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Executive compensation and corporate risk-taking: Evidence from private loan contracts☆

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ABSTRACT

We examine how management stock options affect corporate risk taking. We exploit exogenous variation in stock option grants generated by FAS 123R and use loan spreads to infer risk taking. Using a difference-in-differences approach, we find that the spreads of loans taken by firms that did not expense options before FAS 123R (treated firms) significantly decrease after FAS 123R relative to firms that either did not issue stock options or voluntarily expensed stock options before 123R (control firms). We also find that the effect is stronger for firms with high agency conflicts associated with risk-shifting. Furthermore, loans taken by the treated firms are less likely to contain collateral requirements and are less likely to have covenants restricting capital investment post FAS 123R.

1. Introduction

The classical agency theory argues that executive stock options encourage risk-taking (e.g., Jensen and Meckling, 1976; Haugen and Senbet, 1981; and Murphy, 1999). Others, however, find that stock options do not always increase the incentives of risk taking (e.g. Lambert et al., 1991; Carpenter, 2000; Hall and Murphy, 2003; and Ross, 2004). Empirical studies on the impact of stock options on risk taking also find mixed evidence. For example, Guay (1999), Coles et al. (2006), and Chava and Purnanandam (2010) find that executive stock options increase managerial risk-taking. In contrast, Lewellen (2006) finds the opposite and Hayes et al. (2012) find no effect of stock options on risk-taking.

While the existing empirical literature has offered considerable insights into the effect of option compensation on risk taking, most studies suffer from two weaknesses that may have contributed to the inconclusive results. The first weakness is the potential endogeneity of executive compensation plans, i.e., stock options are endogenously determined and are therefore potentially correlated with unobservable firm characteristics. For example, an *ex ante* less risky firm may award more stock options to its managers to encourage managerial risk-taking, and therefore simply regressing measures of riskiness on stock options may underestimate the true impact of stock options on risk taking. The second weakness is the difficulty to empirically capture risk taking. Existing papers use either investment policy, such as R&D investment and capital expenditure, or financial policies, such as cash holding, leverage to

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measure firm risk. One concern with those measures is that it usually takes time for executive compensation to affect firm investment and financial policies, and as such investment and financial policies measured in the short term may not fully capture the effect. Furthermore, these measures are likely not comprehensive as the true riskiness may be unobservable to researchers at all.

To address the measurement problem, we rely on loan spreads to infer corporate risk-taking. The focus on loan spreads is motivated by the classical agency theory that shareholders and debt holders have a conflict of interest over firm asset risk, a phenomenon commonly known as risk shifting or asset substitution (Fama and Miller, 1972; Jensen and Meckling, 1976), and the risk shifting incentives lead to higher agency costs of debt (Green and Talmor, 1986; Leland, 1998; and Parrino and Weisbach, 1999). Compared with the investment and financial policies, loan spreads are a forward-looking measure and can capture the effect of stock options on both contemporaneous and future risk taking. Loan spreads can also capture any increase or decrease in risk taking not directly observable to researchers, but only observable to the lenders.

The focus on the cost of debt is also supported by professional practices. In 2007, Moody's issued a special report outlining how the new accounting requirements under FAS 123R would assist Moody's understanding of executives' pay-related performance incentives (Moody's, 2007). The report states that “executive pay is incorporated into Moody's credit analysis of rated issuers because compensation is a determinant of management behavior that affects indirectly credit quality. The new SEC rules … will give investors greater insight into more rigorous executive pay analysis for fixed-income investors (p. 1).” Further, the report states that “pay packages that are highly sensitive to stock price and/or operating performance may induce greater risk taking by managers, perhaps consistent with stockholders' objectives, but not necessarily bondholders' objectives (p.8).”

We use a quasi-natural experimental setting to identify the effect of stock options on risk taking. We exploit plausibly exogenous variation in stock options generated by the implementation of FAS 123R, which was passed in 2004 and mandated after June 2005. FAS 123R requires all firms to expense employee stock option compensation, which makes granting stock-options more costly and less attractive. Following the passage of FAS 123R, firms significantly reduced their stock option grants and replaced options with restricted stocks, resulting in lower risk taking incentives (Brown and Lee, 2008; Carter et al., 2007; Chava and Purnanandam, 2010; Hayes et al., 2012; Bakke et al., 2016).

We use the difference-in-differences method to estimate the effect of FAS 123R on loan spreads. In this setting, the treated group consists of firms that awarded stock options to their chief executives but did not expense stock options before the implementation of FAS 123R. Firms in the treated group will find stock options less attractive and thus reduce option awards afterward. Our control firms come from two different groups. The first control group consists of firms that had no stock options granted to their CEOs prior to FAS 123R and the second control group consists of firms that granted stock options but also voluntarily expensed stock options before FAS 123R. As such, FAS 123R unlikely affects option compensation policies for control firms. Consistent with the conjecture, we indeed find that both the number of stock options issued and the percentage of option value relative to total compensation decrease significantly after FAS 123R for treated firms; in contrast, control firms experience a slight increase in both measures. More importantly, our main DID results show that loan spreads of treated firms decrease by as much as 19% relative to control firms after the implementation of FAS 123R, suggesting that treated firms experience a decrease in the cost of debt after FAS 123R. The reduction is also economically significant. The average spread for treated firms prior to FAS 123R is 144 basis point, and a 19% reduction results in a 27 basis points reduction in loan spreads and more than $2.9 million saving in interest payments annually.[1] Our results are also robust to a broad set of controls and firm fixed effects.

Firms endogenously determine their stock options awards to executives and subsequent accounting choices for option compensation, and the assignment of control and treated firms may not be random. The non-randomness may then pose threats to the parallel trend assumption critical for the DID estimation. One mitigating factor is, however, that we obtain similar results when using the two different control groups separately. As the two control groups differ significantly, it is unlikely that they create the same bias towards our baseline results in the absence of the effect of FAS 123R. To further alleviate the endogeneity concern, we conduct several tests to rule out the possibility that the baseline results are driven by the non-random assignment of treated and control firms.

First, we follow the suggestion of Roberts and Whited (2013) and compare the time trends of loan spreads for treated and control firms around FAS 123R. We find that the two trends indeed move in parallel before FAS 123R, and upon the implementation of FAS 123R, they diverge abruptly with loan spreads being significantly lower for treated firms than for control firms. Next, we conduct a placebo test in which we create a pre-FAS 123R placebo event and examine how the placebo event affects the treated and control firms differently. If our results are simply capturing a differential trend between control and treated firms, we should expect the placebo event to have similar effects on loan spreads as does FAS 123R. We indeed find no effects of the placebo event. Hence, both tests suggest that the key identifying assumption of the difference-in-differences estimation, the parallel trend condition, is likely to hold in our sample. The subsequent post-event divergence in the two trends provides support to our baseline results that FAS 123R *causes* costs of debt to decrease for treatment firms relative to control firms.

We next test whether the effect of FAS 123R on loan spreads varies with the extent of risk-shifting conflicts between the lender and the borrowing firm. Firms closer to bankruptcy are arguably more subject to risk shifting incentives, and both the manager and shareholders have the incentive to take excessive risks to avoid bankruptcy. It is therefore expected that the impact of FAS 123R should be more pronounced for firms with higher bankruptcy risk. Using Merton's distance to default measure as the proxy for bankruptcy risk, we find that the effect of FAS 123R is stronger for treated firms with lower distance to default measure, i.e., treated firms with higher bankruptcy risk. In addition, we also find that the negative effect of FAS 123R on loan spreads concentrates in treated firms that experience the largest decline in option compensation, providing further support for the risk-taking hypothesis.

Prior studies argue that FAS 123R can improve the usefulness of borrowers' accounting information in debt contracting and

[1] The average deal amount for treated firms is $1.09 billion.

reduce the cost of debt.[2] An alternative explanation to our results is that increased informativeness of accounting information helps better predict credit risk of borrowers and improves the usefulness of accounting information in loan contracting. Hence, the expensing of stock options can lead to lower costs of debt. However, one may also argue that the informativeness of option compensation expense should be irrelevant to lenders as lenders can always exclude the non-cash expense when determining borrowers' earnings used in the financial covenants.[3] Nevertheless, to rule out this alternative explanation, we examine the impact of FAS 123R on loan covenants usage to infer which of the two channels, i.e., the risk-taking channel or the accounting quality channel, better explain the decrease in loan spreads. Consistent with the risk-taking channel, we find that treated firms experience decreases in the use of covenants designed to curb excessive risk-taking after FAS 123R. Contrary to the accounting quality channel, we do not find a significant increase in the use of financial covenants or earnings-based financial covenants after FAS 123R. Hence, the results suggest that FAS 123R does not change the usefulness of accounting information in facilitating loan contracting efficiency.

