that the impact of corporate taxes on executive delta varies across the executive's cash salary. The tax effect of delta become insignificant for executives who receive more than one million dollar, which is opposite to million rule prediction. To the extent that the coefficients on *Tax increase (%)* in the below one million dollar subsample remain statistically significant at the conventional level, these results suggest that corporate income taxes affect the risk-taking incentives of top executives who are not subject to the million-dollar regulation.

In our baseline analyses, we retain five executives at each firm-year observations. Previous results show that both CEOs and non-CEO executives experience an increase in their delta and vega after tax increases. For robustness, we additionally investigate whether the impact of tax increases on executives' delta and vega at a firm level. In Table XIV, we take (total pay weighted) average of executives' delta and vega in each firm and analyze the change in averaged executive delta and vega after tax changes. Our results are consistent with previous executive level analysis. The coefficients on *Tax Increase (%)* across all columns are positive and statistically significant at below 10 % level. These results indicate that corporate income taxes affect in firm's overall managerial risk-taking incentives, echoing the results in Table VI.

IX. Conclusion

In this paper, we examine the impact of corporate income taxes on managerial compensation. The principal-agent model predicts that the executive's pay-performance sensitivity is increasing in corporate income taxes because corporate taxes reduce the variance of the firm's performance. Besides, corporate tax declines the after-tax profits available to managers and thus reduces the motivation to exert effort. In response to the effect of the corporate tax on the decline in the variance of the performance and the reduced managerial effort, shareholders should provide more equity-based compensation to maximize returns to them.

Consistent with the prediction, we find that firms respond to tax increases by increasing the sensitivity of executives' wealth to stock price (delta) as well as the sensitivity of executives' wealth to stock return volatility (vega). Our findings are robust to controlling for pre-treatment trends, unobserved local economic conditions, and anticipation effects. We find evidence that the increase in corporate tax rate increases the value of their firm-related wealth. We also find that managers immediately receive a higher proportion of equity-based compensation to total pay when the tax increases become effective, providing additional evidence on the role of equity-based compensation in response to corporate income tax.

In cross-sectional tests, we find that the effect of tax changes on executive vega is greater in subsamples in which CEO stock holdings to shares outstanding is low. We also find that the corporate tax effect on executive vega is more pronounced when they are older. These findings indicate that shareholders concern about managers' myopic behaviors to avoid risk-taking in response to tax shocks and provide more equity-based compensation to align managers' interest with theirs. We further provide evidence that our results are also more pronounced when firms are under a higher level of supervision from institutional investors.

Finally, we investigate whether the higher sensitivities to stock return volatility after the changes in tax rates effectively induce managerial risk-taking policies. We find that firms whose executives experience the increase in their delta and vega tend to take more risk and improve operating performance after the effects of tax changes.

Overall, our findings suggest that shareholders consider the demotivated managerial effort and reduced performance variance caused by corporate income tax, factor them into the design of compensation contracts, and try to mitigate the adverse impact of taxes on their firm value.

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Table I
Summary Statistics

This table reports summary statistics for 68,703 executive-year observations covered in Compustat, CRSP, and ExecuComp databases from 1992 to 2015. The Appendix provides detailed descriptions of construction of the variables. We exclude firms in financial and utility industries (SIC code 6000-6999 and 4900-4999). All financial variables are winsorized at 1% on both sides of the distribution.

Variable	N	Mean	Std Dev	Lower Quartile	Median	Upper Quartile
Executive compensation						
Total compensation ($ thousand)	68,703	2,645	5,420	688	1,350	2,855
Option awards ($ thousand)	68,703	812	3,837	0	193	696
Stock awards ($ thousand)	68,703	648	2,429	0	0	527
Salary and bonus ($ thousand)	68,703	700	912	338	500	800
Delta ($ thousand)	68,703	381	6,124	22	63	187
Vega ($ thousand)	68,703	63	182	5	18	54
Firm characteristics						
Total assets ($ million)	68,703	6,272	27,426	412	1,131	3,586
Leverage	68,703	0.210	0.195	0.043	0.193	0.317
Cash	68,703	0.155	0.173	0.027	0.089	0.222
ROA	68,703	0.147	0.113	0.099	0.144	0.196
Tobin's q	68,703	2.155	1.664	1.277	1.683	2.435
Stock return	68,703	0.224	0.739	-0.103	0.129	0.389
Stock return volatility	68,703	0.429	0.207	0.284	0.381	0.522
R&D	68,703	0.035	0.065	0.000	0.005	0.046
Capital expenditure	68,703	0.059	0.058	0.023	0.042	0.074
Marginal tax rate (%)	68,703	20.215	15.759	2.223	31.415	35.000
State Characteristics						
Tax increase (%)	22	1.036	1.038	0.290	0.875	1.300
Tax decrease (%)	64	0.540	0.398	0.250	0.500	0.945
GDP growth (%)	1,018	4.864	3.231	3.140	4.715	6.696
Unemployment (%)	1,018	5.828	1.934	4.400	5.400	6.900
Wage tax (%)	1,018	5.315	2.985	3.400	5.990	7.110
Cap gain tax (%)	1,018	5.129	2.944	3.900	5.340	7.110
Loss carryback period increase (indicator)	1,018	0.010	0.099	0.000	0.000	0.000
Loss carryback period decrease (indicator)	1,018	0.029	0.169	0.000	0.000	0.000
Loss carryforward period increase (indicator)	1,018	0.038	0.192	0.000	0.000	0.000
Loss carryforward period decrease (indicator)	1,018	0.007	0.083	0.000	0.000	0.000

Table II
Effects of Tax Changes on Delta and Vega

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta)* and *Log (1+Vega)*. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. The sample consists of 68,703 executive-year observations covered in Compustat, CRSP, and ExecuComp databases from 1992 to 2015. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit standard industry classification (SIC) codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

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Independent variables	ΔLog(1+Delta) (1)	Δ Log (1+Vega) (2)	Δ Log (1+Delta) (3)	Δ Log (1+Vega) (4)
Tax increase (%)	0.055**	0.054***	0.053**	0.052***
	(0.021)	(0.004)	(0.040)	(0.008)
Tax decrease (%)	0.011	-0.012	0.011	-0.012
	(0.518)	(0.400)	(0.555)	(0.464)
ΔLog(asset)	-0.293***	0.005	-0.291***	0.006
	(0.000)	(0.846)	(0.000)	(0.811)
ΔLeverage	0.124	0.068	0.121	0.066
	(0.119)	(0.166)	(0.124)	(0.186)
ΔCash	0.202***	0.076*	0.202***	0.077**
	(0.001)	(0.054)	(0.001)	(0.049)
ΔROA	0.266***	0.217***	0.269***	0.217***
	(0.007)	(0.009)	(0.006)	(0.009)
ΔTobin's q	-0.058***	0.018***	-0.056***	0.019***
	(0.000)	(0.006)	(0.000)	(0.005)
ΔStock return	0.022***	-0.001	0.020***	-0.002
	(0.002)	(0.934)	(0.008)	(0.788)
ΔStock return volatility	0.162***	0.134***	0.184***	0.146***
	(0.000)	(0.000)	(0.000)	(0.000)
ΔR&D	0.154	0.197	0.148	0.201
	(0.685)	(0.395)	(0.702)	(0.378)
ΔCapital expenditure	-0.293*	-0.036	-0.286*	-0.031
	(0.059)	(0.781)	(0.072)	(0.812)
ΔMarginal tax rate	0.031	0.032	0.032	0.032
	(0.355)	(0.511)	(0.337)	(0.505)
ΔGDP growth			-0.001	-0.000
			(0.386)	(0.718)
ΔUnemploy Rate			0.024***	0.011**
			(0.000)	(0.025)
ΔWage tax rate			0.009	-0.007
			(0.306)	(0.393)
ΔCapital Gain tax rate			0.003	0.012*
			(0.676)	(0.099)
Loss carryback period increase (indicator)			0.026	0.082
			(0.756)	(0.233)
Loss carryback period decrease (indicator)			-0.039	0.007
			(0.354)	(0.762)
Loss carryforward period increase (indicator)			0.010	-0.014
			(0.610)	(0.422)
Loss carryforward period decrease (indicator)			0.066***	0.042***
			(0.000)	(0.001)
Industry-year fixed effects	Yes	Yes	Yes	Yes
Observations	68,703	68,703	68,703	68,703
Adjusted R-squared	0.212	0.095	0.213	0.096

Table III.
Effects of Tax Changes on Delta and Vega:
Pre-trends and Treatment Reversal

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta)* and *Log (1+Vega)*. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. The sample consists of 68,703 executive-year observations covered in Compustat, CRSP, and ExecuComp databases from 1992 to 2015. *Tax increase (%) t* (*Tax decrease (%) t*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in a given fiscal year *t*. We include two lag and three lead terms of *Tax increase (%) t* (*Tax decrease (%) t)* to investigate pre-trends and treatment reversals. *Tax increase (%) t-2* is the two-year lagged value of *Tax increase (%) t*; *Tax increase (%) t-1* is the one-year lagged value of *Tax increase (%) t*; *Tax increase (%) t+1* is the one-year forward value of *Tax increase (%) t*; *Tax increase (%) t+2* is the two-year forward value of *Tax increase (%) t; Tax increase (%) t+3* is the three-year forward value of *Tax increase (%) t*. The two lag and three lead terms of *Tax decrease (%) t* are defined as same as *Tax increase (%) t*. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

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Independent variables	Δ Log (1+Delta) (1)	Δ Log (1+Vega) (2)
Tax increase (%) t-2	-0.005	0.001
	(0.518)	(0.942)
Tax increase (%) t-1	-0.001	0.010
	(0.961)	(0.452)
Tax increase (%) t	0.065**	0.063**
	(0.020)	(0.014)
Tax increase (%) t+1	-0.027	-0.040
	(0.319)	(0.282)
Tax increase (%) t+2	0.015	0.007
	(0.117)	(0.662)
Tax increase (%) t+3	-0.006	0.016
	(0.786)	(0.221)
Tax decrease (%) t-2	-0.025	-0.011
	(0.147)	(0.487)
Tax decrease (%) t-1	0.006	-0.028
	(0.885)	(0.560)
Tax decrease (%) t	0.016	-0.005
	(0.585)	(0.867)
Tax decrease (%) t+1	0.006	0.029*
	(0.763)	(0.098)
Tax decrease (%) t+2	0.025	0.032*
	(0.188)	(0.071)
Tax decrease (%) t+3	-0.001	-0.003
	(0.962)	(0.898)
ΔLog(asset)	-0.295***	0.007
	(0.000)	(0.781)
ΔLeverage	0.153*	0.086
	(0.068)	(0.117)
ΔCash	0.167***	0.045
	(0.008)	(0.341)
ΔROA	0.257***	0.184***
	(0.006)	(0.007)
ΔTobin's q	-0.052***	0.021***
	(0.000)	(0.001)
ΔStock return	0.019***	-0.001
	(0.007)	(0.886)
ΔStock return volatility	0.184***	0.153***
	(0.000)	(0.000)
ΔR&D	0.036	0.080
	(0.923)	(0.759)

ΔCapital expenditure	-0.343**	-0.086
	(0.031)	(0.537)
ΔMarginal tax rate	0.043	0.050
	(0.148)	(0.255)
ΔGDP growth	-0.000	-0.000
	(0.833)	(0.778)
ΔUnemploy Rate	0.024***	0.011**
	(0.000)	(0.040)
ΔWage tax rate	0.014	-0.009
	(0.188)	(0.199)
ΔCapital Gain tax rate	0.002	0.011*
	(0.890)	(0.058)
Loss carryback period increase (indicator)	0.035	0.091
	(0.684)	(0.164)
Loss carryback period decrease (indicator)	-0.009	0.044**
	(0.831)	(0.039)
Loss carryforward period increase (indicator)	0.009	-0.013
	(0.625)	(0.428)
Loss carryforward period decrease (indicator)	0.078***	0.046***
	(0.000)	(0.000)
Industry-year fixed effects	Yes	Yes
Observations	62,500	62,500
Adjusted R-squared	0.226	0.101

Table IV.
Effects of Tax Changes on Delta and Vega:
Neighboring State Tests

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta)* and *Log (1+Vega)*. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. We restrict control firms located the neighbor of treated states (i.e., a state that experience tax increase or tax cut). After excluding non-neighboring control firms, the sample consists of 37,195 executive-year observations. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Independent variables	Δ Log (1+Delta) (1)	Δ Log (1+Vega) (2)
Tax increase (%)	0.054**	0.040*
	(0.041)	(0.099)
Tax decrease (%)	0.013	0.003
	(0.518)	(0.848)
ΔLog(asset)	-0.329***	0.026
	(0.000)	(0.341)
ΔLeverage	0.246***	0.061
	(0.000)	(0.496)
ΔCash	0.382***	0.302***
	(0.000)	(0.000)
ΔROA	0.193	0.100
	(0.168)	(0.497)
ΔTobin's q	-0.049***	0.028***
	(0.000)	(0.000)
ΔStock return	0.026**	0.003
	(0.041)	(0.821)
ΔStock return volatility	0.228***	0.277***
	(0.001)	(0.000)
ΔR&D	0.755	0.440
	(0.242)	(0.271)
ΔCapital expenditure	0.173	0.061
	(0.489)	(0.775)
ΔMarginal tax rate	0.099**	0.071
	(0.010)	(0.188)
ΔGDP growth	-0.004	-0.003*
	(0.143)	(0.072)
ΔUnemploy Rate	0.017	0.003
	(0.159)	(0.807)
ΔWage tax rate	0.003	-0.002
	(0.797)	(0.931)
ΔCapital Gain tax rate	0.011	0.040**
	(0.169)	(0.015)
Loss carryback period increase (indicator)	0.136**	0.156***
	(0.026)	(0.003)
Loss carryback period decrease (indicator)	0.068	-0.012
	(0.182)	(0.744)
Loss carryforward period increase (indicator)	0.023	0.004
	(0.257)	(0.826)
Loss carryforward period decrease (indicator)	0.061	-0.063*
	(0.184)	(0.054)
Industry-year fixed effects	Yes	Yes
Observations	34,959	34,959
Adjusted R-squared	0.262	0.147

