The Effects of Short-Selling Threats on Incentive Contracts: Evidence from a Natural Experiment

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August 19, 2014

Abstract

This paper examines the effects of short-selling on the design of executive incentive contracts. Using a randomized experiment, we find evidence that the removal of short-selling constraints affects firms' contracting environment by increasing downside risk. Treated firms reduce managerial exposure to this risk by granting relatively more stock options to their executives and adopting new anti-takeover provisions. This evidence supports recent models that rationalize the use of convex compensation payoffs and highlights the importance of financial markets in the design of incentive contracts.

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Existing theories predict that short-selling activity can affect the dynamics of stock prices in several ways. For example, Diamond and Verrecchia (1987) show that unrestrained short-selling can improve the informativeness of stock prices by impounding negative information from pessimistic investors. On the other hand, Goldstein and Guembel (2008) argue that unrestrained short-selling can exert downward pressure on stock prices by increasing the probability of bear raids and stock price manipulations. Given that these changes in stock price dynamics can affect the contracting environment between investors and managers, short-selling activity can have a significant impact on the design of equity-based compensation. This paper examines this issue.

Principal-agent theories predict opposing optimal responses to exogenous changes in short-selling activity. According to Holmstrom (1979), firms should increase managerial exposure to stock prices if short-selling improves price informativeness. Alternatively, Holmstrom and Milgrom (1987) predict that firms should do the opposite if short-selling activity increases the risk borne by managers. Consistent with the latter model, we find that an exogenous shock to short-selling activity increases downside risk - the risk of large negative stock price jumps - which causes firms to reduce managerial exposure to this risk by granting relatively more stock options.

Due to the endogenous nature of short-selling, identifying the causal effects of this activity on the design of managerial incentives is empirically challenging. To address this issue, we use Regulation SHO (Reg SHO), which removes restrictions on short sales for a randomly selected sample of firms (pilot firms). Specifically, this regulatory change eliminates the uptick rule, which was designed by the SEC to eliminate stock price manipulation and prevent investors from shorting stocks when stock prices decline.[1,2] Reg SHO provides an ideal setting to test our hypotheses because recent studies show that this regulation caused a large exogenous increase in short-selling activity (SEC's OEA (2007), Diether, Lee, and Werner (2009), Grullon, Michenaud, and Weston (2014)).

We use a difference-in-differences (DiD) approach to identify the causal effects of short-selling on the design of executive incentives. We find a change in the structure of new equity grants (composed of stock options and restricted stocks) but not in their total value. Pilot firms subject to unrestricted short-selling increase the proportion of stock options by 8%. Given that most of the new option awards have a relatively short vesting period, this change directly affects managerial incentives during the two-year period of the experiment.[3] The difference in the structure of new equity grants between pilot and control firms persists over the entire duration of the experiment and disappears immediately following the repeal of the uptick rule on all stocks in 2007.[4] We also study other pecuniary and non-pecuniary forms of incentive contracts and find that pilot firms adopt new anti-takeover provisions (staggered boards and supermajority rules) and provide new severance packages.

[1] On the NYSE, Rule 10a-1 of the Exchange Act only allowed short sales on plus ticks or zero plus ticks, while on the NASDAQ, NASD Rule 3350 prohibited short sales below the bid if the last bid was a down bid. These rules had been in place since 1938 on NYSE and 1994 on Nasdaq.

[2] The objectives of the uptick rule as stated by the SEC are: "allowing relatively unrestricted short-selling in an advancing market; preventing short-selling at successively lower prices, thus eliminating short-selling as a tool for driving the market down; and preventing short sellers from accelerating a declining market by exhausting all remaining bids at one price level, causing successively lower prices to be established by long sellers." Securities Exchange Act Release No. 13091 (December 21, 1976).

[3] Bettis et al. (2013) show that about 80% of the time-vesting option awards exhibit a ratable vesting (i.e. vest uniformly over a given period) and that most of the awards display a 3-year or 4-year vesting period. This would imply that approximately half to two thirds of the new option awards vest during the experiment.

[4] We stop our analysis before the financial crisis to avoid any confounding effect related to this event.