This paper contributes to the literature on the relationship among executive option compensation, risk-taking incentives, and the cost of debt. One line of literature examines how executive compensation affects managerial risk-taking incentives and therefore corporate financing and investment. For example, Guay (1999) finds that managers receiving stock options tend to invest in risky projects. Rajgopal and Shevlin (2002) and Coles et al. (2006) find that a higher option risk leads to more risky investment and higher debt financing. Lewellen (2006), however, finds that stock options decrease rather than increase the use of debt financing. Another line of literature examines how risk-shifting incentives arising from executives' option compensation affect firms' cost of debt. Theories (Jensen and Meckling, 1976; Green and Talmor, 1986; Leland, 1998; and Parrino and Weisbach, 1999) all argue that the risk shifting incentives of shareholders or managers increase the cost of debt, and subsequent empirical studies provide some evidence suggesting that high option compensation risk is associated with higher costs of debt (Daniel et al., 2004; Billett et al., 2010; and Shaw, 2012).

Most above mentioned papers suffer from the endogeneity problem that executive compensation is endogenously determined and is, therefore, unable to establish a causal effect of executive compensation on risk-shifting incentives or the cost of debt.[4] Our paper complements the literature by exploiting plausibly exogenous variation in stock option grants due to the implementation of FAS 123R and therefore is able to provide unequivocal evidence that executive options grants indeed affect managers' risk-taking incentives and subsequently perceived risk by debt lenders.

Our paper is also related to the literature on the effect of managerial option compensation on debt contracting. Ortiz-Molina (2006) and Brockman et al. (2010) find that bond spreads increase with managerial option compensation. Consistently, our results show that an exogenous reduction in option compensation leads to lower bank loan spreads. Our paper adds to the existing literature in two directions. First, while Ortiz-Molina (2006) and Brockman et al. (2010) focus on public bonds, our paper focuses on the cost of private debt. Second, our paper establishes the causal effect of option compensation on debt contracting. Results in previous literature may be biased as they may suffer from potential endogeneity problems,[5] and it is, therefore, difficult to ascertain whether option compensation truly has a causal effect on corporate loan contracts.[6] Our paper complements the literature and provides causal evidence that executive options grants indeed affect managers' risk-taking incentives and hence the cost of debt.

The rest of the paper is organized as follows. Section 2 describes the implementation of FAS 123R and reviews the related literature; Section 3 describes the data, sample construction, and identification strategy; Sections 4 and 5 discuss main empirical results; and Section 6 concludes.

2. Development of FAS 123R and literature review

2.1. The development of FAS 123R

In October 1972, the Accounting Principle Board (APB) issued Opinion No. 25, *Accounting for Stock Issued to Employees*. Under APB

[2] Barth et al. (2003) show that the recognition of a highly unreliable accounting amount (e.g., option expense) leads to greater price informativeness than simply disclosing it. Barth et al. (2012) find that recognized option compensation expense is informative in predicting firm future performance for the majority of their sample firms. However, prior studies also argue that the adoption of fair value accounting can potentially reduce the usefulness of accounting information for debt holders (Ball et al., 2015; Demerjian et al., 2016; Ertan and Karolyi, 2016).

[3] In an experimental setting, Viger et al. (2008) argue that loan officers have limited attention and processing power and fixate on reported earnings as in Hirshleifer and Teoh (2003). The authors argue that loan officers' perception of borrowers' risk depends on borrowers' reported earnings and charge higher risk premiums of granted loans when the fair value of stock options is recognized compared to pro forma disclosure on the earnings impact of option expense. However, prior studies argue that private lenders are sophisticated investors with access to private information; therefore, they should be unlikely to suffer from the earnings-fixation bias as shown in Viger et al. (2008).

[4] Although many of these papers use simultaneous equation as an attempt to resolve the endogeneity problem, they often impose arbitrary exclusion conditions that may undermine the causal inference of the estimation results. The same concern even applies to papers that use instrumental variable approach, for example, Armstrong and Vashishtha (2012) and Shue and Townsend (2013).

[5] Both Ortiz-Molina (2006) and Brockman et al. (2010) acknowledge the threat of endogeneity issue. Brockman et al. (2010) use simultaneous equation as an attempt to resolve the endogeneity problem. However, such approach often imposes arbitrary exclusion conditions that may undermine the causal inference of the estimation results as acknowledged by Brockman, Martin, and Unlu, (2010). Specifically, the authors state in Footnote 18 that "it is important to note that our simultaneous equation estimates can be biased if this and related assumptions fail to hold" (page 1139). The same concern also applies to Ortiz-Molina (2006) that uses instrumental variable approach.

[6] For example, if managers are risk-averse and shareholders, trying to encourage more risk-taking, may award more option compensation to managers, resulting in downward bias on the effect of option compensation on the costs of debt. On the other hand, if shareholders and managers themselves are risk-loving and managers are awarded more option compensation, the effect would be biased upward.

Opinion No. 25, firms were required to expense fixed-plan stock options issued to employees based on the intrinsic value of the options, that is, the difference between the exercise price of the stock options and the grant date price of the underlying stock. Under the intrinsic value method, firms can avoid option expenses by granting fixed-term options with exercise prices equal to or above the underlying stock prices at the grant date. In October 1995, the Financial Accounting Reporting Board (FASB) issued FAS 123, *Accounting for Stock-Based Compensation*, and recommended firms expense the fair value of options granted (the fair value method). However, due to strong opposition based on the worry that recognizing the fair value of options would increase the cost of capital, the fair value method was not mandated by FAS 123, and the intrinsic value method was still allowed as an alternative method. Under FAS 123, if a firm chooses to use the intrinsic value method, it is required that a footnote disclosure of what the costs of stock options would be on a pro forma basis if the fair value method had been used. Not surprisingly, after the passage of FAS 123, most firms chose to continue using the intrinsic value method and avoided expensing options by granting options with exercise prices equal to grant date underlying stock prices.

The favorable accounting treatment for stock options has contributed to a significant increase in the use of options as a form of compensation (Murphy, 1999). The wide use of stock options may even have contributed to a series of corporate frauds at firms like Enron and WorldCom (Hall and Murphy, 2003). In light of the unprecedented accounting scandals, the debate over accounting for option compensation resumed its intensity in the early 2000s. Proponents of option expensing argued that income expense recognition will ensure that financial statements more accurately reflect economic reality and improve financial reporting quality. Opponents dismissed the argument by stating that option compensation was already disclosed in footnotes. However, despite the heavy lobbying from businesses against expensing stock options, in December 2004, the FASB issued FAS 123R, *Accounting for Stock-Based Compensation (Revised)*, which superseded FAS 123 and made the fair value method the only method allowed for stock options compensation. FAS 123R became effective starting from the first fiscal year after June 15, 2005, for large public firms.[7,8] Under FAS 123R, firms have to measure the fair value of stock options granted to employees and recognize option expenses through income statements over the vesting period of the option awards.

2.2. Literature on the effect of FAS 123R

Prior studies also use FAS 123R as a natural experiment to study the relationship between management stock options and corporate risk-taking. Chava and Purnanandam (2010) find that FAS 123R results in lower leverage, higher cash balances, and greater earnings management. Bakke et al. (2016) instead examine the effect of FAS 123R on corporate risk management and find that FAS 123R leads to higher hedging intensities for treated firms. Similar to Bakke et al. (2016), Francis et al. (2017) document that firms' exposure to foreign exchange rate decreases after FAS 123R. In contrast, Hayes et al. (2012) find little effect of FAS 123R on five measures of corporate risk-taking, namely, R&D expense, capital expenditure, leverage, cash holdings, and stock volatility.

Mao and Zhang (2018) examine the effect of FAS 123R on firm innovation. They argue that the innovation process is risky and long and firms become less innovative after FAS 123R as managers become more risk averse. However, after adjusting for the data truncation bias, Biggerstaff et al. (2019) fail to find any significant decrease in innovation activities after FAS 123R. Canil and Rosser (2018) find that firms' cash holding, which is associated with risk-taking incentives, does not change after FAS 123R.

The measures of risk used by these studies may not capture the long-term effect of FAS 123R as changes in firm financial and investment policy are costly and may take time to implement (Cooper and Haltiwanger, 2006). In addition, these measures are likely not comprehensive as the true riskiness may not be observable to researchers. In this study, we infer firm risk from changes in loan spreads, which are forward-looking and can capture the effect of stock options on both contemporaneous and future risk taking. Chu et al. (2019) examine how FAS 123R affects bank risk taking in mortgage lending and the authors find that stock option does not affect the overall risk of the mortgaging lending, but affect banks' securitization decisions.