Table V.
Effects of Tax Changes on Delta and Vega:
Anticipation Effects

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta)* and *Log (1+Vega)*. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. We exclude firms headquartered in states whose tax rate changes are likely to be anticipated. In column (1) and (2), we follow Ljungqvist and Smolyansky (2016) and exclude firms headquartered in New England (i.e., Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island or Vermont). In column (3) and (4), we exclude firms headquartered in Colorado, Connecticut, Minnesota, or New York, following Heider and Ljungqvist (2015). *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Exclude CT, ME, MA, NH, RI, VT		Exclude CO, CT, MN, NY	
	Δ Log (1+Delta)	Δ Log (1+Vega)	Δ Log (1+Delta)	Δ Log (1+Vega)
Independent variables	(1)	(2)	(3)	(4)
Tax increase (%)	0.056**	0.054**	0.059**	0.062***
	(0.044)	(0.011)	(0.034)	(0.003)
Tax decrease (%)	0.026	-0.022	0.031	-0.001
	(0.425)	(0.332)	(0.175)	(0.947)
ΔLog(asset)	-0.285***	0.008	-0.297***	-0.001
	(0.000)	(0.755)	(0.000)	(0.973)
ΔLeverage	0.117	0.067	0.113	0.042
	(0.188)	(0.222)	(0.224)	(0.452)
ΔCash	0.164***	0.045	0.201***	0.093**
	(0.004)	(0.214)	(0.005)	(0.035)
ΔROA	0.330***	0.254***	0.226**	0.191**
	(0.001)	(0.005)	(0.033)	(0.034)
ΔTobin's q	-0.061***	0.014**	-0.059***	0.019***
	(0.000)	(0.022)	(0.000)	(0.005)
ΔStock return	0.019**	-0.003	0.017**	-0.003
	(0.020)	(0.665)	(0.035)	(0.683)
ΔStock return volatility	0.175***	0.127***	0.170***	0.130***
	(0.000)	(0.000)	(0.000)	(0.001)
ΔR&D	0.039	0.097	-0.052	0.091
	(0.923)	(0.696)	(0.891)	(0.700)
ΔCapital expenditure	-0.299*	-0.054	-0.340*	-0.072
	(0.077)	(0.700)	(0.058)	(0.592)
ΔMarginal tax rate	0.030	0.036	0.020	0.012
	(0.412)	(0.506)	(0.565)	(0.812)
ΔGDP growth	-0.000	-0.000	-0.001	0.000
	(0.824)	(0.980)	(0.421)	(0.872)
ΔUnemploy Rate	0.026***	0.013**	0.023***	0.012**
	(0.000)	(0.017)	(0.001)	(0.034)
ΔWage tax rate	0.026***	-0.011	0.020*	-0.017*
	(0.009)	(0.183)	(0.050)	(0.053)
ΔCapital Gain tax rate	-0.014**	0.015**	-0.002	0.018***
	(0.017)	(0.041)	(0.826)	(0.002)
Loss carryback period increase (indicator)	0.017	0.094	-0.171***	-0.065
	(0.839)	(0.143)	(0.007)	(0.379)
Loss carryback period decrease (indicator)	-0.038	0.014	-0.068	0.017
	(0.392)	(0.593)	(0.174)	(0.587)
Loss carryforward period increase (indicator)	0.009	-0.022	0.012	-0.019
	(0.664)	(0.223)	(0.558)	(0.363)
Loss carryforward period decrease (indicator)	0.056***	0.044***	0.074***	0.036***
	(0.000)	(0.002)	(0.000)	(0.001)
Industry-year fixed effects	Yes	Yes	Yes	Yes
Observations	62,512	62,512	58,987	58,987

Adjusted R-squared	0.218	0.097	0.218	0.098

Table VI.
Effects of Tax Changes on CEOs' and Non-CEO Executives' Delta and Vega

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta)* and *Log (1+Vega)* of CEO and non-CEO executive. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. The dependent variables in column (1) and (2) are the change in *Log (1+Delta)* and *Log (1+Vega)* of CEO and the dependent variables in column (3) and (4) are the change in *Log (1+Delta)* and *Log (1+Vega)* of top 5 executives excluding CEO. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. Standard errors are clustered at the state level. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	CEO		Non-CEO executive	
	Δ Log (1+Delta)	Δ Log (1+Vega)	Δ Log (1+Delta)	Δ Log (1+Vega)
Independent variables	(1)	(2)	(3)	(4)
Tax increase (%)	0.043**	0.042**	0.056**	0.056***
	(0.046)	(0.040)	(0.041)	(0.005)
Tax decrease (%)	0.013	-0.040**	0.010	-0.003
	(0.503)	(0.019)	(0.632)	(0.882)
ΔLog(asset)	-0.351***	-0.037	-0.271***	0.020
	(0.000)	(0.198)	(0.000)	(0.438)
ΔLeverage	0.140	0.054	0.116	0.073
	(0.104)	(0.477)	(0.143)	(0.180)
ΔCash	0.199***	0.042	0.202***	0.089**
	(0.001)	(0.551)	(0.002)	(0.019)
ΔROA	0.195*	0.132	0.295***	0.248***
	(0.063)	(0.116)	(0.006)	(0.007)
ΔTobin's q	-0.051***	0.020*	-0.058***	0.018***
	(0.000)	(0.054)	(0.000)	(0.003)
ΔStock return	0.023***	0.007	0.018**	-0.005
	(0.002)	(0.465)	(0.020)	(0.482)
ΔStock return volatility	0.218***	0.182***	0.174***	0.134***
	(0.000)	(0.000)	(0.000)	(0.000)
ΔR&D	0.064	0.125	0.181	0.220
	(0.896)	(0.717)	(0.628)	(0.315)
ΔCapital expenditure	-0.403***	0.028	-0.250	-0.049
	(0.006)	(0.870)	(0.146)	(0.712)
ΔMarginal tax rate	0.045	0.014	0.028	0.039
	(0.237)	(0.732)	(0.444)	(0.468)
ΔGDP growth	-0.000	-0.001	-0.001	-0.000
	(0.873)	(0.496)	(0.317)	(0.857)
ΔUnemploy Rate	0.029***	0.014**	0.022***	0.010*
	(0.000)	(0.019)	(0.001)	(0.063)
ΔWage tax rate	0.016*	-0.006	0.007	-0.007
	(0.075)	(0.646)	(0.435)	(0.362)
ΔCapital Gain tax rate	-0.002	0.009	0.005	0.012*
	(0.781)	(0.460)	(0.552)	(0.073)
Loss carryback period increase (indicator)	0.040	0.041	0.019	0.095
	(0.711)	(0.717)	(0.802)	(0.128)
Loss carryback period decrease (indicator)	-0.029	0.034	-0.043	-0.002
	(0.396)	(0.248)	(0.352)	(0.954)
Loss carryforward period increase (indicator)	0.001	-0.019	0.012	-0.013
	(0.936)	(0.332)	(0.566)	(0.471)
Loss carryforward period decrease (indicator)	0.044**	0.032	0.073***	0.044***
	(0.030)	(0.261)	(0.000)	(0.000)
Industry-year fixed effects	Yes	Yes	Yes	Yes
Observations	16,759	16,759	51,944	51,944
Adjusted R-squared	0.202	0.070	0.207	0.095

Table VII.
Effects of Tax Changes on Firm-specific Wealth

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variable is *Log (1+Wealth)*. *Wealth* is the value of the executives' stock and option portfolio. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Independent variables	Δ Log (1+Wealth) (1)	Δ Log (1+Wealth) (2)
Tax increase (%)	0.049**	0.046*
	(0.039)	(0.073)
Tax decrease (%)	0.007	0.007
	(0.718)	(0.730)
ΔLog(asset)	-0.335***	-0.333***
	(0.000)	(0.000)
ΔLeverage	0.139	0.137
	(0.109)	(0.113)
ΔCash	0.239***	0.239***
	(0.001)	(0.001)
ΔROA	0.343***	0.346***
	(0.002)	(0.002)
ΔTobin's q	-0.068***	-0.067***
	(0.000)	(0.000)
ΔStock return	0.026***	0.024***
	(0.001)	(0.004)
ΔStock return volatility	0.233***	0.258***
	(0.000)	(0.000)
ΔR&D	0.161	0.154
	(0.710)	(0.730)
ΔCapital expenditure	-0.310**	-0.302*
	(0.047)	(0.058)
ΔMarginal tax rate	0.028	0.030
	(0.459)	(0.440)
ΔGDP growth		-0.001
		(0.310)
ΔUnemploy Rate		0.027***
		(0.000)
ΔWage tax rate		0.010
		(0.322)
ΔCapital Gain tax rate		0.004
		(0.679)
Loss carryback period increase (indicator)		0.021
		(0.801)
Loss carryback period decrease (indicator)		-0.050
		(0.300)
Loss carryforward period increase (indicator)		0.015
		(0.482)
Loss carryforward period decrease (indicator)		0.072***
		(0.000)
Industry-year fixed effects	Yes	Yes
Observations	68,703	68,703
Adjusted R-squared	0.211	0.212

Table VIII.
Effects of Tax Changes on Compensation Structure

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in the compensation structure of executives. The dependent variable in column (1) and (2) is the change in the value of option awards (*option_awards_blk_value* before FAS 123R and *options_awards_fv* afterward) divided by total pay (*tdc1*). The dependent variable in column (3) and (4) is the change in the value of option awards plus the value of stock awards (*rstkgrnt* before FAS 123R and *stock_awards_fv* afterward) divided by total pay. The dependent variable in column (5) and (6) is the change in the value of option awards divided by the value of stock awards. Our specification is as follow:

$$\Delta(Compensation\ Structure)_{i,s,t-1+k} = \beta_1\ Tax\ increase\ (\%)_{st-1} + \beta_2 Tax\ decrease\ (\%)_{st-1} + \beta_3 \Delta X_{it-1} + \Delta Z_{st-1} + \alpha_{jt} + \varepsilon_{i,s,t+k}$$

where $i, s, j, t-1+k$ indicate firm, state, industry, and years with *k=0* to 1. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	$\Delta\frac{Option}{Total\ Pay}$		$\Delta\frac{Option + Stock}{Total\ Pay}$		$\Delta\frac{Option}{Stock}$	
	k=0	*k=1*	*k=0*	*k=1*	*k=0*	*k=1*
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Tax increase (%)	0.008*	0.001	0.008**	0.002	0.114*	-0.027
	(0.071)	(0.833)	(0.044)	(0.674)	(0.068)	(0.442)
Tax decrease (%)	0.007	-0.004	0.008	-0.010	-0.146	-0.275
	(0.295)	(0.614)	(0.296)	(0.358)	(0.390)	(0.194)
ΔLog(asset)	0.040***	0.029***	0.053***	0.051***	0.141	-0.038
	(0.000)	(0.001)	(0.000)	(0.000)	(0.554)	(0.716)
ΔLeverage	-0.085***	-0.008	-0.096***	-0.024	-0.117	0.351
	(0.002)	(0.732)	(0.007)	(0.366)	(0.830)	(0.248)
ΔCash	0.033	0.028	0.020	0.032	-0.854*	0.638**
	(0.173)	(0.160)	(0.501)	(0.229)	(0.067)	(0.036)
ΔROA	-0.139***	0.138***	-0.199***	0.259***	1.369	0.006
	(0.001)	(0.000)	(0.000)	(0.000)	(0.153)	(0.989)
ΔTobin's q	0.011***	0.012***	0.012***	0.010**	-0.034	0.124
	(0.000)	(0.004)	(0.000)	(0.047)	(0.690)	(0.109)
ΔStock return	-0.019***	0.006**	-0.024***	0.011***	0.068	-0.053
	(0.000)	(0.026)	(0.000)	(0.002)	(0.255)	(0.219)
ΔStock return volatility	0.043**	-0.010	0.023	-0.035**	0.181	0.401*
	(0.020)	(0.546)	(0.218)	(0.021)	(0.345)	(0.088)
ΔR&D	-0.209**	-0.088	-0.217**	0.045	2.431	-4.095***
	(0.021)	(0.504)	(0.012)	(0.682)	(0.571)	(0.006)
ΔCapital expenditure	0.023	0.080	0.118*	0.026	1.067	-0.799
	(0.681)	(0.186)	(0.087)	(0.656)	(0.433)	(0.424)
ΔMarginal tax rate	0.007	0.025*	-0.000	0.034**	0.043	-0.033
	(0.752)	(0.066)	(0.995)	(0.014)	(0.891)	(0.837)
ΔGDP growth	0.001	0.000	-0.000	0.000	-0.008	0.002
	(0.251)	(0.621)	(0.783)	(0.416)	(0.502)	(0.773)
ΔUnemploy Rate	-0.002	0.000	-0.005*	-0.003	0.018	-0.002
	(0.323)	(0.926)	(0.099)	(0.151)	(0.495)	(0.946)
ΔWage tax rate	-0.002	0.001	0.000	-0.004	-0.019	0.101
	(0.703)	(0.907)	(0.925)	(0.422)	(0.773)	(0.203)
ΔCapital Gain tax rate	-0.003	-0.007	-0.006	-0.001	-0.016	-0.122
	(0.591)	(0.226)	(0.242)	(0.813)	(0.804)	(0.110)
Loss carryback period increase (indicator)	0.017	-0.022	0.028	-0.005	0.197	-0.400
	(0.411)	(0.421)	(0.234)	(0.904)	(0.595)	(0.605)
Loss carryback period decrease (indicator)	-0.012	0.008	-0.011	0.002	-0.385	0.641*
	(0.372)	(0.476)	(0.389)	(0.888)	(0.279)	(0.091)
Loss carryforward period increase (indicator)	-0.006	0.007	-0.000	0.012	0.011	0.316***
	(0.323)	(0.417)	(0.984)	(0.184)	(0.948)	(0.004)

Loss carryforward period decrease (indicator)	0.020**	-0.013*	0.025**	-0.013	1.285	-1.014
	(0.021)	(0.082)	(0.023)	(0.117)	(0.295)	(0.122)
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	54,432	54,421	54,432	54,421	20,373	22,176
Adjusted R-squared	0.049	0.057	0.046	0.054	0.186	0.184

Table IX.
Effects of Tax Changes on Delta and Vega:
Subgroup analyses according to executive characteristics

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta)* and *Log (1+Vega)*. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. The sample consists of 68,703 executive-year observations covered in Compustat, CRSP, and ExecuComp databases from 1992 to 2015. In Panel A (Panel B), we divide the sample into three subgroups according to executives' stock holdings and age and report results for top and bottom tercile subsamples only. In all panels, "High" and "Low" indicate top and bottom tercile samples, respectively. The *F*-statistics and *P*-values (parentheses) of equal tax sensitivity test are reported in column (5) and (6). *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Subsample analyses according to proportion of stock holding

	High		Low		Test of equal tax sensitivity	
	Δ Log (1+Delta)	Δ Log (1+Vega)	Δ Log (1+Delta)	Δ Log (1+Vega)	(1) and (3)	(2) and (4)
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Tax increase (%)	0.029	0.010	0.085**	0.096***	21.66***	32.57***
	(0.180)	(0.682)	(0.023)	(0.001)	(0.000)	(0.000)
Tax decrease (%)	-0.027	-0.069*	-0.006	-0.012	10.81***	17.11***
	(0.333)	(0.076)	(0.853)	(0.604)	(0.002)	(0.000)
Control variables (same as in column (3) of Table II)	Yes	Yes	Yes	Yes		
Industry-year fixed effects	Yes	Yes	Yes	Yes		
Observations	18,522	18,522	18,515	18,515		
Adjusted R-squared	0.228	0.109	0.231	0.111		