To identify the economic channel(s) through which short-selling affects firms' compensation policy, we first explore the effect of Reg SHO on risk and informativeness.[5] Our findings indicate that Reg SHO appears to affect risk asymmetrically by increasing downside risk only (also called left-tail risk) thus allowing to test the tradeoff between risk and incentives predicted in principal-agent models (e.g. Holmstrom and Milgrom (1987)) locally on the left-tail. We first replicate Grullon et al. (2014) in our sample and find that pilot firms exhibit more negative returns on bad-market days but no effect on good-market days. We also use option data to show that Reg SHO increases the volatility skew of pilot firms' put options but not that of call options, suggesting that investors anticipate negative jumps in stock prices.[6] These results are consistent with the bear-raid risk argument in Goldstein and Guembel (2008) and Goldstein, Ozdenoren and Yuan (2013), and thus support the risk channel. In contrast, we do not find that stock price informativeness improves during the experiment using proxies such as the probability of informed trading (Easley, O'Hara, and Hvidkjaer (2002), Brown and Hillegeist (2007)) and R^2 (Roll (1988)).

To further disentangle the channel of causation, we examine how the cross-sectional variation in risk, informativeness and governance affects the structure of compensation contracts. Using a differences-in-differences-in-differences approach, we find that firms with the largest increase in downside risk (the proportion of stock options increases on average by 17% for firms in the top quintile) drive our results. In contrast, our main result does not appear to be driven by firms with weak governance or firms with improved informativeness, suggesting that the informativeness channel is unlikely to be responsible for our findings.

[5] As exposed in Fang, Huang, and Karpoff (2014), an increase in external monitoring by short-sellers leads to more informative stock prices. In this paper, we broaden the notion of informativeness to include monitoring. See also Karpoff and Lou (2010) for a similar argument.

[6] The volatility skew of puts (calls) has been shown to proxy for large expected negative (positive) jumps in individual stocks (Xing, Zhang and Zhao (2010)) and in indices (e.g., Bollen and Whaley (2004)).

In general, our results indicate that firms decrease managerial exposure to risks associated with short-selling by shifting toward stock options and away from restricted stocks.[7] By changing the structure of new equity grants and decreasing managerial exposure to the adverse effects associated with the increased probability of hostile takeovers (Edmans, Goldstein, and Jiang (2012)) and dismissals (Peters and Wagner (2013)), firms reduce the amount of idiosyncratic risk borne by their managers, and thus the expected compensation costs (Holmstrom and Milgrom (1987)). Alternatively, if the informativeness channel was at play, firms should have taken advantage of the information impounded by short-sellers and thus increased managerial exposure to short-selling. Increased use of restricted stocks, not stock options, would have been expected (Holmstrom (1979)).

Overall, our interpretation of the results is in line with recent evidence that managers are averse to short-sellers.[8] For example, Lamont (2012) shows that CEOs go to great lengths to fight short-sellers and limit their influence on stock prices. The uptick rule itself, initially designed to "eliminate short-selling as a tool for driving the market down," was perceived as an important protection against manipulative short-selling practices.[9] Consistent with this view, extensive anecdotal evidence suggests that firms were aware of the experiment, and fearful of its impact on stock price volatility due to concerns about bear-raids.[10] Such concerns are likely exacerbated for top executives whose wealth and human capital is closely tied to firm performance, and with limited flexibility to diversify away risk.

[7] Stock options, with their floor at zero, are less sensitive to changes in downside risk than restricted stocks.
[8] Even absent a measurable effect on risk, CEOs' perception of risk would generate the effects we identify.
[9] In a survey conducted by NYSE and NASDAQ in 2008 on 438 CEOs, CFOs, and investor relations officers, 85% of all respondents were in favor of "re-instituting the "tick-test" rule as soon as practical."
[10] For instance, Darla C. Stuckey, Corporate Secretary of the NYSE, wrote to the SEC during the consultation period: "The Exchange, its members and its listed companies strongly support continued price restrictions for short sales in all securities. These restrictions, whether by a "tick" test or a "bid" test, dampen undue volatility and prevent unscrupulous market participants from forcing stock prices lower for reasons unrelated to actual market forces."