Prior studies also investigate the effect of FAS 123R on other corporate policies. For example, Ferri and Li, 2020 argue that large stock option grants cause managers to favor stock repurchase over stock dividends. Using FAS 123R as an instrument for variation in option grants, the authors fail to find any significant changes in payout policy. The authors conclude that option compensation does not have a first-order impact on firms' payout policy.[9] Dou et al. (2019) argue that FAS123R improves the financial reporting quality, which leads to increased monitoring from equity investors. They find an increase in investment efficiency for underinvesting firms after FAS 123R. In this study, we examine how FAS 123R affects the cost of bank loans. Private lenders are sophisticated investors and have access to private information. In addition, option compensation costs were disclosed in footnotes prior to FAS 123R, and private lenders should have already incorporated such information in their risk assessment of the borrower. Hence, the changes in the financial reporting quality around FAS 123R should have less impact on private lenders. We empirically test this possible alternative channel later.

Several papers also examine changes in debt structures around FAS 123R. Chen et al. (2019) argue that managers with high stock option prefer public debt over private loans in order to avoid direct bank monitoring. They find that private loan borrowing increases after FAS 123R as managers receive fewer stock option awards. Related to our study, Hong (2019) argues that the agency costs between debt and equity holders decrease after FAS 123R, and the author shows that firms affected by FAS 123R experience an increase in private loan maturity after FAS 123R.

[7] For example, if a firm's fiscal year ended in July 2005, the firm started adopting FAS 123R in August 2005. Likewise, if a firm's fiscal year ended in December 2005, January 2006 was the first month that FAS123R became mandatory for the firm.

[8] The effective date for small public firms and private firms was the first fiscal year after December 15, 2005.

[9] We also explicitly control payout policy in our regressions. Therefore, our results are unlikely driven by the changes in payout policy.

3. Data, sample construction, and summary statistics

3.1. Data and sample construction

We consider all U.S. firms covered in the annual ExecuComp database from 2003 through 2008, from which we also obtain CEOs' characteristics and options compensation data. Firms are required to adopt FAS 123R in the first fiscal year after June 2005. As such, we treat the fiscal year 2006 as the first year in the post-FAS 123R period.[10] We limit our sample to six years surrounding the passage of FAS 123R (three years prior to FAS 123R: 2003 to 2005, and three years post-FAS 123R: 2006 to 2008) in order to isolate the effect of FAS 123R. To ensure that our results are not driven by the financial crisis in 2008, we also exclude 2008 from our sample. To be symmetrical, we also remove 2003 from the sample and our results are robust to this alternative sample selection. In addition, we require firms to have valid financial information from Compustat for measuring the variables used in the empirical analyses. We also exclude firms in financial (SIC codes 6000–6999) and utility (SIC codes 4910–4940) industries because those firms are likely to face constraints and incentives different from firms in other industries.

We then merge the ExecuComp sample with LPC DealScan to obtain information on bank loans borrowed by the sample firms. We delete all facilities with missing all-in-drawn pricing information. Our final sample consists of 4457 facilities (3337 packages) issued to 1144 unique firms, among which 939 firms are in the treated group and 205 firms are in the control group. For the 205 control firms, 58 firms elected to expense their options prior to FAS 123R and 147 firms did not issue any options to their chief executives prior to FAS 123R. We winsorize all continuous variables at the 1st and 99th percentiles to mitigate the influence of outliers.[11]

3.2. Empirical strategy

To empirically identify the causal effect of risk shifting incentives on the cost of debt, we explore plausibly exogenous variation in managerial risk shifting incentives due to the implementation of FAS 123R. Specifically, we compare the effect of FAS 123R on firms that are affected by FAS 123R (the treated group) and on firms that are not affected by FAS 123R (the control group). Following Bakke et al. (2016), we identify two control groups whose options awards and therefore accounting policy for options awards are unlikely to be affected by the implementation of FAS 123R. The first control group consists of firms that did not pay options to their executives in the pre-treatment period, and therefore the requirement of expensing the fair value of stock options does not apply to them. The second control group consists of firms that already expensed executive stock options before the implementation of FAS 123R.[12] The treated firms are all remaining firms covered by the ExecuComp database that paid options to executives but did not expense the fair value of the options before the implementation of FAS 123R.

In our main specification, we combine the two control groups in the empirical analyses. However, we also present results using the two control groups separately. Formally, our main difference-in-differences specification is as follows:

$$Y_{it} = \alpha + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i \times Post_t + \gamma X_{it} + \varepsilon_{it} \quad (1)$$

where Y_{it} is the loan contract terms, including the natural logarithm of loan spreads over LIBOR and various other measures of loan covenants; $Treat_i$ equals one if firm i is in the treated group, and equals zero otherwise; $Post_t$ equals one if a loan is borrowed after the implementation of FAS 123R, and equals zero otherwise; X_{it} is a vector of firm, loan, and CEO characteristics. In this setting, β_3 is the difference-in-differences estimate, which captures the effect of FAS 123R on loan contract terms for treated firms relative to control firms. We estimate this equation with either industry fixed effects or firm fixed effects. When the model is estimated with year and firm fixed effects, *Post* and *Treat* will be subsumed by the fixed effects.

For firm characteristics, we include firm size (*Size*); Tobin's Q (*TobinQ*); profitability (*ROA*), which is defined as operating income divided by total assets; prior year stock return (*LReturn*); sales growth (*SalesGrowth*), leverage (*Leverage*); asset tangibility (*Tangibility*); R&D expenses (*RDExp*); whether a firm is missing R&D expenses (*MissingR&D*); capital expenditure (*CapExp*); and whether a firm pays dividends (*PayDiv*). In addition, we also include a dummy variable indicating whether a firm has an investment grade credit rating based on Standard & Poor's long-term credit rating (*Rating_IG*).

For loan characteristics, we control for loan maturity (*LogMaturity*), which is measured in months; loan amount (*LogAmount*); and covenant intensity (*LogCovIntensity*). All these variables are transformed using the natural logarithm. Covenant intensity is measured following Bradley and Roberts (2015). We also control for performance pricing (*PerPricing*). *PerPricing* is a dummy variable equal to one if a loan contains covenants linking loan spreads with firm future performances, and zero otherwise. All variable definitions are provided in the Appendix.

[10] Compustat classifies a firm's fiscal year starting after June 2005 as fiscal year 2006.

[11] The results are qualitatively similar with original variables.

[12] We obtain the list of firms that voluntarily adopted the expensing of the fair value of stock options prior to the effective date of FAS 123R from Bear Stearns Equity Research dated December 16, 2014 (Bear Sterns & Company, Inc., 2004).

Table 1
Descriptive statistics.

	Treated Firms		Control firms No Option Awards		Control firms Early Adopters	
	Mean	Median	Mean	Median	Mean	Median
Panel A: Loan characteristics						
LogSpread	4.653	4.828	4.925	5.011	4.565	4.700
Secured	0.426	0.000	0.510	1.000	0.366	0.000
MaxCapex	0.163	0.000	0.204	0.000	0.144	0.000
NumFinCov	1.200	1.000	1.704	2.000	0.986	1.000
NumEarnCov	1.521	2.000	1.332	1.000	0.685	0.000
LogMaturity	3.772	4.111	3.809	4.111	3.728	4.111
LogAmount	19.422	19.432	19.153	19.114	20.109	20.152
LogCovIntensity	0.765	0.693	0.797	0.693	0.547	0.000
PerPricing	0.596	1.000	0.574	1.000	0.431	0.000
Panel B: Firm characteristics						
Size	7.969	7.858	7.782	7.620	9.661	9.825
TobinQ	1.788	1.524	1.614	1.420	1.558	1.352
ROA	0.138	0.129	0.130	0.120	0.097	0.118
LReturn	0.142	0.060	0.180	0.062	0.245	0.073
SalesGrowth	0.133	0.092	0.127	0.083	0.080	0.079
Leverage	0.289	0.268	0.300	0.268	0.344	0.295
Tangibility	0.277	0.209	0.301	0.249	0.383	0.334
MissingRD	0.378	0.000	0.550	1.000	0.401	0.000
RDExp	0.026	0.000	0.019	0.000	0.012	0.000
CapExp	0.068	0.034	0.096	0.034	0.078	0.044
PayDiv	0.537	1.000	0.433	0.000	0.675	1.000
Rating_IG	0.376	0.000	0.189	0.000	0.472	0.000
Sample Size	3593		545		369	

This table presents the summary statistics of variables used in the empirical analyses. The loan-level data are obtained from LPC DealScan from 2003 to 2008; firm-level data are from Compustat Industrial Annual; data on CEO characteristics and option awards are from ExecuComp. Definitions of variables are in the Appendix.