Panel B: Subsample analyses according to executive age

	High		Low		Test of equal tax sensitivity	
	Δ Log (1+Delta)	Δ Log (1+Vega)	Δ Log (1+Delta)	Δ Log (1+Vega)	(1) and (3)	(2) and (4)
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Tax increase (%)	0.068	0.068**	0.025	0.020**	0.13	0.00
	(0.108)	(0.046)	(0.185)	(0.015)	(0.721)	(0.952)
Tax decrease (%)	0.015	-0.053**	0.003	0.003	10.71***	23.87***
	(0.531)	(0.049)	(0.950)	(0.925)	(0.002)	(0.000)
Control variables (same as in column (3) of Table II)	Yes	Yes	Yes	Yes		
Industry-year fixed effects	Yes	Yes	Yes	Yes		
Observations	16,459	16,459	18,758	18,758		
Adjusted R-squared	0.206	0.096	0.238	0.107		

Table X.
Effects of Tax Changes on Delta and Vega:
Subgroup analyses according to governance

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta)* and *Log (1+Vega)*. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. The sample consists of 68,703 executive-year observations covered in Compustat, CRSP, and ExecuComp databases from 1992 to 2015. In Panel A (Panel B and Panel C), we divide the sample into three subgroups according to institutional ownership, dedicated institutional shareholder ownership, and independent director ratio, and report results for top and bottom tercile subsamples only. In all panels, "High" and "Low" indicate top and bottom tercile samples, respectively. The *F*-statistics and *P*-values (parentheses) of equal tax sensitivity test are reported in column (5) and (6). *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Subsample analyses according to institutional ownership

	High		Low		Test of equal tax sensitivity	
	Δ Log (1+Delta)	Δ Log (1+Vega)	Δ Log (1+Delta)	Δ Log (1+Vega)	(1) and (3)	(2) and (4)
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Tax increase (%)	0.102***	0.109**	0.067	0.053	1.61	3.77*
	(0.001)	(0.012)	(0.173)	(0.267)	(0.211)	(0.058)
Tax decrease (%)	0.001	0.012	0.067	0.005	0.76	0.24
	(0.968)	(0.692)	(0.108)	(0.896)	(0.388)	(0.629)
Control variables (same as in column (3) of Table II)	Yes	Yes	Yes	Yes		
Industry-year fixed effects	Yes	Yes	Yes	Yes		
Observations	22,268	22,268	22,344	22,344		
Adjusted R-squared	0.274	0.159	0.236	0.132		

Panel B: Subsample analyses according to dedicated institution ownership

	High		Low		Test of equal tax sensitivity	
	Δ Log (1+Delta)	Δ Log (1+Vega)	Δ Log (1+Delta)	Δ Log (1+Vega)	(1) and (3)	(2) and (4)
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Tax increase (%)	0.056***	0.079***	0.030	0.021	8.63***	6.57**
	(0.002)	(0.000)	(0.166)	(0.264)	(0.005)	(0.014)
Tax decrease (%)	-0.017	-0.063*	0.048	0.004	0.68	1.09
	(0.632)	(0.079)	(0.130)	(0.910)	(0.413)	(0.302)
Control variables (same as in column (3) of Table II)	Yes	Yes	Yes	Yes		
Industry-year fixed effects	Yes	Yes	Yes	Yes		
Observations	22,076	22,076	30,504	30,504		
Adjusted R-squared	0.302	0.163	0.218	0.116		

Panel C: Subsample analyses according to independent director ratio

	High		Low		Test of equal tax sensitivity	
	Δ Log (1+Delta)	Δ Log (1+Vega)	Δ Log (1+Delta)	Δ Log (1+Vega)	(1) and (3)	(2) and (4)
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Tax increase (%)	0.050*	0.105***	0.026	0.078**	1.15	0.57
	(0.067)	(0.003)	(0.627)	(0.047)	(0.290)	(0.455)
Tax decrease (%)	0.033	0.023	0.016	-0.022	1.29	3.95*
	(0.489)	(0.617)	(0.749)	(0.658)	(0.263)	(0.053)
Control variables (same as in column (3) of	Yes	Yes	Yes	Yes		

Table II)				
Industry-year fixed effects	Yes	Yes	Yes	Yes
Observations	11,426	11,426	13,678	13,678
Adjusted R-squared	0.299	0.172	0.245	0.163

Table XI.
Tax changes, CEO compensation and firm's risk and performance

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Total risk* (Panel A), *Idio risk* (Panel B), *ROA* (Panel C), and *Pretax income* (Panel D). The sample consists of 21,552 firm-year observations covered in Compustat, CRSP, and ExecuComp databases from 1992 to 2015. We first average executives' *Delta* and *Vega* for each firm-year. We then divide the sample based on whether both averaged *Delta* and *Vega* are increased in *year t*, where *year t* is defined as the next one fiscal year after a tax change. The dependent variable in Panel A is the change in *Total risk*, defined as the natural logarithm of annualized variance of daily stock returns over fiscal year. The dependent variable in Panel B is the change in *Idio risk*, defined as the natural logarithm of annualized variance of the residuals from the market model. We use the CRSP value-weighted return as market index. The dependent variable in Panel C is *ROA*, defined as operating income before depreciation scaled by total assets. The dependent variable in Panel D is *Pretax income*, defined as pretax income scaled by total assets. In all panels, column (1) reports the regression results for full sample and column (2) and (3) present the regression results for subsamples of the change in *Delta* and *Vega*. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Total risk			
	Full sample	$\Delta\text{Delta}_t > 0$ and $\Delta\text{Vega}_t > 0$	$\Delta\text{Delta}_t \leq 0$ or $\Delta\text{Vega}_t \leq 0$
Independent variables	$\Delta\text{Total risk}_{t+1}$ (1)	$\Delta\text{Total risk}_{t+1}$ (2)	$\Delta\text{Total risk}_{t+1}$ (3)
Tax increase (%)	-0.010**	-0.004	-0.013***
	(0.011)	(0.614)	(0.009)
Tax decrease (%)	0.002	-0.005	0.006
	(0.861)	(0.725)	(0.700)
Control variables (same as in column (3) of Table II)	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes
Observations	21,338	6,688	14,650
Adjusted R-squared	0.433	0.457	0.422

Panel B: Idio risk			
	Full sample	$\Delta\text{Delta}_t > 0$ and $\Delta\text{Vega}_t > 0$	$\Delta\text{Delta}_t \leq 0$ or $\Delta\text{Vega}_t \leq 0$
Independent variables	$\Delta\text{Idio risk}_{t+1}$ (1)	$\Delta\text{Idio risk}_{t+1}$ (2)	$\Delta\text{Idio risk}_{t+1}$ (3)
Tax increase (%)	-0.012***	-0.007	-0.013***
	(0.001)	(0.353)	(0.001)
Tax decrease (%)	-0.001	-0.007	0.002
	(0.933)	(0.552)	(0.873)
Control variables (same as in column (3) of Table II)	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes
Observations	21,338	6,688	14,650
Adjusted R-squared	0.349	0.377	0.335

Panel C: ROA			
	Full sample	$\Delta\text{Delta}_t > 0$ and $\Delta\text{Vega}_t > 0$	$\Delta\text{Delta}_t \leq 0$ or $\Delta\text{Vega}_t \leq 0$
Independent variables	ΔROA_{t+1} (1)	ΔROA_{t+1} (2)	ΔROA_{t+1} (3)
Tax increase (%)	-0.001	0.003**	-0.002*
	(0.593)	(0.027)	(0.056)
Tax decrease (%)	-0.002	-0.002	-0.003
	(0.403)	(0.578)	(0.274)
Control variables (same as in column (3) of Table II)	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes
Observations	21,529	6,684	14,845
Adjusted R-squared	0.116	0.080	0.123

Panel D: Pretax income			
	Full sample	$\Delta \mathrm{Delta}_t > 0$ and $\Delta \mathrm{Vega}_t > 0$	$\Delta \mathrm{Delta}_t \leq 0$ or $\Delta \mathrm{Vega}_t \leq 0$
	ΔPretax income_{t+1}	Pretax income_{t+1}	Pretax income_{t+1}
Independent variables	(1)	(2)	(3)
Tax increase (%)	-0.000	0.004***	-0.003
	(0.849)	(0.002)	(0.196)
Tax decrease (%)	-0.003	-0.005	-0.004
	(0.396)	(0.510)	(0.396)
Control variables (same as in column (3) of Table II)	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes
Observations	21,552	6,690	14,862
Adjusted R-squared	0.129	0.076	0.138

Table XII.
Effects of Tax Changes on Delta and Vega:
Tax increases with no subsequent tax cuts

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta)* and *Log (1+Vega)*. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. We exclude tax increases with no subsequent tax cuts. In column (1) and (2), we exclude firms that experience tax cuts in two years after tax increases. In column (3) and (4), we exclude firms that experience tax cuts in three years after tax increases. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Tax increases with no tax cuts in 2 years		Tax increases with no tax cuts in 3 years	
	Δ Log (1+Delta)	Δ Log (1+Vega)	Δ Log (1+Delta)	Δ Log (1+Vega)
Independent variables	(1)	(2)	(3)	(4)
Tax increase (%)	0.052**	0.052***	0.054**	0.052**
	(0.049)	(0.009)	(0.042)	(0.012)
Tax decrease (%)	0.011	-0.012	0.011	-0.012
	(0.551)	(0.463)	(0.545)	(0.461)
ΔLog(asset)	-0.292***	0.005	-0.292***	0.004
	(0.000)	(0.847)	(0.000)	(0.855)
ΔLeverage	0.124	0.068	0.122	0.068
	(0.116)	(0.171)	(0.121)	(0.171)
ΔCash	0.203***	0.078**	0.203***	0.077**
	(0.001)	(0.049)	(0.001)	(0.049)
ΔROA	0.273***	0.216***	0.273***	0.218***
	(0.005)	(0.009)	(0.005)	(0.008)
ΔTobin's q	-0.057***	0.018***	-0.056***	0.018***
	(0.000)	(0.005)	(0.000)	(0.005)
ΔStock return	0.020***	-0.002	0.020***	-0.002
	(0.007)	(0.780)	(0.007)	(0.787)
ΔStock return volatility	0.185***	0.147***	0.184***	0.147***
	(0.000)	(0.000)	(0.000)	(0.000)
ΔR&D	0.148	0.193	0.152	0.200
	(0.704)	(0.397)	(0.697)	(0.382)
ΔCapital expenditure	-0.282*	-0.032	-0.282*	-0.032
	(0.075)	(0.808)	(0.075)	(0.807)
ΔMarginal tax rate	0.034	0.038	0.032	0.038
	(0.316)	(0.438)	(0.339)	(0.436)
ΔGDP growth	-0.001	-0.000	-0.001	-0.000
	(0.378)	(0.762)	(0.398)	(0.780)
ΔUnemploy Rate	0.023***	0.011**	0.024***	0.011**
	(0.000)	(0.028)	(0.000)	(0.028)
ΔWage tax rate	0.009	-0.007	0.009	-0.007
	(0.302)	(0.370)	(0.288)	(0.370)
ΔCapital Gain tax rate	0.004	0.012	0.003	0.012*
	(0.673)	(0.102)	(0.680)	(0.094)
Loss carryback period increase (indicator)	0.025	0.082	0.025	0.082
	(0.761)	(0.243)	(0.762)	(0.243)
Loss carryback period decrease (indicator)	-0.039	0.006	-0.039	0.006
	(0.353)	(0.802)	(0.353)	(0.804)
Loss carryforward period increase (indicator)	0.011	-0.014	0.011	-0.014
	(0.590)	(0.439)	(0.590)	(0.443)
Loss carryforward period decrease (indicator)	0.066***	0.041***	0.066***	0.041***
	(0.000)	(0.001)	(0.000)	(0.001)
Industry-year fixed effects	Yes	Yes	Yes	Yes
Observations	68,583	68,583	68,527	68,527

Adjusted R-squared	0.213	0.095	0.213	0.096

Table XIII.
Effects of Tax Changes on Vega:
Subgroup analyses according to salary

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta)* and *Log (1+Vega)*. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. The sample consists of 68,703 executive-year observations covered in Compustat, CRSP, and ExecuComp databases from 1992 to 2015. We divide the sample based on whether the executives' salary is above or below $1,000,000. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Salary above $1,000,000		Salary below $1,000,000	
	Δ Log (1+Delta)	Δ Log (1+Vega)	Δ Log (1+Delta)	Δ Log (1+Vega)
Independent variables	(1)	(2)	(3)	(4)
Tax increase (%)	0.084	0.123***	0.051**	0.046***
	(0.136)	(0.007)	(0.029)	(0.009)
Tax decrease (%)	-0.151*	-0.251*	0.016	-0.004
	(0.075)	(0.066)	(0.367)	(0.839)
ΔLog(asset)	-0.241***	-0.087	-0.294***	0.008
	(0.006)	(0.367)	(0.000)	(0.728)
ΔLeverage	-0.090	0.316	0.129	0.061
	(0.640)	(0.336)	(0.110)	(0.214)
ΔCash	0.162	0.252	0.209***	0.074*
	(0.532)	(0.331)	(0.000)	(0.063)
ΔROA	-0.082	0.375	0.273***	0.219***
	(0.792)	(0.461)	(0.006)	(0.006)
ΔTobin's q	0.001	0.025	-0.058***	0.018***
	(0.971)	(0.430)	(0.000)	(0.004)
ΔStock return	0.017	-0.039	0.020***	-0.001
	(0.615)	(0.278)	(0.008)	(0.853)
ΔStock return volatility	0.344***	0.242*	0.180***	0.143***
	(0.006)	(0.055)	(0.000)	(0.000)
ΔR&D	-0.760	-1.257**	0.162	0.225
	(0.379)	(0.036)	(0.679)	(0.326)
ΔCapital expenditure	-0.497	-0.301	-0.272*	-0.024
	(0.411)	(0.711)	(0.095)	(0.858)
ΔMarginal tax rate	0.108	0.161	0.032	0.030
	(0.496)	(0.395)	(0.357)	(0.540)
ΔGDP growth	-0.003	-0.004	-0.001	-0.000
	(0.572)	(0.333)	(0.420)	(0.818)
ΔUnemploy Rate	0.014	0.004	0.024***	0.011**
	(0.307)	(0.787)	(0.000)	(0.039)
ΔWage tax rate	0.053	-0.078**	0.007	-0.006
	(0.205)	(0.030)	(0.458)	(0.394)
ΔCapital Gain tax rate	-0.039	0.076**	0.004	0.011*
	(0.283)	(0.039)	(0.621)	(0.092)
Loss carryback period increase (indicator)	0.065	0.089	0.024	0.079
	(0.664)	(0.630)	(0.772)	(0.264)
Loss carryback period decrease (indicator)	-0.228**	0.021	-0.035	0.011
	(0.012)	(0.891)	(0.411)	(0.637)
Loss carryforward period increase (indicator)	-0.024	0.029	0.009	-0.015
	(0.668)	(0.740)	(0.640)	(0.407)
Loss carryforward period decrease (indicator)	-0.050	-0.190	0.066***	0.041***
	(0.739)	(0.157)	(0.000)	(0.002)
Industry-year fixed effects	Yes	Yes	Yes	Yes
Observations	3,478	3,478	65,225	65,225
Adjusted R-squared	0.317	0.138	0.210	0.094