4. Empirical results

4.1. Summary statistics

Table 1 presents summary statistics for all variables used in the analyses. We present the statistics for treated firms and the two control groups separately. Panel A of Table 1 lists all the loan characteristics. Our main dependent variable is DealScan all-in-drawn spread, which is the spread over LIBOR for each dollar drawn under the loan commitment. Following the literature, we use the natural logarithm transformation of the spread (*LogSpread*). The average *LogSpread* is 4.653 for the treated firms, and 4.925 and 4.565 for control firms that do not issue options and control firms that voluntarily adopt fair value recognition of stock options.

Panel B presents the summary statistics of variables measuring firm characteristics. We find that voluntary adopters are much larger in size (mean = 9.661) than treated firms (mean = 7.969) and control firms with no option awards (mean = 7.782). Early adopters are also more likely to have an investment-grade credit rating (mean = 0.472). Our treated firms are more similar to the control firms that did not issue stock options prior to FAS 123R. As discussed later, our DID estimates remain robust when we only use the no option awards control firms, suggesting that our results are not driven solely by the early adopter control firms.

4.2. The effects of FAS 123R on option compensation

In this section, we examine whether FAS 123R indeed reduces managers' option compensation and risk-taking incentives. To measure option compensation, we scale the dollar values of option compensation as a fraction of total compensation (*Option_TotalComp*).[13] We estimate a difference-in-differences model similar to Eq. (1), and results are reported in Column (1) of Table 2. As shown in Column (1), the coefficient on *Post* is positive but not statistically different from zero, suggesting that control firms did not change their option compensation around FAS 123R. However, the DID estimate is negative (coefficient = −0.138) and statistically significant at the 1% level, suggesting that treated firms reduced options grants significantly relative to control groups after FAS 123R. Hence, results in Column (1) of Table 2 support the assumption underlying the risk-taking channel that treated firms reduce their option grants after FAS 123R.

For Columns (2)–(5), we report changes in other components of compensation: salary (*Salary_TotalComp*), bonus (*Bonus_TotalComp*), restricted stock (*RS_TotalComp*), and long-term incentive awards (*LTIA_TotalComp*), and total compensation (*TotalComp*). We measure all components as the ratios relative to total compensation. Results show that there is no difference in other components of compensation

[13] Alternatively, we also use unscaled the number of options granted and we obtain similar results.

Table 2
FAS 123R and changes in CEO option compensation.

	Option_ TotalComp	Salary_ TotalComp	Bonus_ TotalComp	RS_ TotalComp	LTIA_ TotalComp	Log_ TotaolComp
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.238***	−0.100***	−0.065***	−0.022	−0.031	0.297***
	(0.000)	(0.000)	(0.000)	(0.275)	(0.106)	(0.000)
Post	0.006	−0.035	−0.000	0.067***	−0.032*	0.133
	(0.810)	(0.120)	(0.984)	(0.004)	(0.098)	(0.129)
Treat × Post	−0.138***	0.036	0.021	0.006	0.061***	−0.088
	(0.000)	(0.120)	(0.376)	(0.800)	(0.004)	(0.344)
Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed	Yes	Yes	Yes	Yes	Yes	Yes
N	4449	4449	4449	4449	4449	4457
Adj. R-Square	0.321	0.294	0.101	0.128	0.099	0.482

This table presents the results estimating the following equation

$$Y_{it} = \alpha + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i \times Post_t + \gamma X_{it} + \tau Trend_t + \varepsilon_{it}$$

The dependent variable is the fraction of individual compensation components relative to total compensation. *Treat* equals one for treated firms, and zero otherwise, and *Post* equals one after the implementation of FAS 123R and zero otherwise. Other control variables are as defined in the Appendix. Statistics in parentheses are *p*-values clustered at the firm level. Significance levels at 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

between treated and control firms around FAS 123R except for long-term incentive awards. We find a significant and positive DID estimate in Column (5) for long-term incentive awards, suggesting that treated firms significantly increased long-term incentive awards to their CEOs relative to control firms after FAS 123R.[14] Consistent with Hayes et al. (2012), we find that control firms increased restricted stocks to their CEOs as the coefficient on *Post* is positive and statistically significant in Column (4). However, the DID estimate is not statistically significant, suggesting that treated firms also increased restricted stocks after FAS 123R but the increase is not different from that of control firms. In Column (6), we examine total compensations for treated and control firms, which is measured as the logarithm of the fair value of total compensation (*Log_TotalComp*). The coefficient on DID is again not statistically significant, indicating no change in total compensation for treated firms relative to control firms. Hence, it seems like treated firms replace option compensation to CEOs by long-term incentive awards.

4.3. Main DID results

We present the baseline DID results in Table 3, with Panel A for the whole sample with two control groups combined and Panel B for two control groups separately. We also report results both with and without loan characteristics to mitigate the concern that loan characteristics and loan spreads are simultaneously determined and are thus endogenous. In Column (1) of Panel A, the DID estimate, i.e., the coefficient on *Treat* × *Post*, is negative and statistically significant, suggesting that, after FAS 123R, loan spreads of treated firms decrease relative to control firms. In Column (2), we add loan characteristics as additional control variables and the DID estimate remains negative and statistically significant. The effect is also economically significant: FAS 123R reduces loan spreads for treated firms by as much as 19% relative to control firms. It translates to a 27 basis-point reduction in loan spreads and more than $2.9 million saving in interest every year for treated firms.

To show the robustness of our results, in Columns (3) and (4) of Panel A, we include firm and year fixed effects, which subsume the effects of *Treat* and *Post*. The DID estimates are again negative and statistically significant both with and without loan characteristics. Economically, the effect of FAS 123R on the reduction of loan spreads is close to 10%.

In Panel B, we present the results of estimating the DID model by comparing our treated firms with the two control groups separately. The DID estimate remains negative and statistically significant for both control groups. Because the two control groups are different in many firm level characteristics, the results in Panel B suggest that the negative effect is unlikely to be driven by the inherent differences between the treated and control firms. For example, one may argue that the baseline results are driven by the difference between firms with stock options and firms without stock options (instead of driven by FAS 123R). However, if that is the case, we would not observe the negative effect of FAS 123R when comparing the treated firms and firms in the other control group, that is, firms that voluntarily recognized the fair value of their stock options before FAS 123R. On the other hand, if the baseline results are driven by firms' decision whether to recognize the fair value of stock options before FAS 123R, we should not observe any effect when comparing the treated firms with firms in the second control group.[15]

Overall, the negative effects of FAS 123R on loan spreads are consistent with the idea that FAS 123R reduces treatment firms' risk taking incentives and hence the agency costs of debt.

[14] Untabulated results show that our results do not vary in the changes in the long-term incentive awards around FAS 123R. Hence, our results are not driven by the increase in the long-term incentive awards.

[15] See Rosenbaum (1987), Heckman and Hotz (1989), and Roberts and Whited (2013) for the idea of using multiple control group to improve identification in the difference-in-differences setting.

Table 3
Baseline results of the effects of FAS 123R on loan spreads

Panel A: Whole sample

	Whole Sample			
	(1)	(2)	(3)	(4)
Treat	0.116**	0.126***		
	(0.016)	(0.008)		
Post	0.057	0.080		
	(0.353)	(0.169)		
Treat × Post	−0.176***	−0.188***	−0.098**	−0.099**
	(0.006)	(0.002)	(0.040)	(0.036)
Size	−0.104***	−0.008	−0.103**	−0.042
	(0.000)	(0.622)	(0.024)	(0.348)
TobinQ	−0.249***	−0.213***	−0.102***	−0.087***
	(0.000)	(0.000)	(0.003)	(0.009)
ROA	−1.037***	−0.852***	−0.474**	−0.448**
	(0.001)	(0.001)	(0.033)	(0.011)
LReturn	0.071***	0.047***	0.055***	0.050***
	(0.000)	(0.010)	(0.005)	(0.006)
SalesGrowth	−0.002	−0.078	0.053	0.043
	(0.970)	(0.113)	(0.398)	(0.482)
Leverage	1.164***	0.988***	1.161***	1.054***
	(0.000)	(0.000)	(0.000)	(0.000)
Tangibility	0.096	0.108*	−0.046	−0.010
	(0.139)	(0.076)	(0.738)	(0.939)
MissingRD	0.021	−0.010	−0.127	−0.115
	(0.604)	(0.792)	(0.334)	(0.356)
RDExp	0.245	0.203	0.611**	0.576**
	(0.354)	(0.415)	(0.040)	(0.013)
CapExp	0.015	0.070	−0.168	−0.145
	(0.947)	(0.705)	(0.276)	(0.336)
PayDiv	−0.319***	−0.262***	−0.225***	−0.187***
	(0.000)	(0.000)	(0.000)	(0.002)
Rating_IG	−0.695***	−0.557***	−0.483***	−0.407***
	(0.000)	(0.000)	(0.000)	(0.000)
LogMaturity		0.024		0.020
		(0.258)		(0.300)
LogAmount		0.303***		0.172***
		(0.000)		(0.000)
LogCovIntensity		−0.239***		−0.231***
		(0.000)		(0.000)
PerfPricing		−0.143***		−0.082***
		(0.000)		(0.000)
Intercept	6.047***	7.752***	5.893***	6.826***
	(0.000)	(0.000)	(0.000)	(0.000)
Industry Fixed	Yes	Yes	No	No
Year Fixed	No	No	Yes	Yes
Firm Fixed	No	No	Yes	Yes
N	4457	4457	4457	4457
Adj R-Square	0.588	0.638	0.807	0.824