Table XIV.
Effects of Tax Changes on Vega:
Firm-level analysis

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variables are the change in *Log (1+Delta average)*, *Log (1+Vega average)*, *Log (1+ Delta weighted average)* and *Log (1+ Vega weighted average)*. *Delta* (*Vega*) *average* is the average of top five executives' *Delta (Vega)* for each firm-year and *Delta (Vega) weighted average* is the total pay-weighted average of top five executives' *Delta (Vega)*. *Delta* is the dollar change in wealth associated with a 1% change in the firm's stock price and *Vega* is the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return. The sample consists of 18,333 firm-year observations covered in Compustat, CRSP, and ExecuComp databases from 1992 to 2015. *Tax increase (%)* (*Tax decrease (%)*) is the magnitude (as measured in percentage points) of top marginal corporate income tax increase (decrease) in a firm's headquartered state in given fiscal year. All specifications are estimated using OLS in first differences and include industry (two-digit SIC codes)-year fixed effects. The appendix provides detailed descriptions of the construction of the variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Independent variables	Δ Log (1+Delta average) (1)	Δ Log (1+Vega average) (2)	Δ Log (1+Delta weighted average) (3)	Δ Log (1+Vega weighted average) (4)
Tax increase (%)	0.046*	0.053***	0.043*	0.059***
	(0.062)	(0.001)	(0.088)	(0.000)
Tax decrease (%)	0.005	-0.008	-0.008	-0.030
	(0.864)	(0.712)	(0.779)	(0.212)
ΔLog(asset)	-0.250***	0.064**	-0.281***	0.020
	(0.000)	(0.021)	(0.000)	(0.396)
ΔLeverage	0.109	-0.037	0.081	-0.055
	(0.215)	(0.538)	(0.342)	(0.304)
ΔCash	0.268***	0.063	0.237***	0.059
	(0.000)	(0.268)	(0.000)	(0.335)
ΔROA	0.252**	0.279***	0.187**	0.152*
	(0.011)	(0.002)	(0.045)	(0.076)
ΔTobin's q	-0.038***	0.032***	-0.037**	0.030***
	(0.006)	(0.000)	(0.011)	(0.000)
ΔStock return	0.017*	-0.009	0.023**	-0.007
	(0.065)	(0.275)	(0.010)	(0.331)
ΔStock return volatility	0.226***	0.162***	0.241***	0.127***
	(0.000)	(0.000)	(0.000)	(0.000)
ΔR&D	0.178	0.509**	0.206	0.341
	(0.635)	(0.013)	(0.586)	(0.109)
ΔCapital expenditure	-0.027	-0.059	-0.099	-0.039
	(0.868)	(0.723)	(0.609)	(0.810)
ΔMarginal tax rate	-0.006	0.026	0.011	0.016
	(0.868)	(0.529)	(0.756)	(0.555)
ΔGDP growth	-0.003*	-0.001	-0.003*	-0.001
	(0.098)	(0.521)	(0.066)	(0.355)
ΔUnemploy Rate	0.018**	0.013**	0.015*	0.009*
	(0.017)	(0.018)	(0.065)	(0.092)
ΔWage tax rate	0.016	-0.011	0.024*	-0.002
	(0.272)	(0.289)	(0.093)	(0.812)
ΔCapital Gain tax rate	0.000	0.014	-0.010	0.002
	(0.996)	(0.194)	(0.387)	(0.791)
Loss carryback period increase (indicator)	0.086	0.132*	0.100	0.147**
	(0.338)	(0.070)	(0.382)	(0.032)
Loss carryback period decrease (indicator)	-0.046	-0.015	-0.014	0.007
	(0.397)	(0.545)	(0.789)	(0.712)
Loss carryforward period increase (indicator)	0.000	-0.020	-0.015	-0.025**
	(0.982)	(0.119)	(0.536)	(0.036)
Loss carryforward period decrease (indicator)	0.036	0.046***	0.021	0.030*
	(0.107)	(0.001)	(0.264)	(0.058)
Industry-year fixed effects	Yes	Yes	Yes	Yes

Observations	18,263	18,263	18,263	18,263
Adjusted R-squared	0.160	0.090	0.132	0.061

Appendix

The appendix provides detailed descriptions of all the variables used in the tables.

Variable names	Variable definitions	Source
Firm characteristics		
Capital expenditure	Capital expenditures/ total assets *capx/at*	Compustat
Cash	Cash and short-term investments/ total assets *che/at*	Compustat
Dedicated institution ownership	Percentage of shares outstanding owned by dedicated institutions. We classify a dedicated institutions following the permanent transient /quasi-indexer/dedicated classifications of Bushee (1998).	Institutional (13f) Holdings
Independent director ratio	Proportion of independent directors in a board	RiskMetrics
Institutional blockholder ownership	Percentage of shares outstanding owned by institution blockholders. Institutional blockholders are defined as institutional shareholders that own more than 5% of a firm's equity scaled by the total number of shares outstanding.	Institutional (13f) Holdings
Leverage	(Long-term debt + Debt in current liabilities)/ total assets *(dltt+dlc)/at*	Compustat
Log(assets)	Natural logarithm of total assets Log(*at*)	Compustat
Marginal tax rate	Simulated marginal tax rates provided by John Graham. We use after-interest marginal tax rates (mtraft).	John Graham(http://faculty.fuqua.duke.edu/~jgraham/taxform.html)
R&D	R&D expenditure. Missing values are coded as zero. Max(0,*xrd)/at*	Compustat
Stock return	Buy-and-hold daily stock returns during a fiscal year	CRSP
ROA	Return on asset. Operating income before depreciation/total assets *oibdp/at*	Compustat
Tobin's q	(Market value of equity - book value of equity +total assets)/Total assets (*csho*prcc_f+at-ceq)/at*	Compustat
Stock return volatility	Standard deviation of daily stock returns during a fiscal year	CRSP
Executive characteristics		
CEO tenure	Number of years the CEO has held the office	ExecuComp
Executive age	Executives' age	ExecuComp
Proportion of unvested options to vested options	Unexercised unexercisable options (ExecuComp: *opt_unex_unexer_num*) divided by unexercised exercisable options (ExecuComp: *opt_unex_exer_num*)	ExecuComp
Stock holdings	Executives' stock ownership (ExecuComp: *shrown_excl_opts*) divided by the number of shares outstanding (Compustat: *csho*)	ExecuComp
State characteristics		
Capital gain tax rate	Maximum state tax rate on long-term capital gains	Feenberg and Coutts (1993), http://www.nber.org/~taxsim
Loss carryback period increase	Indicator that takes the value of one if a state increases in the length of tax loss carryback periods, and zero otherwise	Ljungqvist, Zhang, and Zuo (2017)
Loss carryback period decrease	Indicator that takes the value of one if a state decreases in the length of tax loss carryback periods, and zero otherwise	Ljungqvist, Zhang, and Zuo (2017)
Loss carryforward period increase	Indicator that takes the value of one if a state increases in the length of tax loss carryforward periods, and zero otherwise	Ljungqvist, Zhang, and Zuo (2017)
Loss carryforward period decrease	Indicator that takes the value of one if a state decreases in the length of tax loss carryforward periods, and zero otherwise	Ljungqvist, Zhang, and Zuo (2017)
State GDP growth	State-level GDP growth rate	Bureau of Economic Analysis

Tax decrease (%)	Magnitude (as measured in percentage points) of top marginal corporate income tax decrease in a firm's headquartered state in given fiscal year.	Heider and Ljungqvist (2015)
Tax increase (%)	Magnitude (as measured in percentage points) of top marginal corporate income tax increase in a firm's headquartered state in given fiscal year.	Heider and Ljungqvist (2015)
Unemploy rate	State-level unemployment rate	Bureau of Labor Statistics
Wage tax rate	Maximum state tax rate on wage income, estimated for an additional $1,000 of income on an initial $1,500,000 of wage income	Feenberg and Coutts (1993), http://www.nber.org/~taxsim

Essay three

Option listing and cost of bank debt

Option Listing and Cost of Bank Debt

Abstract

We investigate whether the introduction of equity options influences the cost of bank debt. We use the listing of exchange-traded equity options and examine the effect of option listing on the cost of bank debt. We find a significant decline in loan spreads after a firm is listed in option exchange. The declining effect is more pronounced for informationally opaque firms. We also find that listed firms receive better credit rating after listing. Our results suggest that options improve the informational environment and reduce monitoring costs and credit risk faced by banks, in turn, reduce the cost of debt for the borrower.

Keywords: Option listing, Cost of debt, Bank loan, Information asymmetry

JEL Classification: G14, G21, G32

I. Introduction

Equity option markets have grown considerably over the past few decades. The ratio of firms with exchange-listed options on their equities to the entire U.S. firms has risen from less than 10% in 1985 to more than 60% in 2014. Despite widespread equity options in the capital market, there is limited evidence on the link between equity options and corporate financing. Some studies focus on the relation between the equity options and underlying firms' price and valuation and find that options are positively associated with future stock prices and firm value.[1] In this paper, we establish one important channel where options affect firm valuation. Specifically, we examine the effect of option listing on firms' costs of bank debt.

Previous literature suggests that option markets enhance stock prices more efficient. Easley, O'Hara, and Srinivas (1998) argue that options increase stock price efficiency because informed investors can cover more states when the option market is available. Similarly, Cao (1999) argues that investors with private information can trade more efficiently on their information with the options, options improve price informativeness. Pan and Poteshman (2006) show that the volume of options traded contains information about future stock prices. Hu (2017) find that option listing increases informed and uninformed trading hence reduces relative information risk. These studies suggest that the option trading improves the information about firms' prospects.

Given the salient role of options in stock price efficiency, it is ambiguous how the options affect banks' loan pricing decision. We set two competing views that oppositely predict the effect of option listing on the cost of debt financing. First of all, option listing increases the cost of debt because the access to equity option market triggers managerial risk-taking behaviors (risk-taking view). Since stock prices provide more information about the fundamental value of the firm and especially long-term investments, this provides managers with more incentives to engage in value-increasing activities for their shareholders. Blanco and

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[1] For example, Easley, O'Hara, and Srinivas (1998) and Pan and Poteshman (2006) find that the volume of options traded provides information about the future stock prices. Roll, Schwartz, and Subrahmanyam (2009) show that firms with greater option trading volume enjoy higher firm valuation.

Wehrheim (2017) find that firms with more option trading activity generate more patents and patent citations per dollar invested in R&D. Besides, increased price efficiency from option trading serves as a more effective disciplining mechanism, mitigating the agency conflicts between manager and shareholders (Blanco and Garcia (2017)). Gao (2010) uses the availability of the firm's options on option exchange and option trading volume as the proxies for hedging cost. He shows that option trading reduces managerial hedging costs and enhances the manager's ability to bear the risk. These studies suggest that equity options induce managers to increase long-term risky investments and the ability to bear risk. Increased risk-taking behaviors due to options inflate a firm's default risk and thus increase the cost of debt. Therefore, the risk-taking view expects that option listing firms would pay higher loan spreads.

On the other hand, the option listing decreases the cost of debt for firms because option listing reduces the monitoring costs (monitoring advantage view). Many studies show that an option listing increases the informational environment of the firm. Option listing firms are associated with higher analyst coverage, higher institutional ownership, and more new releases (Skinner (1990), Damodaran and Lim (1991), Ho (1993)). After firms are listed in option exchanges, banks profit from the improved informational environment as well as stock price efficiency and save monitoring efforts. Given that monitoring is costly, the option listing provides banks with favorable borrowing conditions. Therefore, we would expect that the option introduction leads to a significant drop in the cost of debt to the extent that option listing declines informational opacity of borrowing firms.

To distinguish these competing views, we exploit the listing in option exchanges because option listing provides reasonable properties for identifying the effect of options on loan spread. First, listing decisions are made within by the exchanges and not by the firm. Stocks are selected for option listing by committees composed of members of the exchange after soliciting feedback from the general membership (Mayhew and Mihov (2004), Gao (2010)). Second, option listing is repeated multiple points in times for multiple groups of observations, which mitigates our concern about a single shock, especially the violation of parallel trend assumption.

Using option listing events, we perform a propensity score matching difference-in-differences test. We examine 1,898 loans issued by 216 pairs of option listing and non-listing firms. We control for firm characteristics, loan characteristics, firm fixed effects, year fixed effects, loan type fixed effects, and loan purpose fixed effects known to affect bank loan spreads. We find that, following the listing in option exchange, loan spread decrease by 13.65%, implying that 30.62 basis points decline in borrowing costs. This result is consistent with the monitoring advantage view.

We test the essential identifying assumption central to a causal interpretation of the difference-in-differences specification: the treated and control firms have similar trends before option listing. We find that there is no trend of declining loan spread before option listing and that the cost of bank debt drops only after option listing. The finding indicates that the option listing impact does not suffer pre-treatment trends.

We examine the cross-sectional heterogeneity in the cost of bank debt to option listing. If options play a role in enhancing banks' monitoring environment, we expect that the effect of option listing on the cost of bank debt is more pronounced when there is a higher level of informational opacity about borrowers. Consistent with our prediction, the decrease in loan spreads is concentrated among firms with high informational opacity. We find that the effect of option listing on loan spreads is more pronounced when a firm has higher analysts' forecasts dispersion, higher absolute value of discretionary accruals, higher R&D expenditures, and lower ownership of institutional investors. Our findings provide supportive evidence on monitoring advantage view that options mitigate information asymmetry between banks and firms and thus reduces the cost of bank debt.

We examine whether managerial risk-taking incentives intensify or mitigate our main results. Risk-taking view predicts that managers can take more risk after their firms are listed in option exchanges since option listing provides managers with risk-taking incentives. Thus the risk taking view expects the effect of option listing on loan spread to be stronger when managers have higher sensitivities to stock price movements and stock volatility (delta and vega), higher stock holdings, and higher proportion of equity-based pay. Contrary to this conjecture, we find that firms with high risk-taking incentives do not experience higher loan spread after the option listing. We also investigate the role of institutional investors in the relationship between option listing and bank loan spreads. Since large and long-term institutional investors (dedicated institutions) are more likely to affect managerial risk taking decisions, the risk taking view predicts that the higher proportion of dedicated institutional ownership amplifies the effect of option listing on loan rates. In a similar vein, the risk taking view expects the increasing option effect on loan spreads becomes weaker for firms with lower transient institutional ownership. However, we find inconsistent results with the prediction of risk taking view.

Additionally, we examine whether the introduction of equity options affect firms' risk taking behavior. If the risk taking view holds, we expect firms' risk taking to be more pronounced after option listing. We use various measures to gauge the impact of options on corporate risk taking: the daily stock return volatility, the volatility of the residuals from a market model, leverage, cash holdings, R&D expenditures, and capital expenditures. We find no significant relation between option listing and any of the corporate risk taking proxies. Again, these results are inconsistent with the risk taking view that option listing induces the manager to take more risk.

Our results are consistent with the monitoring advantage view that options improve the information environment and reduce banks' monitoring costs. To validate our argument that options reduce informational opacity of firms, we further test whether the introduction of option influences borrower's credit rating. Credit rating agencies are important information intermediaries in the market, and they sensitively respond to borrower's informational opacity as well as banks. Based on the monitoring advantage view, we expect that after option listing, a firm experiences better credit rating because of the improved information environment. We find that the option listing leads firms to receive better credit ratings, which is consistent with the monitoring advantage view.

Our paper contributes to the literature in two ways. First, our paper complements the studies that examine the effect of derivatives on the cost of debt. This study provides new evidence that the option listing has a declining impact on the cost of debt. Ashcraft and Santos (2009) find no evidence that the onset of CDS trading lowers the cost of debt. Our study is different in two ways. First, as Ashcraft and Santos (2009) point out, the credit derivatives provide a new and direct hedging mechanism to banks, which results in losing monitoring effectiveness for firms. We provide the positive externality of derivatives by showing that equity option trading increases the overall informational environment of firms and thus leads to a decrease in the cost of debt for corporate borrowers. Further, while Ashcraft and Santos (2009) confine firms with credit derivatives, the results in this paper cover a broader set of corporate borrowing behavior because equity options become more common over time.

Second, our study contributes to the literature on the effect of option listing. This literature shows the relations between option listing and various aspects of underlying firm including price (Conrad (1989)), volume (Bansal, Pruitt, and Wei (1989), Damodaran and Lim (1991)), liquidity (Kumar, Sarin and Shastri (1998)), informational environment (Skinner (1990), Damodaran and Lim (1991), Ho (1993)) , and cost of equity (Naiker, Navissi, and Truong (2013)). We build on this research by showing that the option listing has on impact on debt financing. Our study establishes a new channel through option listing affects firm value. The results are in line with Roll, Schwartz, and Subrahmanyam (2009) findings that stocks with high options volumes have higher information efficiency and enjoy higher valuation. In a closely related paper, Blanco and Garcia (2017) find that an increase in options volume is associated with an increase in bond yield spreads, suggesting that options exacerbate shareholder-debtholder conflicts. Our paper differs from Blanco and Garcia (2017) by focusing on the option listing effect on bank debt pricing, not bond pricing. While the results in Blanco and Garcia (2017) suggests bondholders' wealth from information enhancement seem to be outweighed by the threat of shareholders' expropriation, our results banks benefit from options because of the enhanced informational environment.

The remainder of this paper proceeds as follows. We describe our sample construction and present the summary statistics in Section II. In section III, we examine the determinants of option listing. In section IV, we present the results from the effect of option listing on loan spreads and the cross-sectional tests focusing on the interaction between option listing and various firm and manager characteristics. In section V, we investigate the relation between option listing and credit rating. We conclude the paper in Section VI.

II. Sample

Our sample period is from 1993 to 2014. We obtain data on option listing date from the OptionMetrics that began offering comprehensive coverage of options since 1996. Our sample starts in 1993, three year before the first option listing year in 1996. The firms that have listing date as of January 4, 1996, are excluded because their options are already traded in the option exchanges before 1996. If a firm has more than one listing event during the sample period, we take only the first record. We restrict our sample to firms whose financial data are available in Compustat and Center for Research in Security Prices (CRSP) database. Following Mayhew and Mihov (2004), we define eligible firms for option listing that meet three requirements: (1) the stock is listed on a national exchange; (2) a firm's stock price is over $3; (3) the firm has at least 7 million publicly held shares.

Loan information is from the Loan Pricing Corporation's Dealscan. We use all-in-drawn spread as an empirical proxy for the cost of bank debt. All-in-drawn spread is the additional basis points the borrower pays over the London Interbank Offered Rate (LIBOR) and includes any recurring annual fees paid to the lenders. We merge Dealscan with Compustat using Dealscan-Compustat link file provided by Chava and Roberts (2008). We drop the loans without borrower ID (GVKEY), all-in-drawn spread. We exclude the loans whose borrowers are financial, utility, and public administration firms (Standard Industrial Classification (SIC) codes 4900–4999, 6000-6999, and 9000-9999). We also restrict loans whose borrowers have financial data in Compustat and CRSP. We require a listed firm has at least one loan issued during both three years before and three years after its equity options are traded in exchange. These procedures result in a final sample of 914 option listings.

Table I presents a distribution of 941 option listings by industry and year. We find that the industry that comprises the most substantial proportion is manufacturing (38.86%), followed by service industries (21.49%), wholesale trade and retail trade (15.37%), and mineral and construction (14.37%). We also find that the number of option listings decreases after 2006.

III. Determinants of option listing

We examine which firms are more likely to be listed in options exchange. We use 2,575 firm-year observations covered in Compustat, CRSP, and Dealscan from 1996 to 2014. We estimate the logit regression in which the dependent variable is an indicator that takes the value of one if a firm is listed in option exchange a given year and zero otherwise. Previous studies show that firm size, trading volume, past stock return, stock return volatility, and bid-ask spread are important determinants of option listing (Mayhew and Mihov (2004), Danielsen, Van Ness, and Warr (2007), Hu (2017)). In addition to these variables, we include other firm characteristics that might influence option listing. We include stock return, ROA to account for firm performance. We control for firm's investment opportunities, the ratio of tangible assets to total assets, operating cash flow volatility, and default risk. We control for financial constraints because option exchanges might choose firms with better financial conditions. We use three financial constraint measures: KZ index, WW index, and HP index. We divide firms into three subgroups based on the three financial constraint measures and define firms in top tercile as constrained. Lastly, we include industry fixed effects (Fama-French 48 industries) and year fixed effects.

Table II reports the results. In column (1), we include the logarithm of the market value of equity, the logarithm of trading volume, the volatility of daily stock return over a fiscal year, buy-and-hold daily stock return over a fiscal year, and bid-ask spread. Our results show that firms are more likely to be listed when their size is large and when they have high trading volume. These results confirm the role of firm size and trading volume in option listing (Mayhew and Mihov (2004), Danielsen, Van Ness, and Warr (2007)). We also find the coefficient on stock return is positive and significant at 1 % level, implying that the option listing decision is positively associated with past stock performance. We additionally control for leverage, ROA, Tobin's q, and cash flow volatility that might affect a firm's option listing. We find that higher level of cash flow volatility is negatively associated with the likelihood of listing. In column (3), we examine whether financial constraints affect the option listing decision. We find that the coefficients on financial constraint indicators (KZ financial constraint (indicator), WW financial constraint (indicator), and HP financial constraint (indicator)) are statistically different from zero.[2] Together with the insignificant coefficient on leverage, firms' financial strength does not have a first-order impact on an option listing decision. These results alleviate a concern that option listings convey a positive signal on firms' financial health conditions, which lenders adjust their loan rates accordingly.

IV. Impact of option listings on loan spreads

A. difference-in-differences test using propensity score matching

To estimate the effect of option listing on the cost of bank debt, we use a difference-in-differences test using propensity score matching. We match each listing firm to a firm that has not been listed with the closest propensity score (with a max difference between propensity scores of 0.1). The propensity score is calculated using the logit regression of *Listing*, an indicator that takes the value one if a firm is listed in equity option exchange, and zero otherwise, on the logarithm of market value of equity, the logarithm of trading volume, the buy-and-hold daily stock return over a fiscal year, the volatility of daily stock return over a fiscal year, leverage, ROA, Tobin's q, tangibility, cash flow volatility, and KZ financial constraint (indicator), industry dummies (Fama-French 48 industries), and year dummies. We require both listing and non-listing firms to be in the same industry and the same fiscal year. To fairly compare the change in loan spreads before and after option listing, we require that treatment and control firms have at least one loans

[2] We examine whether multicollinearity among different financial constraint measures reduces the predictive power of the financial constraint on option listing. To check multicollinearity issues, we estimate the logit regression by keeping one financial constraint measure and dropping the other two measures. We find similar results in Table II. The coefficients on the financial constraint indicators are insignificant.

during both three years before and three years after the option listing. Our final propensity score matched sample includes 432 firms (216 listing firms and matched 216 control firms) with 1,898 loans. Panel A of Table III compares firm characteristics between listing firms and matched non-listing firms. We find that none of the firm characteristics are significantly different implying that matched firms share similar characteristics and our matching procedure is successful. Panel B of Table III presents summary statistics of our propensity score matched sample. We winsorize all ratios at the 1st and 99th percentile to mitigate the effect of outliers. The average loan spread is 224 basis points over LIBOR. The loan maturity is 48 months on average. The size of the average loan is $145 million. Loans have an average of 2.23 and 2.49 financial and non-financial covenants. The average market value of equity is $1.008 billion. The average book leverage is 0.322, the average profitability (ROA) is 0.127, and the average Tobin's q is 1.684.

To implement the difference-in-differences analysis, we estimate the following ordinary least squares (OLS) regression model:

$$Log(Spread_{it}) = \alpha + \beta_1 Post \ \times \ Listing + \beta_2 X_{t-1} + \gamma_t + \omega_i + \eta_l + \varphi_p + \varepsilon_{it} \qquad (1)$$

where $Spread_{it}$ is all-in-spread-drawn, which is a rate a borrower pays in basis points over LIBOR or the LIBOR equivalent. *Post* is an indicator that takes the value of one for post-listing period, and zero for pre-listing period, where the post-listing period is defined as the next three year period starting one day after equity options are traded in exchange. *Listing* is an indicator that takes the value one if an indicator that takes the value of one if a firm is listed in the option exchange and zero otherwise. We include firm characteristics used in propensity-score matching which could affect the loan spreads. We control for firm characteristics that might affect loan spreads. Specifically, we include the natural logarithm of the market value of equity, book leverage, ROA, Tobin's q, tangibility, and cash flow volatility. We include stock return, stock return volatility, trading volume, and as control variables following previous studies on the determinants of option listing. We control for loan characteristics that may influence the cost of bank debt. Specifically, we include loan size, maturity, and performance pricing dummy. We also include loan type fixed effects (η_l), loan purpose fixed effects (φ_p), year fixed effects (γ_t), and firm fixed effects (ω_i) in the regression. Lastly, given that option listings occur at the firm level, standard errors are adjusted for heteroskedasticity and clustered at the firm level.

B. Impact of option listing on loan spreads

Table IV presents the regression results from Equation (1). In Column (1), we only include *Post*Listing*, firm fixed effects, year fixed effects, loan type fixed effects and loan purpose fixed effects. The coefficient of *Post*Listing* has a value of -0.168 and is negatively significant. This estimate suggests that banks reduce loan spreads by 18.29% in the aftermath of the option listing, all else equal.

In Column (2), we additionally control for firm characteristics, and in column (3), we add loan characteristics. In all columns, the coefficients on *Listing*Post* are negative and significant at least 5% level. The coefficient on *Post*Listing* in Column (3) indicates a 13.65% drop of loan spreads after the option listing. In term of economic magnitudes, the option listing leads to a drop in loan spreads by 30.62 basis point considering sample average loan spread (224.26 basis point). This figure implies that a borrower with sample average of loan size ($145.869 million) can save $ 476,000 interest payments.

The coefficients of the control variables are broadly consistent with results in prior literature (Graham, Li, and Qiu (2008), Chava, Livdan, and Purnanandam (2009), Valta (2012), Gao, Li, and Ma (2017)). Firms with large size and high profitability have lower loan spreads. Contrary to previous results, the coefficient on cash flow volatility is negative. We interpret this result is due to the high correlation between stock return volatility and cash flow volatility. When we exclude stock return volatility, the coefficient on cash flow volatility become positive and insignificant. We find that loan spreads decrease when loans have a large size, and they have pricing clauses.

C. Pre-treatment trends

An important assumption when using a difference-in-differences approach is the parallel trends assumption — that is, in the absence of option listing, the treatment and control groups would have behaved similarly. To examine the pre-treatment trend assumption, we replace *Post*Listing* with six indicators: *Year t-3, Year t-2, Year t-1, Year t, Year t+1, Year t+2*, where *Year t* is defined as the next one year starting one day after equity options are traded in exchange. Other indicators are defined similarly.

Table V presents results. In Column (1), we include six indicator variables, firm fixed effects, year fixed effects, loan type fixed effects and loan purpose fixed effects in the regression. The coefficients on *Year t-3*, *Year t-2*, and *Year t-1* are small in magnitude and are not significant, implying there is no trend of declining loan spreads before the option listing. The coefficients on *Year t* and *Year t+1*, are negative and statistically significant at 10% level. Besides, the magnitude of the coefficients on *Year t*, *Year t+1*, and *Year t+2* is much larger than those on *Year t-3*, *Year t-2*, and *Year t-1*. Figure 1 shows similar pre-treatment trends. We plot the estimated coefficients on each year indicator variable in column (1) of Table V. The dashed lines correspond to the 90% confidence intervals of the estimated coefficients. The figure shows that loan spreads are not different between treatment firms and control firms before the option listing. However, loan spreads are significantly lower for option listed firms after they are listed in option exchanges.

In Column (2) and (3) of Table V, we include firm characteristics and loan characteristics. The coefficients on pre-listing period are not statistically different from zero in all specifications. We find that the coefficient on *Year t+1* in each column remains negative and significant at 10% level. Our findings indicate that there is no difference in loan spread between treatment and control firms prior to the option listing, suggesting that the parallel trend assumption of the difference-in-differences test is not violated.

D. Impact of option listing according to information opacity

Previous studies show that the information transparency between borrowers and lenders is an important determinant of lenders' loan pricing decisions (Sengupta (1998), Bharath et al. (2011), Lin et al. (2011)). In this subsection, we examine whether banks' loan pricing decision for listed firms depends on their informational environment. Monitoring advantage view suggests that options improve information environment and reduce monitoring costs for lenders. If options do play a role in enhancing listed firms information environment, we expect that the effect of option listing on loan spreads is more pronounced when there is a higher level of information opacity in borrowing firms. We use four proxies for information opacity of firms: the dispersion of analysts' earnings forecasts, the absolute value of discretionary accruals estimated by Jones (1991) model, R&D expenses and the ownership of institutional investors. We expect the effect of option listing on loan spreads is more pronounced for firms with higher analyst forecast dispersion, higher absolute discretionary accruals, higher R&D expenditures, and lower institutional investors ownership. To test our prediction, we split the sample into two subgroups based on whether each information opacity measure is above or below sample median and re-estimate the regressions separately for each subsample.