Panel B: Two control groups

	Control Firms: No Option Awards		Control Firms: Early Adopters	
	(1)	(2)	(3)	(4)
Treat	0.132***		0.058	
	(0.009)		(0.479)	
Post	0.060		0.088	
	(0.364)		(0.392)	
Treat × Post	−0.172**	−0.084*	−0.211**	−0.122**
	(0.012)	(0.081)	(0.045)	(0.043)
Firm Characteristics	Yes	Yes	Yes	Yes

(*continued on next page*)

Table 3 (*continued*)

Panel B: Two control groups

	Control Firms: No Option Awards		Control Firms: Early Adopters	
	(1)	(2)	(3)	(4)
Loan Characteristics	Yes	No	Yes	No
Industry Fixed	Yes	No	Yes	No
Year Fixed	No	Yes	No	Yes
Firm Fixed	No	Yes	No	Yes
N	4138	4138	3962	3962
Adj R-Square	0.582	0.804	0.601	0.818

This table presents the results estimating the following equation

$$Y_{it} = \alpha + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i \times Post_t + \gamma X_{it} + \tau Trend_t + \varepsilon_{it},$$

The dependent variable is *LogSpread*, the natural logarithm of all-in-drawn; the key independent variable, i.e., the difference-in-differences term, is *Treat* x *Post*, in which *Treat* equals one for treated firms, and equals zero otherwise, and *Post* equals one after the implementation of FAS 123R and zero otherwise. Other control variables are as defined in the Appendix. Statistics in parentheses are *p*-values clustered at the firm level. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

4.4. Threats to identification

The assignment of control and treated firms may not be random, and systematic differences between treated and control firms may result in differential trends in loan spreads, violating the parallel trend assumption necessary for the DID analysis. If treated and control firms experience a gradual divergence in the costs of debt over time due to unknown reasons, the DID estimate in Eq. (1) will capture this increasing differential in loan spreads even in the absence of the treatment effect. For example, treated firms may be more risk-averse and tend to improve their credit risk over time even in the absence of FAS 123R, and our difference-in-differences tests may simply pick up the trend of improving credit risk due to treated firms' inherent risk attitude.

Our two control groups differ along many dimensions. The very fact that one control group issues and expenses the fair value of stock options, while the other control group does not use options at all, makes it unlikely that the two control groups share the same bias that may cause similar different trends in loan spreads between treated and control firms. Hence, observing the same estimation for both control groups as shown in Panel B of Table 3 suggests that the parallel trend assumption likely holds in our sample. Nevertheless, in the next section, we conduct further tests to alleviate this concern.

4.4.1. Moving trends of loan spreads

First, we follow Roberts and Whited (2013) and provide a visual examination by comparing the trends of the demeaned natural logarithm of loan spreads for treated and control firms. Specifically, we first calculate for each firm the average log loan spreads over all facilities made to the firm and then obtain the difference between individual log loan spreads and the average log loan spreads. We then calculate the cross-sectional average of the demeaned log loan spreads for all treated and control firms separately. We use the demeaned log spreads because our sample firms are different from year to year, and firm-specific factors of different firms can cause the annual cross-sectional means of loan spreads to change dramatically from one year to the next.[16]

As shown in Panel A of Fig. 1, in which we pool the two control groups together, the trends for both treated and control firms indeed move in parallel prior to FAS 123R. However, upon the implementation of FAS 123R, the two trends diverge abruptly with loan spreads trending downward for the treated firms from 2006 to 2007. In contrast, in the same period, control firms' loan spreads increase slightly in 2006 before they decrease to the 2005 level in 2007. Both control and treated firms' loan spreads increase sharply in 2008 due to the financial crisis. Panels B and C of Fig. 1 present the same moving trend comparison using control firms of no option awards (Panel B) and control firms of early adopters (Panel C). Overall, the two graphs present a similar pattern as in Panel A.[17]

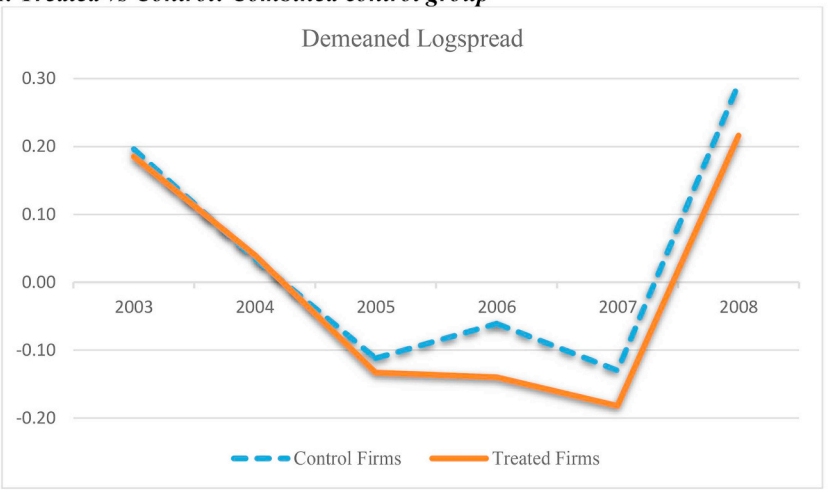
4.4.2. Placebo tests

In this section, we conduct a placebo test to further show the validity of our DID analysis. Specifically, we create a placebo experiment and use 2000–2002 as the pre-event period and 2003–2005 as the post-event period. We maintain the same assignment of

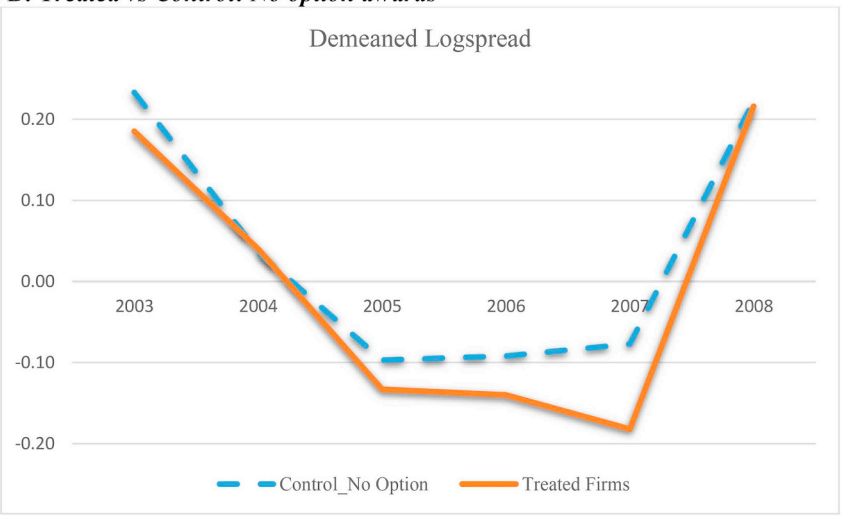
[16] Using unadjusted loan spreads provides qualitatively similar patterns and the graph is not presented

[17] Panel A of Figure 1 shows an increase in loan spreads in 2006 for the control firms. This increase is mostly driven by control firms that are early adopters as shown in Panel C of Figure 1. To rule out the possibility that this increase in loan spreads is unique to our early adopter control firms, we also examine spreads for loans that are not borrowed by our sample firms based on DealScan data. Specifically, we find match loans based on loan amount, maturity, type, whether the loan is secured, and borrowing year (we allow a two-way 5% margins for continuous matching variables). Unreported results show a similar upward tick in demeaned loan spreads for the matched loans as well. More importantly, our results are robust after excluding early adopter control firms from the sample as shown in Panel B of Table 2.