Table VI presents the results. Consistent with our expectation, the coefficients on *Post *Listing* are negative and significant in subgroups of firms with higher analyst forecast dispersion, higher absolute discretionary accruals, higher R&D, and lower institutional investor ownership, while the coefficients on *Post*Listing* are insignificant in subgroups of firms with better information environment. The economic magnitude of the option listing effect is substantial. For example, firms in higher analyst forecast dispersion (Column (5)) experience a drop in loan rates by 23.99 %. These results suggest that opaque borrowers are more likely to benefit from option listing because options relieve the information asymmetry between banks and borrowers, supporting the monitoring advantage view.

E. Impact of option listing according to risk-taking incentives

The results of Table VI provide supportive evidence on the monitoring advantage view. In this subsection, we examine the alternative hypothesis, the risk taking view, by focusing on managerial risk-taking incentives. If managers understand the role of options in enhancing stock prices and reducing asymmetric information problems, managers are incentivized to engage in more risky activities after option listing (Blanco and Wehrheim (2017), Blanco and Garcia (2017)). Above the conjecture, the risk taking view expects that the expected costs of financial distress will increase and in turn, option listing leads to an increase in loan spreads.

Besides, equity options provide new hedging opportunities to managers.[3] When managers can reduce hedging costs on their shareholdings (i.e., a firm is listed in the equity option exchange), managers increase their ability to bear risk (Gao (2010)). However, this is not highly feasible. Above all things, policymakers restrict for insiders to trade options based on material value-relevant information (insider trading). Securities and Exchange Commission requires mandatory disclosure on equity options ownership and exercise.[4] Since disclosed hedging behavior provides substantial information with the market, managers should consider potential costs for active hedging, especially when they trade put options. Evidence suggests that litigation risk associated with insider sales is higher than with insider purchases (Cheng and Lo (2006) Johnson, Nelson, and Pritchard (2007), Rogers (2008)). Therefore, managers have practical limitation to take more risk with active option hedging.

We address for alternative hypothesis (the risk taking view) by using managerial risk-taking proxies. If the risk taking view holds, we expect that the declining effect of option listing on loan spread should be weakened with higher risk-taking incentives, or banks increase their loan rates in post-listing period for firms in which CEO have high risk-taking incentives. To test this prediction, we use the sensitivity of CEO wealth to stock price (delta), the sensitivity of CEO wealth to stock return volatility (vega), CEO ownership, and CEO's proportion of equity-based compensation as proxies for managerial risk taking. Delta and vega

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[3] It is legal for managers to buy call and put options in option exchange market. See Section 16(c) of the Securities and Exchange Act of 1934.
[4] See SEC filing form 4 (Statement of changes of beneficial ownership of securities).

are constructed following Core and Guay (2002) and Coles, Daniel, Naveen (2006). We obtain the number of shares owned by CEO, the value of equity-based compensation, and total pay from ExecuComp. Since managers' information coverage is limited to S&P 1500 companies, our sample substantially shrinks after combining managerial characteristics. We employ the similar test procedure used in the previous subsection to test cross-sectional variation in the effect of option listing. We divide subsamples into two groups based on the median CEO characteristics and estimate *Equation (1)* separately for each subsample.

Panel A of Table VII presents the estimates of ordinary least squares (OLS) regressions for each subgroup. We find that the coefficient on *Post * Listing* is negative and significant in low CEO vega subgroup and it is positive and insignificant in high CEO vega subgroup. These results suggest that a higher level of managerial risk-taking incentive (i.e., CEO vega) mitigates the declining impact of option listing on loan spreads, but it does not significantly increase loan rates. Tuning to other risk-taking incentives, the coefficients on *Post *Listing* are not statistically significant across all specifications. Besides, the signs of coefficients are inconsistent with the risk-taking story. These findings do not support the risk-taking explanation and the positive relation between option listing and loan spreads.

Since the sample size in Panel A of Table VII becomes much smaller than the sample in previous analyses, the interpretation from the results may not provide a definitive conclusion. To further shed light on the alternative hypothesis, we additionally examine the circumstances under which the managers' risk-taking incentives are amplified. First, we investigate whether the effect of option listing varies across the industry to which firms belong. We split the firms into firms in high R&D intensity industries and those in low R&D intensity industries. The risk taking view predicts that the increasing effect of option listing on loan spreads is more pronounced firms in high R&D intensity industries because managers in high R&D intensity industries weigh more on the importance of R&D activities to the operations and profit potential and pour substantial resources in high-risk investments. In column (1) and (2) in Panel B, Table VII, we find a significant decrease in loan spreads for firms in high R&D intensity industries and no significant changes in loan spreads for firms in low R&D intensity industries. Together with the results in column (5) and (6) of Table VI, the results show that banks decrease their loan rates for firms with high R&D expenses and those in high R&D intensity industries rather than increase their loan rates for risky investment firms, supporting monitoring advantage view, not risk taking view.

Second, we use the composition of institutional investors and investigate whether the effect of option listing differs across the type of institutional investors. Bushee (1998) categorizes institutional investors into three groups – dedicated, quasi-indexer, transient and finds that firms with higher level of dedicated institutional ownership are less likely to cut R&D to meet short-term earnings goals because the dedicated institutional investors alleviate pressures for myopic investment behavior because of their long-term and large shareholdings. On the other hand, transient institutional investors that have high portfolio turnover and engage in momentum trading significantly increases the probability that managers reduce R&D to reverse an earnings decline. Given long-term and large ownership, dedicated institutions have stronger incentives to motivate managers to take more risk than other institutions (i.e., quasi-index and transient institutions). The risk-taking view predicts that the increasing effect of option listing on loan spreads to be more pronounced when the ownership of dedicated institutions is higher. Similarly, the opposite effect is expected when the ownership of transient institutions is higher from the perspective of risk taking view.

To examine the cross-sectional variation in the relationship between the option listing and loan spreads across the ownership of dedicated (transient) institutional investors, we divide our sample into two subgroups based on whether a firm's the ownership of transient institutions is above or below sample median and re-estimate the baseline regression in *Equation (1)* for each subgroups. Column (3)-(6) reports the regression results. In column (3) and (4), we find that the coefficient on *Post*Listing* is negative and insignificant for firms with higher dedicated institutional ownership and it is negative and significant for firms with lower dedicated institutional ownership. In column (5) and (6), we find that the coefficients on *Post*Listing* are negative and significant in both columns, suggesting that the impact of option listing on loan pricing does not vary across the ownership of transient institutions. In sum, the results in column (3)-(6) are inconsistent with the prediction of risk taking view.

To examine the risk taking view, we further investigate the link between option listing and corporate risk taking. If the option listing affects managerial risk taking incentives, we expect that option listings lead to higher stock return volatility, higher leverage, lower cash holdings, higher investment in R&D expenditure and lower investment in capital expenditures. To test corporate risk taking at the firm level, we begin with propensity-score matched 216 pairs of the listing and non-listing firms in Table III and construct 2,327 firm-year observations from 1993 to 2014. We estimate *Equation (1)* in which the dependent variables are proxies for corporate risk taking. We control for all firm-level characteristics used in Table IV. Loan-level control variables (loan size, loan maturity, performance dummy, and loan purpose) are excluded from the regression.

We first examine the firm risk as a summary measure of firms' risk-taking. We use *Total risk*, the natural logarithm of annualized daily stock return volatility over a fiscal year, and *Idio risk*, the natural logarithm of annualized volatility of the residuals from the market model, as dependent variables. We use the CRSP value-weighted return as a market index. In column (1) and (2) of Table VIII, we find that the coefficients on *Listing*Post* are positive but insignificant. These findings reflect the inconclusive evidence on whether the option trading increases or decreases a firm's risk. Stein (1987) implies that the entry of speculators in options markets may incur price destabilization. In support of his argument, Blau, Bowles, and Whitby (2016) show that speculative trading in options markets results in more volatile stock prices. In contrast, Conrad (1989) and Skinner (1989) provide empirical evidence that stock return volatility declines with option introduction, attributing it to the improvement in the market liquidity.

We further examine whether option listing affects firms' leverage and cash holding policies. The results are reported in column (3) and (4) of Table VIII. We find that firms do not experience the change in their leverage and cash holdings after they are listed in option exchanges. The coefficients on *Listing*Post* are insignificant at the conventional level. Gao (2010) find that firms increase financial leverage after managers can hedge their equity holdings via option markets, which is different from our results. The different results are due to the different sample size, sample period, and different regression specification:

our sample size is smaller because we confine our sample firms covered in Dealscan, our sample period expands from 1996 to 2014, and we use firm fixed effect while Gao (2010) employs industry fixed effects in his regression specification. In sum, the impact of options on leverage seems to be sensitive to sample and empirical specification.

Next, we investigate the change in investment policies. In column (5) and (6), we find that the option listing is positively associated with R&D expenditure and negatively associated with capital expenditures, but the association is not statistically significant. These results do not support the view that option listing increases firm's risky investments.

V. Option Listing and Credit rating

As discussed, the monitoring advantage view predicts that the beginning of equity option trading helps banks facilitate the access of borrower's information. To shed further light on the informational role of options, we investigate whether options influence credit ratings. Credit rating agencies allow investors to quickly assess the broad risk properties using a single and well-known scale (Becker and Milbourn (2011)). To the extent that credit rating agencies concern about information asymmetry between firms and market, information from option markets could mitigate this concern. In this section, we examine whether firms listed in the option exchanges are more likely to receive better credit ratings.

We obtain S&P domestic long-term issuer credit rating from Compustat and assign a numerical score for each credit rating. In our numeric transformation, higher rate score means better credit rating. For example, AAA is assigned a rating score of 22; AA is assigned a rating score of 21; D or SD is assigned a rating score of 2; No rating is coded as 1. We start with propensity-score matched 216 pairs of the listing and non-listing firms in Table III. We use credit ratings measured at the fiscal year end and construct 2,324 firm-year observations from 1993 to 2014. We estimate *Equation (1)* which the dependent variable is a numeric rate score. We control for all firm-level characteristics used in Table IV. Loan-level control variables (loan size, loan maturity, performance dummy, and loan purpose) are excluded.

Table IX presents the results. In column (1), the coefficient on *Post*Listing* is positive and significant at 1 % level. When we include firm characteristics in Column (2), the magnitude and statistical significance become smaller but remains positive and significant. The results indicate that after option listing, credit rating agencies reassess firms' credit risk with reduced costs and thus raise firms' credit ratings. These results provide further evidence on the informational role of options by showing that credit rating agencies also benefit from option listing.

VI. Conclusion

This paper investigates how the introduction of derivative assets affects the cost of bank debt. Specifically, we use equity option listings and perform a difference-in-differences test to examine the impact of option listing on the cost of bank debt. Using a propensity score matched sample, we find that the option listing lowers the bank loan spreads. We find that the declining effect of option listing on loan spreads is more pronounced if borrowers have lower analyst forecasts dispersion, if they have lower absolute value of discretionary accruals, if they have higher R&D expenditures, and if they have lower institutional ownership. These findings imply that the reduced information asymmetry between option-listed borrowers and banks plays a role in loan pricing contracts.

There is little evidence that the treatment effect differs across managerial risk-taking incentives, suggesting that the option listing does not increase managerial risk-taking by facilitating hedging. We show that the impact of option listing is more pronounced for firms in high R&D intensity industries. Also, we find that the treatment effect does not vary across the level of transient institutional investors, while the impact of option listing is more pronounced for firms with lower ownership of dedicated institutional investors. These results suggest that the change in managerial risk-taking incentives after option listing is less likely to affect banks' loan pricing decisions. We provide further evidence that corporate risk-taking behaviors do not change after option listing, which is not consistent with the hypothesis that the option listing, a decrease in managerial hedging costs, induces managers to engage in risk taking.

In additional analyses, we find that the option listing leads to increase borrowing firm's credit rating, suggesting that the informational role of options help credit rating agencies as well as banks. Overall, our findings are consistent with the monitoring advantage view, equity options improve firms' information environment, reduce lenders' monitoring costs, and thereby benefit corporate financing conditions.

Our results illustrate that the improved information environment reduces the cost of bank debt. Further research might investigate such issues as under which circumstances options provide information to banks; whether the information is more sensitive to banks than other debtholders; and are there any other channels that options affect bank loans' pricing in addition to the informational environment? For example, one unexplored area is the banks' transactions in option markets. It would be interesting to extend whether banks tacitly or overtly utilize option contracts to reduce their exposure to credit risk.[5]

Finally, we admit that this study has limitations. First, even though option listing provides plausible properties to test the impact of options on the cost of bank debt, the option listing decisions is not entirely exogenous. Besides, we construct propensity score matched sample to control firms that share similarity in terms of their propensity to be listed in option exchanges, but there remains a possibility that unobservable and omitted characteristics affect firm's borrowing conditions. Future research might use a quasi-natural experiment such as legislations or listing rule changes and establish a causal link between options and borrowing costs

[5] However, this untested hypothesis seems unlikely. First, other authors have examined the effect of CDS contracts and did not find results. Given that CDS contracts would be much better hedging instruments it is unlikely that equity options would drive the results. Second, there are regulatory reasons why banks would not want to own options. Third, the cost of hedging with options would almost certainly be far larger than the benefits.

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Table I.
Distribution of option listing by year and industry

The table presents the distribution of 898 option listing firms covered in Compustat, CRSP, and Dealscan over the period 1996 to 2014 by fiscal year and industry (Two-digit Standard Industrial Classification (SIC) codes are reported in parentheses). We exclude firms that operate in utility industry (SIC codes between 4900 and 4999), financial industry (SIC codes between 6000 and 6999), and public administration industry (SIC codes between 9000 and 9999). The number in parentheses is the percentage of option listing firms in a given industry and year during the sample period.

Fiscal year	Agriculture, forestry, fisheries (01-09)	Mineral, construction (10-17)	Manufacturing (20-39)	Transport, communication (40-48)	Wholesale trade and retail trade (50-59)	Service industries (70-89)	Total
1996	0	19	31	7	14	24	95 (10.58)
1997	0	24	56	11	19	28	138 (15.37)
1998	1	7	25	9	19	31	92 (10.24)
1999	0	2	19	6	9	14	50 (5.57)
2000	0	1	11	9	4	5	30 (3.34)
2001	1	2	31	3	10	18	65 (7.24)
2002	1	7	30	3	15	17	73 (8.13)
2003	1	13	1	6	2	7	30 (3.34)
2004	2	11	23	6	13	7	62 (6.90)
2005	0	13	18	5	7	5	48 (5.35)
2006	0	8	20	5	9	9	51 (5.68)
2007	0	3	22	4	1	5	35 (3.90)
2008	0	5	15	0	2	9	31 (3.45)
2009	0	2	12	0	3	3	20 (2.23)
2010	0	4	6	2	0	3	15 (1.67)
2011	0	5	11	2	0	4	22 (2.45)
2012	0	2	6	4	5	4	21 (2.34)
2013	0	1	7	1	5	0	14 (1.56)
2014	0	0	5	0	1	0	6 (0.67)
Total	6 (0.67)	129 (14.37)	349 (38.86)	83 (9.24)	138 (15.37)	193 (21.49)	898

Table II.
Determinants of option listing

The table reports estimates of logit regression in which the dependent variable is an indicator that takes the value of one if a firm is listed in option exchange a given year, and zero otherwise. The sample consists of 2,607 firm-year observations covered in Compustat, CRSP, and Dealscan from 1996 to 2014. We exclude firms that operate in utility industry (SIC codes between 4900 and 4999), financial industry (SIC codes between 6000 and 6999), and public administration industry (SIC codes between 9000 and 9999). The Appendix provides detailed descriptions of variables. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustering at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable =Option list (indicator)		
Independent variables	(1)	(2)	(3)
Log(market value)	0.498**	0.417*	0.410*
	(0.016)	(0.063)	(0.083)
Log(volume)	1.261***	1.318***	1.323***
	(0.000)	(0.000)	(0.000)
Volatility	0.122	-0.003	-0.048
	(0.784)	(0.995)	(0.918)
Stock return	0.530***	0.510***	0.509***
	(0.000)	(0.000)	(0.000)
Bid Ask spread	-0.092	-0.081	-0.082
	(0.145)	(0.203)	(0.198)
Leverage		0.083	-0.100
		(0.795)	(0.791)
ROA		0.539	0.576
		(0.502)	(0.491)
Tobin's q		0.097	0.097
		(0.107)	(0.120)
Tangibility		-0.246	-0.335
		(0.610)	(0.503)
Cash Flow Volatility		-6.014*	-5.890
		(0.090)	(0.100)
KZ financial constraint (indicator)			0.175
			(0.316)
WW financial constraint (indicator)			-0.126
			(0.296)
HP financial constraint (indicator)			-0.020
			(0.904)
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	2,575	2,462	2,462
pseudo-R-squared	0.349	0.352	0.353

Table III.
Summary statistics

Panel A presents descriptive statistics of a propensity-score matched sample. The sample consist of treatment firms that experience an option listing over the period 1996 to 2014 and control firms that do not experience an option listing over the same period (216 treatment firms and 216 control firms). The propensity score is calculated using the logit regression of *Listing* (an indicator that takes the value one if a firms is listed in equity option exchange, and zero otherwise) on log (volume), Stock return volatility, Stock return, Leverage, ROA, Tobin's q, tangibility, cash flow volatility, KZ financial constraint (indicator), industry dummies (Fama-French 48 industry) and year dummies. We require both treatment and control firms to be in the same industry and in the same fiscal year. We also require that treatment and control firms have at least one loans during both three years before and three years after the option listing. In Panel B, the sample consists of 1,898 loans issued by 432 firms in Panel A. Panel B shows means, medians, and standard deviations of loan and borrower characteristics. Variable definitions are provided in Appendix.

Panel A: Comparison of means across matched sample				
	Treatment firms with an Option listing (N=216): a	Control firms without an option listing (N=216): b	Difference (a – b)	t-test
Log(market value)	5.540	5.504	0.036	-0.342
Log(volume)	11.051	11.086	-0.034	0.431
Stock return volatility	0.539	0.543	-0.004	0.159
Stock return	0.507	0.405	0.102	-0.678
Leverage	0.308	0.291	0.018	-0.838
ROA	0.134	0.124	0.011	-0.699
Tobin's q	2.092	2.214	-0.123	0.250
Tangibility	0.322	0.322	-0.000	0.003
Cash flow volatility	0.016	0.022	-0.005	0.820
KZ financial constraint (indicator)	0.361	0.296	0.065	-1.434

Panel B: Loan and firm characteristics						
	N	Mean	Median	SD	Min	Max
Loan Characteristics						
All in drawn (basis point)	1898	224.256	200	134.254	5	1000
Loan size ($ million)	1898	145.869	75	252.152	0.158	5500
Loan maturity (months)	1898	48.318	50	22.686	1	166
Performance dummy	1898	0.5422	1	0.498	0	1
Number of financial covenants	1654	2.230	2	1.467	0	7
Number of general covenants	1644	2.491	2	1.567	0	5
Firm Characteristics						
Market value ($ million)	1898	1008.524	260.831	5367.327	6.403	77549.47
Volume (Shares, Million)	1898	0.138	0.070	0.266	0.002	3.601
Stock return volatility	1898	0.550	0.510	0.233	0.162	1.378
Stock return	1898	0.322	0.146	0.895	-0.816	5.667
Leverage	1898	0.322	0.316	0.215	0	0.973
ROA	1898	0.127	0.125	0.082	-0.142	0.359
Tobin's q	1898	1.684	1.404	0.867	0.740	5.582
Tangibility	1898	0.325	0.245	0.257	0.015	0.921
Cash flow volatility	1898	0.015	0.010	0.016	0.002	0.105
KZ financial constraint (indicator)	1898	0.309	0	0.462	0	1

Table IV.
Effect of option listing on loan spread

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variable is the logarithm of all-in-spread-drawn, a rate a borrower pays in basis points over LIBOR or LIBOR equivalent. The propensity score is calculated using the logit regression of *Listing* (an indicator that takes the value one if a firms is listed in equity option exchange, and zero otherwise) on log (volume), Stock return volatility, Stock return, Leverage, ROA, Tobin's q, tangibility, cash flow volatility, KZ financial constraint (indicator), industry dummies (Fama-French 48 industry) and year dummies. We require both treatment and control firms to be in the same industry and in the same fiscal year. We also require that treatment and control firms have at least one loans during both three years before and three years after the option listing. *Post* takes the value of one for post-listing period (year *t*, year *t*+1, and year *t+2*), and zero for pre-listing period (year *t*-1 and year *t*-2, and year *t*-3), where year *t* is defined as the next one year starting one day after equity options are traded in exchange. Variable definitions are provided in Appendix. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = Log (spread)		
Independent variables	(1)	(2)	(3)
*Post * Listing*	-0.168***	-0.138**	-0.128**
	(0.003)	(0.020)	(0.027)
Log(market value)		-0.126**	-0.101*
		(0.014)	(0.051)
Log(volume)		0.048	0.038
		(0.143)	(0.259)
Stock return volatility		0.350**	0.369**
		(0.020)	(0.016)
Stock return		-0.002	-0.002
		(0.942)	(0.942)
Leverage		0.108	0.142
		(0.616)	(0.509)
ROA		-1.562***	-1.569***
		(0.000)	(0.000)
Tobin's q		0.009	0.001
		(0.791)	(0.979)
Tangibility		-0.265	-0.258
		(0.356)	(0.385)
Cash flow volatility		-3.781***	-3.522**
		(0.008)	(0.015)
KZ financial constraint (indicator)		0.074	0.045
		(0.150)	(0.386)
Log(loan size)			-0.055***
			(0.003)
Log(loan maturity)			0.057
			(0.197)
Performance pricing(indicator)			-0.090*
			(0.055)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes
Observations	1,898	1,898	1,898
Adjusted R-squared	0.631	0.670	0.678

Table V.
Option listing and the timing of loan spread changes

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variable is the logarithm of all-in-spread-drawn, a rate a borrower pays in basis points over LIBOR or LIBOR equivalent. The propensity score is calculated using the logit regression of *Listing* (an indicator that takes the value one if a firms is listed in equity option exchange, and zero otherwise) on log (volume), Stock return volatility, Stock return, Leverage, ROA, Tobin's q, tangibility, cash flow volatility, KZ financial constraint (indicator), industry dummies (Fama-French 48 industry) and year dummies. We require both treatment and control firms to be in the same industry and in the same fiscal year. We also require that treatment and control firms have at least one loans during both three years before and three years after the option listing. In this table, we replace *Post*Listing* with the indicators *Year t-3*, *Year t-2*, *Year t-1*, *Year t*, *Year t+1*, *Year t+2*, where *Year t* is defined as the next one year starting one day after equity options are traded in exchange. Variable definitions are provided in Appendix. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = Log(spread)		
Independent variables	(1)	(2)	(3)
Year t-3	0.048	-0.003	-0.002
	(0.563)	(0.963)	(0.976)
Year t-2	-0.002	-0.007	-0.005
	(0.980)	(0.902)	(0.935)
Year t-1	0.046	0.081	0.063
	(0.550)	(0.203)	(0.312)
Year t	-0.147*	-0.085	-0.082
	(0.059)	(0.211)	(0.220)
Year t+1	-0.147*	-0.135*	-0.134*
	(0.096)	(0.092)	(0.094)
Year t+2	-0.098	-0.057	-0.062
	(0.380)	(0.565)	(0.533)
Log(market value)		-0.132**	-0.106**
		(0.010)	(0.040)
Log(volume)		0.052	0.042
		(0.136)	(0.234)
Stock return volatility		0.354**	0.370**
		(0.019)	(0.017)
Stock return		-0.000	-0.000
		(0.999)	(0.993)
Leverage		0.094	0.127
		(0.656)	(0.549)
ROA		-1.543***	-1.543***
		(0.000)	(0.000)
Tobin's q		0.008	-0.000
		(0.808)	(0.998)
Tangibility		-0.285	-0.270
		(0.323)	(0.366)
Cash flow volatility		-3.845***	-3.549**
		(0.007)	(0.014)
KZ financial constraint (indicator)		0.080	0.052
		(0.128)	(0.331)
Log(loan size)			-0.054***
			(0.004)
Log(loan maturity)			0.056
			(0.206)
Performance pricing(indicator)			-0.087*
			(0.061)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes

Loan purpose fixed effects	Yes	Yes	Yes
Observations	1,898	1,898	1,898
Adjusted R-squared	0.630	0.670	0.677

Table VI.
Informational opacity and the effect of option listing on loan spreads

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variable is the logarithm of all-in-spread-drawn, a rate a borrower pays in basis points over LIBOR or LIBOR equivalent. The propensity score is calculated using the logit regression of *Listing* (an indicator that takes the value one if a firms is listed in equity option exchange, and zero otherwise) on log (volume), Stock return volatility, Stock return, Leverage, ROA, Tobin's q, tangibility, cash flow volatility, KZ financial constraint (indicator), industry dummies (Fama-French 48 industry) and year dummies. We require both treatment and control firms to be in the same industry and in the same fiscal year. We also require that treatment and control firms have at least one loans during both three years before and three years after the option listing. We divide our sample to two subgroups according to the sample median informational opacity and report results for each subsample. The informational opacity used to split the sample are analyst forecast dispersion (column 1 and 2), the absolute value of discretionary accruals using Jones (1991) model (column 3 and 4), R&D expenditures (column 5 and 6) and institutional investor ownership (column 7 and 8). *Post* takes the value of one for post-listing period (year *t*, year *t*+1, and year *t+2*), and zero for pre-listing period (year *t*-1 and year *t*-2, and year *t*-3), where year *t* is defined as the next one year starting one day after equity options are traded in exchange. Variable definitions are provided in Appendix. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = Log (spread)							
	Analyst forecast dispersion		Absolute value of discretionary accruals		R&D expenditures		Institutional investor ownership	
	High	Low	High	Low	High	Low	High	Low
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Post * Listing*	-0.270**	-0.016	-0.222**	-0.128	-0.215*	-0.096	-0.127	-0.145*
	(0.022)	(0.900)	(0.029)	(0.135)	(0.055)	(0.133)	(0.131)	(0.073)
Control variables (Same as in column 3 of Table IV)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	405	358	908	883	543	1,355	895	1,003
Adjusted R-squared	0.727	0.755	0.680	0.753	0.629	0.707	0.658	0.756

Table VII.
Risk-taking incentives and the effect of option listing on loan spreads

This table reports the estimates of ordinary least squares (OLS) regressions in which the dependent variable is the logarithm of all-in-spread-drawn, a rate a borrower pays in basis points over LIBOR or LIBOR equivalent. The propensity score is calculated using the logit regression of *Listing* (an indicator that takes the value one if a firms is listed in equity option exchange, and zero otherwise) on log (volume), Stock return volatility, Stock return, Leverage, ROA, Tobin's q, tangibility, cash flow volatility, KZ financial constraint (indicator), industry dummies (Fama-French 48 industry) and year dummies. We require both treatment and control firms to be in the same industry and in the same fiscal year. We also require that treatment and control firms have at least one loans during both three years before and three years after the option listing. In panel A, we divide our sample into two subgroups according to the sample median of CEOs' risk-taking incentives. CEO characteristics used to split the sample are CEO delta (column 1 and 2), CEO vega (column 3 and 4), CEO ownership (column 5 and 6), proportion of equity-based compensation to total pay (column 7 and 8). In Panel B, we divide our sample based on the sample median of industry R&D intensity (column 1 and 2), transient institutional investors' ownership (column 3 and 4), and dedicated institutional investors' ownership (column 5 and 6). *Post* takes the value of one for post-listing period (year *t*, year *t*+1, and year *t+2*), and zero for pre-listing period (year *t*-1 and year *t*-2, and year *t*-3), where year *t* is defined as the next one year starting one day after equity options are traded in exchange. Variable definitions are provided in Appendix. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Subsample analyses according to risk-taking incentives

	Dependent variable = Log (spread)							
	CEO Delta		CEO Vega		CEO stock holdings		Proportion of equity-based pay	
	High	Low	High	Low	High	Low	High	Low
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post * *Listing*	-0.040	-0.146	0.126	-0.331**	-0.124	0.032	-0.147	-0.113
	(0.804)	(0.104)	(0.226)	(0.011)	(0.589)	(0.748)	(0.417)	(0.298)
Control variables (Same as in column 3 of Table IV)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	269	282	284	286	247	325	308	272
Adjusted R-squared	0.767	0.828	0.819	0.816	0.780	0.806	0.749	0.832

Panel B: Subsample analyses according to industry R&D intensity and transient(dedicated) institutional investors

	Dependent variable = Log (spread)					
	Industry R&D intensity		Dedicated institutional investor		Transient institutional investor	
	High	Low	High	Low	High	Low
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Post * *Listing*	-0.189*	-0.068	-0.097	-0.137**	-0.152*	-0.202***
	(0.087)	(0.313)	(0.302)	(0.043)	(0.088)	(0.009)
Control variables (Same as in column 3 of Table IV)	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	601	1,297	902	996	888	1,010