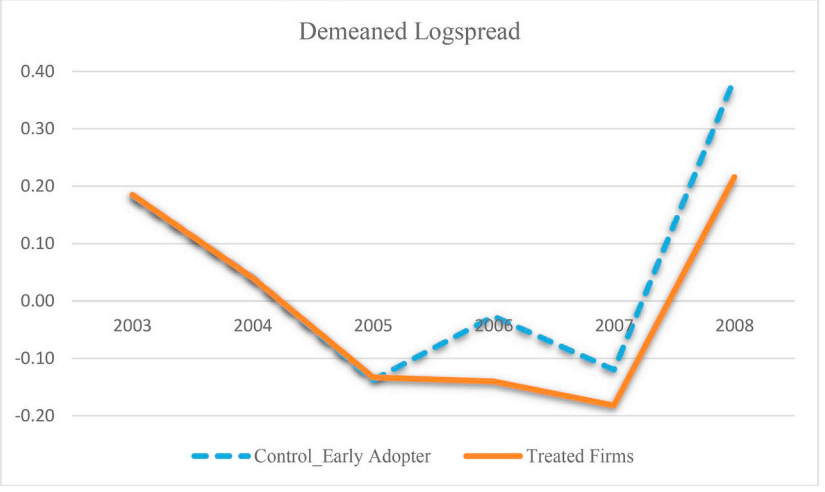
Panel A: Treated vs Control: Combined control group



Panel B: Treated vs Control: No option awards



Panel C: Treated vs. Control: Early adopters



(caption on next page)

Fig. 1. Demeaned log spread of treated and control firms.
Panel A: Treated vs Control: Combined control group
Panel B: Treated vs Control: No option awards
Panel C: Treated vs. Control: Early adopters
This figure presents the time trend of demeaned log value of loan spreads for the treated and control firms separately. We calculate for each firm the average log loan spreads over all facilities made to the firm during the sample period, and then calculate for each facility the demeaned log loan spreads as the difference between the log loan spreads and the average log loan spreads. Last, we calculate the cross-sectional average of the demeaned log loan spreads for all treated and control firms separately.

Table 4
Placebo tests: The impact of the placebo event

	(1)	(2)
Treat	−0.008	−0.008
	(0.888)	(0.862)
Post_Placebo	−0.091	−0.076
	0.194)	(0.183)
Treat × Post_Placebo	0.026	0.049
	(0.689)	(0.381)
Firm Characteristics	Yes	Yes
Loan Characteristics	No	Yes
Industry Fixed	Yes	Yes
N	5149	5149
Adj R-Square	0.604	0.691

This table presents the results of estimating the following equation

$$Y_{it} = \alpha + \beta_1 Treat_i + \beta_2 Post\_PLacebo_t + \beta_3 Treat_i \times Post_t + \gamma X_{it} + \tau Trend_t + \varepsilon_{it}$$

We use the year 2003 as the placebo event year. This test uses the same treated and control firms as in Table 3 but with a sample covering 2000 to 2005. The dependent variable is *LogSpread*, the natural logarithm of all-in-drawn; the key independent variable, i.e., the difference-in-differences term, is *Treat* × *Post_Placebo*, in which *Treat* equals one for treated firms, and equals zero otherwise, and *Post_Placebo* equals one after the fiscal year 2003, placebo event, and zero otherwise. Other control variables are as defined in the Appendix. Statistics in parentheses are *p*-values clustered at the firm level. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

firms into the treated and control groups as in our baseline tests. We then use the same DID method to examine whether the placebo experiment has any effect on loan spreads. If our results are driven by the differential trends in loan spreads between control and treated firms, we should expect to observe a similar treatment effect by the placebo event as shown in the baseline results. The results are presented in Table 4. The placebo DID estimates are all statistically insignificant and are much smaller in magnitude than those in Table 3. Hence, there is no trend difference in loan spreads for treated and control firms around the placebo event.

5. Additional tests

5.1. Cross-sectional heterogeneity in the effect of FAS 123R

After documenting the evidence that FAS 123R decreases the agency cost of debt related to risk-shifting conflicts for treated firms, in this subsection, we explore whether the effect of FAS 123R on loan spreads varies with the extent of risk-shifting conflicts faced by treated firms. We identify a scenario in which the agency cost of debt due to CEOs' risk-shifting incentives is expected to be more severe. Shareholder/managerial risk-shifting becomes exacerbated when the firm is in financial distress or is closer to bankruptcy (e.g., Jensen and Meckling, 1976; Smith and Warner, 1979; Aghion and Bolton, 1992; Nini et al., 2009). It is therefore expected that the impact of FAS 123R in reducing the agency costs of debt should be more pronounced for firms closer to bankruptcy. To test the conjecture, we follow Bharath and Shumway (2008) to construct Merton's distance-to-default measure. We then partition the sample according to whether the distance-to-default measure is below or above the sample median and re-estimate Eq. (1) on these two subsamples.

The results are presented in Table 5. Consistent with our conjecture, the difference-in-differences estimates are negative and statistically significant in Columns (1) and (3), for firms with high default risk, i.e., for firms with the distance-to-default measure below the sample median. On the other hand, the estimates are much smaller in magnitudes and are statistically insignificant in Columns (2) and (4), for firms with low default risk, i.e., for firms with the distance-to-default measure above the sample median. Furthermore, the Chi-square tests show that the differences between high and low default risk firms are statistically significant. The results further confirm that the effect is indeed driven by the decrease in risk taking incentives.

Table 5
The effect of default risk.

	High	Low	High	Low
	(1)	(2)	(3)	(4)
Treat	0.156**	0.031	0.175***	0.047
	(0.016)	(0.628)	(0.007)	(0.436)
Post	0.084	0.043	0.096	0.074
	(0.301)	(0.609)	(0.211)	(0.356)
Treat x Post	−0.257***	−0.091	−0.261***	−0.124
	(0.004)	(0.303)	(0.002)	(0.144)
Chi-Squared of testing diff.	5.06**		3.86**	
p-value of testing diff.	0.024		0.049	
Firm Characteristics	Yes	Yes	Yes	Yes
Loan Characteristics	No	No	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
N	2068	2389	2068	2389
Adj R-Square	0.476	0.584	0.546	0.638

This table presents the results estimating the equation

$$Y_{it} = \alpha + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i \times Post_t + \gamma X_{it} + \varepsilon_{it}$$

on subsamples partitioned on Merton's distance-to-default measure. The dependent variable is *LogSpread,* the natural logarithm of all-in-drawn; the key independent variable, i.e., the difference-in-differences term is *Treat* × *Post,* in which *Treat* equals one for treated firms, and equals zero otherwise, and *Post* equals one after the implementation of FAS 123R and zero otherwise. Columns (1) and (2) present the results with firm characteristics only and Columns (3) and (4) present the results with both firm and loan characteristics. Other control variables are as defined in the Appendix. Statistics in parentheses are p-values clustered at the firm level. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

5.2. Changes in option compensation and the effects of FAS 123R on loan spreads

The reduction in risk-taking after FAS 123R arises from the decrease in the use of stock options. To further test that FAS 123R indeed reduces managers' risk-taking incentives, we check whether the reduction in the loan spreads after FAS 123R is more pronounced for treated firms that experience the largest decrease in option compensation.

For this purpose, we partition the treated firms into quartiles based on changes in option compensation surrounding FAS 123R. For each treated firm, we obtain the average option compensation both before FAS 123R and after FAS 123R and measure the changes in option compensation as the difference between the two averages. Treated firms in the bottom quartile are those that have the largest decrease in option compensation and treated firms in the top quartile are those with the least decrease in option compensation. We then estimate the DID model separately for the two subgroups of treated firms with the control firms unchanged. We expect the DID estimate to be more pronounced for the treated firms in the bottom quartile than those in the top quartile, which would suggest that firms with larger decreases in option compensation enjoy a higher reduction in loan spreads after FAS 123R.

The results are reported in Table 6. Consistent with our expectation, we indeed find that the decrease in loan spreads becomes more pronounced for treated firms experiencing the largest decrease in option awards. For example, the DID estimate is −0.130 and statistically significant at the 10% level for the top quartile treated firms as reported in Column (1) and, in contrast, the same DID estimate is −0.204 and statistically significant at the 5% level for the bottom quartile treated firms as shown in Column (2). The difference between the two DID estimates is statistically different at the 5% level. In Columns (3) and (4), when loan characteristics are included, we obtain similar results.