Adjusted R-squared	0.732	0.677	0.685	0.759	0.622	0.776

Table VIII.
Effect of option listing on risk-taking behavior

This table reports the estimates of OLS regressions in which the dependent variables are firm risk, investment and financing policies. *Total risk* is the natural logarithm of annualized daily stock return volatility over fiscal year. *Idio risk*, is the natural logarithm of annualized volatility of the residuals from the market model. We use the CRSP value-weighted return as market index. *Leverage* is defined as the sum of long-term debt and debt in current liabilities divided by total assets. *Cash holding* is defined as cash and short-term investments divided by total assets. We use *R&D*, defined as research and development expense divided by total assets, and *Capex*, defined as capital expenditures divided by total assets as firm investment policies. The sample consists of 2,327 firm-year observations of 432 unique firms (216 treatment firms that are listed in equity option exchange over the perioed 1996 to 2014 and 216 control firms that have not experienced an option listing over the same period). The propensity score is calculated using the logit regression of *Listing* (an indicator that takes the value one if a firms is listed in equity option exchange, and zero otherwise) on log (volume), Stock return volatility, Stock return, Leverage, ROA, Tobin's q, tangibility, cash flow volatility, KZ financial constraint (indicator), industry dummies (Fama-French 48 industry) and year dummies. We require both treatment and control firms to be in the same industry and in the same fiscal year. We also require that treatment and control firms have at least one loans during both three years before and three years after the option listing. *Post* takes the value of one for post-listing period (year *t*, year *t*+1, and year *t+2*), and zero for pre-listing period (year *t*-1 and year *t*-2, and year *t*-3), where year *t* is defined as the next one year starting one day after equity options are traded in exchange. Variable definitions are provided in Appendix. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	*Total risk* (1)	*Idio risk* (2)	*Leverage* (3)	*Cash holdings* (4)	*R&D* (5)	*Capex* (6)
*Post * Listing*	0.006	0.010	-0.003	0.009	0.000	-0.001
	(0.831)	(0.706)	(0.819)	(0.284)	(0.992)	(0.812)
Log(market value)	-0.110***	-0.130***	0.004	-0.006	-0.001	0.003
	(0.000)	(0.000)	(0.742)	(0.323)	(0.287)	(0.496)
Log(volume)	0.047***	0.041***	0.004	-0.007*	0.001	0.002
	(0.002)	(0.007)	(0.717)	(0.098)	(0.436)	(0.611)
Stock return volatility			0.043	0.009	0.000	-0.020*
			(0.137)	(0.576)	(0.899)	(0.093)
Stock return	0.002	0.002	-0.004	-0.003	0.001	0.003
	(0.781)	(0.830)	(0.521)	(0.362)	(0.346)	(0.223)
Leverage	0.174**	0.182**		-0.036	-0.005	-0.091***
	(0.037)	(0.029)		(0.104)	(0.223)	(0.000)
ROA	-0.404***	-0.424***	-0.108	0.014	-0.007	-0.014
	(0.006)	(0.003)	(0.188)	(0.811)	(0.577)	(0.610)
Tobin's q	0.056***	0.060***	-0.014	0.017***	-0.003***	0.001
	(0.000)	(0.000)	(0.112)	(0.003)	(0.005)	(0.743)
Tangibility	-0.041	-0.037	0.027	-0.064	0.004	-0.049
	(0.753)	(0.781)	(0.679)	(0.163)	(0.417)	(0.209)
Cash flow volatility	-0.277	-0.345	-0.840**	0.421**	0.045	-0.259
	(0.740)	(0.672)	(0.034)	(0.034)	(0.288)	(0.178)
KZ financial constraint (indicator)	0.027	0.024	0.061***	-0.005	-0.001	0.002
	(0.270)	(0.322)	(0.000)	(0.454)	(0.299)	(0.730)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,236	2,236	2,234	2,231	2,237	2,220
Adjusted R-squared	0.721	0.737	0.763	0.683	0.900	0.736

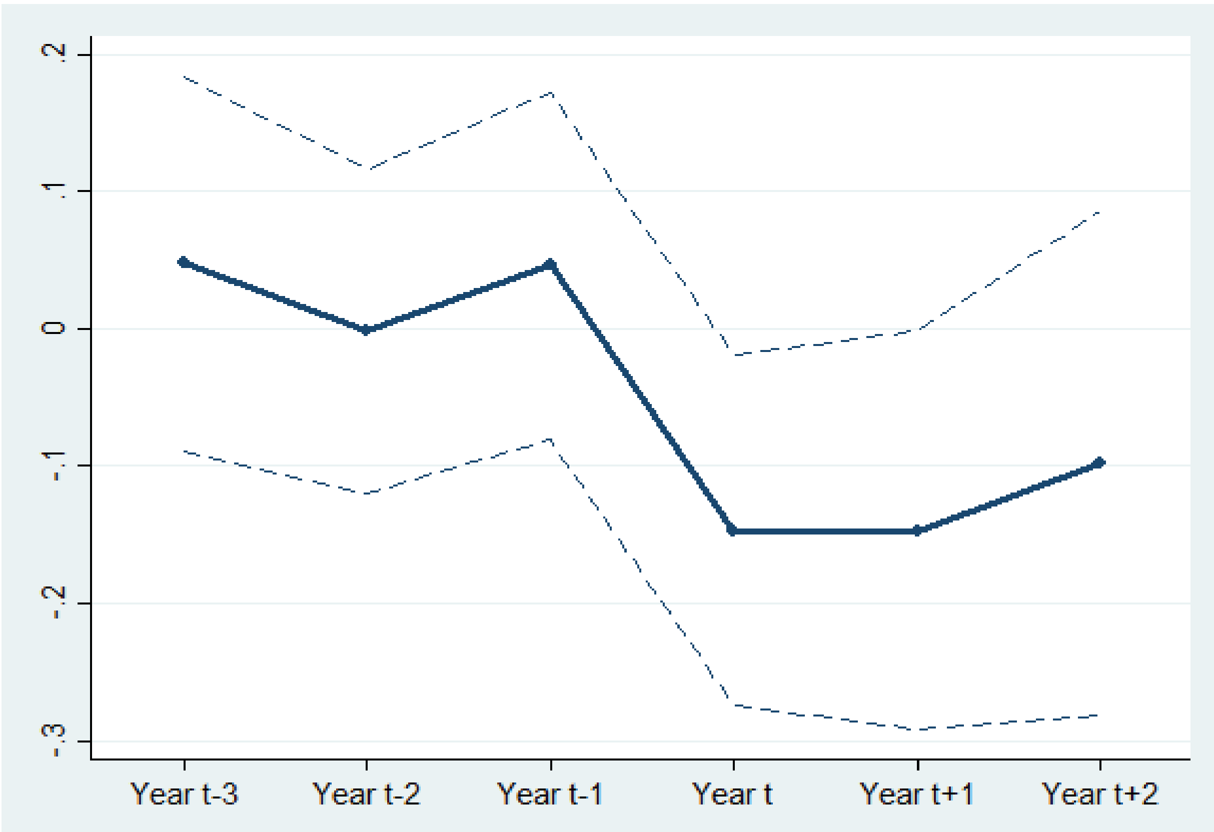
Table IX.
Effect of option listing on credit rating

This table reports the estimates of OLS regressions in which the dependent variable is firm's credit rating score. The sample consists of 2,324 firm-year observations of 432 unique firms (216 treatment firms that are listed in equity option exchange over the perioed 1996 to 2014 and 216 control firms that have not experienced an option listing over the same period). The propensity score is calculated using the logit regression of *Listing* (an indicator that takes the value one if a firms is listed in equity option exchange, and zero otherwise) on log (volume), Stock return volatility, Stock return, Leverage, ROA, Tobin's q, tangibility, cash flow volatility, KZ financial constraint (indicator), industry dummies (Fama-French 48 industry) and year dummies. We require both treatment and control firms to be in the same industry and in the same fiscal year. We also require that treatment and control firms have at least one loans during both three years before and three years after the option listing. *Post* takes the value of one for post-listing period (year *t*, year *t*+1, and year *t+2*), and zero for pre-listing period (year *t*-1 and year *t*-2, and year *t*-3), where year *t* is defined as the next one year starting one day after equity options are traded in exchange. Variable definitions are provided in Appendix. *P*-values reported in parentheses are based on standard errors adjusted for heteroskedasticity and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable = Credit rating score	
Independent variables	(1)	(2)
*Post * Listing*	0.132***	0.086*
	(0.006)	(0.075)
Log(market value)		0.117***
		(0.003)
Log(volume)		0.015
		(0.631)
Stock return volatility		0.006
		(0.944)
Stock return		-0.042*
		(0.073)
Leverage		0.494***
		(0.001)
ROA		0.030
		(0.883)
Tobin's q		-0.050**
		(0.015)
Tangibility		-0.412**
		(0.034)
Cash flow volatility		0.119
		(0.894)
KZ financial constraint (indicator)		0.040
		(0.355)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	2,324	2,324
Adjusted R-squared	0.770	0.780

Figure 1.
The effect of option listing on loan spreads

This figure shows the effect of option listing on loan spreads. The y-axis plots the coefficient estimates from a regression of loan spreads on the indicators *Year t-3*, *Year t-2*, *Year t-1*, *Year t*, *Year t+1*, and *Year t+2*, where *Year t* is defined as the next one year starting one day after equity options are traded in exchange, firm fixed effects, year fixed effects, loan type fixed effects and loan purpose fixed effects. The regression specification is the same in column 1 in Table VI. The x-axis shows the time relative to the option listing. The dashed lines correspond to the 90% confidence intervals of the coefficient estimates. Confidence intervals are calculated from standard errors adjusted for heteroskedasticity and clustered at the firm level.



Appendix. Variable Definitions

Variable names	Variable definitions	Source
Firm characteristics		
Absolute value of discretionary accruals	The discretionary accrual is the residual from a regression of total accruals on lagged size, the change in sales, and gross property, pland, and equipment scaled by total assets for sample firms in the same 2-digit SIC industry. The calculation is followed by Jones (1991)	Compustat
Analyst forecast dispersion	Standard deviation of in analyst forecasts. We use the annual (fourth quarter) earnings and forecasts made in the 90 days prior to the earnings announcement date.	I/B/E/S
Bid-ask spread	Annual average of a firm's daily bid-ask spread. We use daily closing bid and ask data to calculate the spread (100 × (ask − bid) / [(ask + bid) / 2]). We then compute the average value of these daily bid-ask spreads over the fiscal year.	CRSP
Cash flow volatility	Standard deviation of quarterly operating cash flows (*oiadpq*) over eight quarters prior to a loan issuance scaled by book value of total assets (*at*).	Compustat
CEO stock holdings	Number of shares owned by CEO (ExecuComp: *shrown_excl_opts*) divided by Common Shares Outstanding (*csho*)	ExecuComp
Credit rating score	Scale numbers of alphabetical symbols of S&P domestic long-term issuer credit ratings (*splticrm*) 1=AAA,2=AA,3=A,4=BBB,5=BB, 6=B or worse, and 7=no rating (Lin et al. (2011))	Compustat
Dedicated institutional investor	Percentage of shares outstanding owned by dedicated institutions. We classify a dedicated institutions following the permanent transient /quasi-indexer/dedicated classifications of Bushee (1998).	Institutional (13f) Holdings
Delta	Dollar change in wealth associated with a 1% change in the firm's stock price (thousand USD).	Coles, Daniel, Naveen (2006)
HP financial constraint (indicator)	Indicator that takes the value one if a firm's HP index is in the top tercile of the sample in a given fiscal year, and zero otherwise. HP index is calculated following Hadlock and Pierce (2010)	Compustat
Industry R&D intensity	Median R&D of all firms in the same two-digit Standard Industrial Classification(SIC) codes in a given fiscal year	Compustat
Institutional investor ownership	The percentage of shares outstanding owned by institutions that hold at least $100 million in equity securities.	Institutional (13f) Holdings
KZ financial constraint (indicator)	Indicator that takes the value one if a firm's KZ index is in the top tercile of the sample in a given fiscal year, and zero otherwise. KZ index is calculated following Lamont, Polk, and Saaa-Requejo (2001)	Compustat
Leverage	(Long-term debt + Debt in current liabilities)/ Total assets, (*dltt+dlc)/at*.	Compustat
Log(market value)	Natural logarithm of market capitalization, market capitalization is number of shares outstanding (*csho*) multiplied by the stock price at the last day of fiscal year (*prcc_f*)	Compustat
Log(volume)	Natural logarithm of average daily trading volume over fiscal year.	CRSP
Proportion of equity-based pay	Value of option awards (*option_awards_blk_value* before FAS 123R and *options_awards_fv* afterward) plus the value of stock grants (ExecuComp: *rstkgrnt* before FAS 123R and *stock_awards_fv* afterward) divided by the amount of total compensation (*tdc1*)	ExecuComp
R&D expenditures	R&D expenditure. Missing values are coded zero Max (0, *xrd*) / *at*	Compustat
ROA	Operating Income Before Depreciation/total assets, OIBDP/AT	Compustat
Stock return	Buy-and-hold daily stock returns over a fiscal year	CRSP
Stock return volatility	Standard deviation of daily returns over a fiscal year.	CRSP
Tangibility	Net property, plant and equipment/ total assets, PPENT/AT.	Compustat
Tobin's q	(Market value of equity - book value of equity +total assets)/total assets, (*csho*prcc_f+at-ceq*)/*at*	Compustat

Transient institutional investor	Percentage of shares outstanding owned by transient institutions. We classify a dedicated institutions following the permanent transient /quasi-indexer/dedicated classifications of Bushee (1998).	Institutional (13f) Holdings
Vega	Dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's return (thousand USD).	Coles, Daniel, Naveen (2006)
WW financial constraint (indicator)	Indicator that takes the value one if a firm's WW index is in the top tercile of the sample in a given fiscal year, and zero otherwise. WW index is calculated following Whited and Wu (2006)	Compustat
Loan characteristics		
Loan purpose dummies	Indicator variables for loan purposes, including debt repayment, general corporate purposes (corporate purposes, working capital), acquisitions (takeover, acquisition line, LBO), CP backup, and others	Dealscan
Log(loan maturity)	Natural logarithm of loan maturity. Maturity is measured in months.	Dealscan
Log(loan size)	Natural logarithm of the loan facility amount. Loan amount is measured in millions of dollars.	Dealscan
Log(loan spread)	Natural logarithm of all-in spread drawn, defined as the amount the borrower pays in basis points over LIBOR or LIBOR equivalent for each dollar drawn down	Dealscan
Number of financial covenants	Number of financial covenants in loan contract	Dealscan
Number of general covenants	Number of general covenants in the loan contract	Dealscan
Performance dummy	Indicator that equals one if the loan facility uses performance pricing	Dealscan