5.3. Accounting quality as an alternative explanation

Prior studies argue that, by requiring fair value recognition of stock options, FAS 123R can improve the usefulness of borrowers' accounting information in debt contracting and reduce the cost of debt. For example, Barth et al. (2012) find that recognized option compensation expense is informative in predicting firm future performance for the majority of their sample firms. As such, the increased informativeness of accounting information may help better predict borrower credit risk and improve the usefulness of accounting information in loan contracting (Lambert et al., 2007; Bharath, Sunder, and Sunder, 2008; Zhang, 2008).[18] Hence, an

[18] Prior studies argue that the adoption of fair value accounting can potentially reduce the usefulness of accounting information for debt holders. This is so because high uncertainty and managers' discretion in the estimation of fair values can reduce the reliability of accounting information to lenders (Ball et al., 2015; Demerjian et al., 2016; Ertan and Karolyi, 2017). Empirical studies, however, provide mixed results. For instance, studying the implementation of FAS 159, which adopts the fair value accounting for financial assets, Ertan and Karolyi (2017) find that the use of financial covenants is reduced, suggesting that the usefulness of fair value accounting is reduced for debt holders. Ball et al. (2015) find that after the adoption of IFRS, which broadens the use of fair value accounting, the use of financial covenants is reduced. In contrast, Demerjian et al. (2016) find no change in the use of financial covenants after FAS 159.

Table 6
Changes in option compensation and the effects of FAS 123R on loan spreads.

	Quartiles Partition Based on Changes in Treated Firms' Option Compensation			
	Top	Bottom	Top	Bottom
Treat	0.161***	0.081	0.149***	0.110
	(0.003)	(0.286)	(0.005)	(0.126)
Post	0.024	0.042	0.039	0.074
	(0.692)	(0.480)	(0.516)	(0.212)
Treat × Post	−0.130*	−0.204**	−0.126*	−0.212***
	(0.080)	(0.011)	(0.068)	(0.004)
F-Test (Top = Bottom)	3.15**		3.85**	
Firm Characteristics	Yes	Yes	Yes	Yes
Loan Characteristics	No	No	Yes	Yes
Industry Fixed	Yes	Yes	Yes	Yes
N	1603	1530	1603	1530
Adj R-Square	0.639	0.604	0.668	0.621

This table presents the results estimating the following equation:

$$Y_{it} = \alpha + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i \times Post_t + \gamma X_{it} + \tau Trend_t + \varepsilon_{it.}$$

The dependent variable is *LogSpread*, the natural logarithm of all-in-drawn; Treat, is an indicator variable which equals one if a firm experiences a decrease in option compensation after FAS 123R and zero otherwise, *Post* equals one after the implementation of FAS 123R and zero otherwise. Other control variables are as defined in the Appendix. Statistics in parentheses below the coefficient estimates are p-values clustered at the firm level. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

alternative interpretation of our results is that improved accounting information leads to reduced information risk and therefore reduced cost of debt rather than reduced risk-taking incentives. However, the effect of financial reporting quality should not depend on whether option grants indeed decrease. In Section 5.2, we show that the effect of FAS 123R is more pronounced for treated firms that experience the larger decrease in option compensation, also inconsistent with the financial reporting quality story. Nevertheless, in this section, we examine the effect of FAS 123R on the use of loan covenants to infer whether the reduction in loan spreads is driven by the improvement in accounting quality or by the reduction in risk-taking.

Under the accounting quality explanation, the increase in accounting quality will improve the usefulness of accounting information in loan contracting because covenants written based on accounting information will become more effective in protecting lenders from the detrimental behavior of borrowers. In fact, the literature documents that contracting parties will use more financial covenants when the quality of financial information is high (Ball et al., 2008; Nikolaev, 2010; Costello and Wittenberg-Moreman, 2011). If accounting information becomes more informative about borrowers' credit risk, the use of financial covenants should increase after FAS 123R. To test this hypothesis, we focus on the number of financial covenants contained in loan contracts around FAS 123R. Following prior studies, we use the following covenants as financial covenants: interest, cash, or debt coverage ratios, net-worth covenants, current or quick ratios, leverage, and equity asset ratios. In addition, we also narrow down the focus and examine earnings-based financial covenants to account for the possibility that FAS 123R will have a direct effect on earnings and therefore the use of earnings-based financial covenants. Examples of such covenants include levels of EBITDA, the debt to EBITDA ratio, and the senior debt to EBITDA ratio.

We estimate the DID model using the number of financial (*NumFinCov*) or earnings-based financial covenants (*NumEarnCov*) as the dependent variables. The results are reported in Panel A of Table 7. The DID estimates are statistically insignificant and the use of financial covenants and earnings-based financial covenants does not change after FAS 123R for treated firms. Hence, the decrease in loan spreads after FAS 123R cannot be explained by the accounting quality channel. This result is consistent with the fact that firms were already required to disclose the fair value of option compensation in footnotes prior to FAS 123R.

Next, we examine whether the decrease in loan spreads is consistent with the risk-taking explanation. When faced with strong risk-taking incentives by either shareholders or managers, creditors often demand covenants to curb managerial risk-taking behavior (Jensen and Meckling, 1976; Smith and Warner, 1979; and Nini et al., 2009). For instance, creditors restrict capital expenditures to protect them from potential risk-shifting behavior (Nini et al., 2009). If FAS 123R indeed decreases managerial risk-taking incentives, the likelihood of using these covenants restricting risk-taking behavior should be lower. After all, these covenants, while curbing risk-taking behaviors, also put unnecessary restrictions that may prevent firms from making optimal decisions (Bradley and Roberts, 2015).

Empirically, we focus on two types of covenants that can help mitigate managerial risk-shifting incentives: loan collateral and covenant restricting capital expenditure (Jensen and Meckling, 1976; Smith and Warner, 1979; Bradley and Roberts, 2015; Nini et al., 2009). Specifically, we define two indicator variables: *Secured*, which equals one if a loan is secured, and *MaxCapex*, which equals one if a loan has a covenant restricting maximum capital expenditure. Because the two dependent variables are dummy variables, we use the logit model to examine the effects of FAS 123R on the probability that a loan contract contains these two types of covenants. The results are presented in Panel B of Table 7.

In Columns (1) and (2) of Panel B, Table 7, the DID estimates are negative and statistically significant at the 10% level or lower,

Table 7
The effects of FAS 123R on loan covenants

Panel A: OLS regressions of financial covenants

	NumFinCov		NumEarnCov	
	(1)	(2)	(3)	(4)
Treat	0.018	−0.129**	0.041	−0.067
	(0.908)	(0.035)	(0.687)	(0.148)
Post	−0.149	−0.115	−0.102	−0.166***
	(0.190)	(0.131)	(0.126)	(0.117)
Treat ×Post	−0.031	0.028	0.015	0.050
	(0.731)	(0.661)	(0.962)	(0.354)
Firm Characteristics	Yes	Yes	Yes	Yes
Loan Characteristics	No	Yes	No	Yes
Industry Fixed	Yes	Yes	Yes	Yes
N	4457	4457	4457	4457
Adj R-Square	0.228	0.670	0.241	0.628

Panel B: Logistic regression of risk-constraining covenants

S	Secured		MaxCapex	
	(1)	(2)	(3)	(4)
Treat	0.451**	0.408	0.0521	−0.174
	(0.034)	(0.132)	(0.834)	(0.528)
Post	0.447*	0.636**	−0.169	−0.291
	(0.057)	(0.026)	0.546)	(0.352)
Treat × Post	−0.458*	−0.630**	−0.407*	−0.291**
	(0.081)	(0.027)	(0.058)	(0.045)
Firm Characteristics	Yes	Yes	Yes	Yes
Loan Characteristics	No	Yes	No	Yes
Industry Fixed	Yes	Yes	Yes	Yes
N	4457	4457	4457	4457
Pseu. R-Square	0.329	0.529	0.237	0.436

This table presents the results of the effects of FAS 123R on financial covenants based on OLS regression in Panel A and on risk-constraining covenants based on logit regression in Panel B. *NumFinCov* is the number of financial covenants; *NumEarnCov* is the number of earnings-based financial covenant; *Secured* equals one for secured loans and zero otherwise; *MaxCapex* equals one if the loan contains covenants restricting maximum capital expenditure and zero otherwise; The key independent variable, i.e., the difference-in-difference term, is *Treat* x *Post*, in which *Treat* equals one for treated firms, and equals zero otherwise, and *Post* equals one after the implementation of FAS 123R and zero otherwise. Other control variables are as defined in the Appendix Statistics in parentheses below the coefficient estimates are p-values clustered at the firm level. Significance levels at 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

suggesting that FAS 123R reduces the pledge of collateral for loans made to treated firms relative to control firms. The effect is also economically significant. For instance, the marginal effect estimate of Column (1) suggests that FAS 123R reduces the probability of pledging collateral by treated firms by almost 12%. In Columns (3) and (4), we examine the effect of FAS 123R on the use of *MaxCapex*. The DID estimates are negative and statistically significant at the 10% level or lower. The marginal effect of the estimate from Column (3) suggests that FAS 123R decreases the probability of the investment restriction covenant by about 3%. Given that only about 16% of the loans in our sample have the investment restriction covenant, the effect is substantial. Hence, the results suggest that the reduction in loan spreads is likely driven by the reduction in CEOs' risk-taking incentive of treated firms.

5.4. The propensity score matched sample

As shown in Table 1, treated and control firms differ significantly in some firm characteristics. Although the DID method allows treated and control firms to be different as long as the parallel trend condition holds (see, e.g., Roberts and Whited, 2013), we conduct an additional test to address the potential concern that our results may be driven by the differences between treated and control firms. To do so, we match treated firms with control firms based on propensity scores by estimating a logit equation predicting the probability of a firm being a treated firm. The predicting variables we include are those included in Eq. (1). We find for each control firm (our sample consists of 205 control firms and over 939 treated firms) a treated firm with the nearest propensity score, and obtain a total of 205 matched pairs. Panels A and B of Table 8 present the results comparing treated firms with control firms in the years prior to FAS 123R. Panel A shows the comparison between treated firms and control firms with no option grants prior to FAS 123R and Panel B with control firms adopting fair value accounting early. As shown in both panels, the matched treated and

Table 8
Propensity score matched sample

Panel A: Control firms with no option awards

	Treated Firms		Control Firms No Option Awards		Mean Diff.
	Mean	Median	Mean	Median	T – C
Size	7.299	7.236	7.359	7.225	−0.060
TobinQ	1.776	1.500	1.677	1.425	0.099
ROA	0.154	0.127	0.139	0.125	0.015
LReturn	0.459	0.192	0.404	0.194	0.055
SalesGrowth	0.191	0.128	0.153	0.095	0.038
Leverage	0.257	0.228	0.286	0.242	−0.029
Tangibility	0.639	0.543	0.581	0.545	0.058
MissingRD	0.396	0.000	0.584	1.000	−0.188*
RDExp	0.012	0.000	0.011	0.000	0.001
CapExp	0.078	0.034	0.060	0.029	0.018
PayDiv	0.448	0.000	0.433	0.000	0.015
Rating_IG	0.201	0.000	0.191	0.000	0.010

Panel B: Control firms as early adopters

	Treated Firms		Control Firms Early Adopters		Mean Dif.
	Mean	Median	Mean	Median	T – C
Size	9.430	9.268	9.585	9.802	−0.055
TobinQ	1.579	1.473	1.530	1.326	0.049
Leverage	0.274	0.268	0.344	0.295	−0.070
ROA	0.146	0.137	0.068	0.102	0.078
LReturn	0.077	0.074	0.132	0.283	−0.055
SalesGrowth	0.083	0.079	0.080	0.079	0.003
Tangibility	0.678	0.582	0.679	0.745	−0.001
RDExp	0.012	0.000	0.012	0.000	0.000
MissingRD	0.481	0.000	0.401	0.000	0.080
CapExp	0.065	0.038	0.078	0.044	−0.013
PayDiv	0.627	1.000	0.675	1.000	0.055
Rating_IG	0.486	0.000	0.472	0.000	0.014

Panel C: DID analysis

	Whole Sample	
	(1)	(2)
Treat	0.113*	0.126**
	(0.082)	(0.035)
Post	0.013	0.033
	(0.827)	(0.552)
Treat × Post	−0.130*	−0.130**
	(0.080)	(0.046)
Firm Characteristics	Yes	Yes
Loan Characteristics	No	Yes
Industry Fixed	Yes	Yes
N	1707	1707
Adj R-Square	0.566	0.647

This table presents the results estimating the following equation using propensity score matched sample:

$$Y_{it} = \alpha + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i \times Post_t + \gamma X_{it} + \tau Trend_t + \varepsilon_{it}$$

The dependent variable is *LogSpread*, the natural logarithm of all-in-drawn; Treat, is an indicator variable which equals one if a firm experiences a decrease in option compensation after FAS 123R and zero otherwise, *Post* equals one after the implementation of FAS 123R and zero otherwise. Other control variables are as defined in Appendix. P-values clustered at the firm level are reported in parentheses below the coefficient estimates. Significance levels at 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

control firm characteristics are very similar, as most of the mean differences are small and statistically insignificant with the exceptions of the percentage of firms missing R&D in Panel A. Hence, the propensity score matched pairs are very similar to each other in the measured firm characteristics.

In Panel C of Table 8, the DID estimates remain negative and statistically significant. For example, the DID coefficient in Column (2) is −0.130 and statistically significant at the 5% level. Hence, our results are robust after explicitly controlling for differences in firm characteristics that can potentially drive the option compensation policy as well as the costs of debt.

6. Conclusion

We examine the effect of management stock options on risk taking. We exploit exogenous variation in stock options generated by the implementation of FAS 123R, which increases the cost of option grants, to deal with the endogeneity of stock options. In addition, we overcome the difficulty of measuring firm risk-taking and use loan spreads to infer firm risk as spreads can capture more corporate risk taking. Using a difference-in-differences framework, we find that firms affected by FAS 123R, our treated firms, experience significant reductions in the cost of debt after FAS 123R relative to firms that are likely unaffected by FAS 123R, our control firms. The results suggest that option compensation increases managerial risk taking, and hence increases cost of loans.

Appendix: Variable Definitions

Dependent Variables: Loan Characteristics

LogSpread: Natural logarithm of all-in-drawn facility spread, which is the total cost paid over LIBOR for each dollar drawn down under the loan commitment;
MaxCapex: A dummy variable equal to one if a loan contains maximum capital expenditure covenants;
NumFinCov: Number of financial covenants;
NumEarnCov: Number of earnings-based financial covenants;
Secured: A dummy variable equal to one if a loan is secured and zero otherwise;

Dependent Variables: CEO Option Awards

Bonus_TotalComp: The ratio of cash bonus over total compensation for a CEO;
LTIA_TotalComp: The ratio of fair value of long-term incentive awards over total compensation for a CEO;
Log_TotalComp: Natural logarithm of fair value of total compensation to a CEO;
Option_TotalComp: The ratio of fair value of option awards over total compensation for a CEO;
RS_TotalComp: The ratio of fair value of restricted stocks over total compensation for a CEO;
Salary_TotalComp: The ratio of salary over total compensation for a CEO;

Control Variables: Firm Characteristics

Capexp: The capital expenditures (CAPX) divided by sales (SALE);
Leverage: Firm book leverage, calculated as the book value of debt over the book value of assets (DLC + DLTT) / (AT);
LReturn: Prior year's annual stock return;
MissingRD: A dummy variable equal to one if R&D expense is missing;
PayDiv: A dummy variable equal to one if a firm pays common dividends (DVC) and zero otherwise;
Post: A dummy variable equal to one if a firm's fiscal year falls in 2006, 2007, or 2008, and zero if a firm's fiscal year falls in 2003, 2004, or 2005;
ROA: The operating income before depreciation (OIBDP) as a percentage of the firm's assets (AT);
Rating_IG: A dummy variable equal to one if a firm has an investment grade credit rating (SPLTICRM) and zero otherwise;
RDExp: The R&D expense (XRD) over sales (SALE) and R&D expenses, set equal to zero when missing;
SaleGrowth: Changes in sales (SALE) from year t-1 to year t over year t sales;
Size: Natural logarithm of assets (AT);
Tangibility: The gross property, plant, and equipment (PPEGT) divided by total assets (AT);
Treated: A dummy variable equal to one if a firm issued stock options prior to and at the same time adopted FAS 123R in the fiscal year 2006 and zero otherwise;
TobinQ: Market value of assets to book value of assets (PRCC_F * SCHO + AT - CEQ)/AT;

Control Variables: Loan Characteristics

LogCovIntensity: Natural logarithm of the sum of (1) multiple financial covenants (equal to one if the loan contains two or more financial ratio covenants), (2) secured, (3) asset sales sweep, (4) debt issuance sweep, (5) equity issuance sweep, and (6) dividend restriction;
LogAmount: Natural logarithm of facility amount in millions;

LogMaturity: Natural logarithm of facility maturity in months;
PerPricing: A dummy variable equal to one if a bank loan contains performance pricing covenants and zero otherwise;

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