As a consequence, the removal of the uptick rule should induce managers to decrease firm risk in order to limit their exposure to short-selling. Consistent with this conjecture, Grullon et al. (2014) show that treated firms reduce investment and equity issuance. These real outcomes are likely to be suboptimal for investors who can diversify away short-selling risk. In that case, firms should encourage risk-taking by increasing the convexity of the compensation payoff (Jensen and Meckling (1976)).[11] Our main result is consistent with this prediction. In addition, we find that pilot firms that respond most aggressively to the removal of the uptick rule experience the largest increase in investment.[12]

Our interpretation of the results is also in line with recent models that rationalize the use of convex compensation payoff. Dittmann, Maug, and Spalt (2010) argue that the presence of options can be justified by CEOs' loss-aversion.[13] To the extent that downside risk is related to loss-aversion, our results support their argument.[14, 15] Finally, our findings are also consistent with Hemmer, Kim, and Verrecchia (2000), who show that the convexity in the compensation payoff is related to the skewness of the price distribution, which is arguably a measure of downside risk.

We perform a number of robustness tests. First, we test whether a decrease in stock prices leads firms to reload managers' incentives. We show that firms that exhibit large negative announcement returns do not drive our main results. We also re-run our analysis using the

[11] This view is controversial. Ross (2004) shows that a convex compensation payoff does not necessarily induce greater risk-taking incentives. It depends on the agent's utility function. See also Carpenter (2000).

[12] We cannot rule out that firms provide more risk-taking incentives via stock options *because* they have more investment opportunities, and thus we are cautious not to draw any causal inferences from this analysis.

[13] Dittmann and Maug (2007) show that it is difficult to explain the presence of stock options in optimal compensation contracts using standard principal-agent models.

[14] More formally, downside risk can be made observationally equivalent to loss-aversion in their model if two distributions of these variables can generate the same distribution of optimal contracts. See, e.g., Koopmans (1949) and Dufour and Hsiao (2008) for a discussion of observational equivalence in modeling and econometrics.

[15] Consistent with this argument, the magnitude of our main results is in line with the optimal piecewise linear loss-aversion contract estimates in Dittmann, Maug, and Spalt (2010). A 10% increase in loss-aversion is estimated to lead to an increase of 2% to 8% in the ratio of option to total equity (median estimates in Panel A of Table VI in Dittmann et al (2010)) depending on the starting loss-aversion parameter.

number of options and stocks (instead of their grant value) to verify that our results are not mechanically driven by changes in stock and option prices. Second, given the randomized nature of the experiment, selection bias and, by extension, endogeneity should not be an issue. Nevertheless, we examine whether our findings are the result of chance by randomizing inclusion of firms in the pilot group. Out of 5,000 simulations, we do not find a single instance in which all our main variables experience statistically significant changes.

Our study contributes to the literature that links financial markets to corporate decisions. Chen, Goldstein and Jiang (2007) and Edmans, Goldstein and Jiang (2012) show that financial markets influence investment policy and takeovers. More closely related to our work, Grullon et al. (2014) and Fang et al. (2014) find an effect of short-selling on investment and financing policies, and on earnings management. Our study complements this growing literature by being the first one to relate short-selling to the design of incentives and by highlighting the importance of financial markets in corporate governance mechanisms, as was first suggested in Holmstrom and Tirole (1993).

Our paper makes several contributions to the literature on executive incentives. First, to the extent that we can interpret our findings as driven by an exogenous increase in downside risk, this paper sheds light on how risk affects compensation policies. This is an important issue because identification of a causal relationship between incentives and risk has been controversial due to the fundamental endogenous relation between these two variables (Aggarwal and Samwick (1999), Core and Guay (2001), and Prendergast (2002)). Second, we provide evidence that firms move quickly to readjust executive incentives following a shock to the environment. It thus complements the findings in Hayes, Lemmon, and Qiu (2012), who show that firms readjust compensation packages after the adoption of FAS 123R.[16] Third, our study also sheds light on the rationales behind the nonlinearities observed in incentive contracts. In particular, by studying an asymmetric shock on risk, we provide empirical support to recent models that predict the use of options in compensation contracts (Dittmann, Maug, and Spalt (2010), Hemmer, Kim, and Verrecchia (2000)).[17]

The remainder of the paper is organized as follows. Section I describes our data and main variables. Section II discusses our identification strategy and examines how firms adjust their executive incentives in response to an exogenous shock to short-selling activity. Section III examines several channels through which short-selling could affect firms' compensation policy. Section IV presents robustness tests. Section V concludes.

I. Sample, Data, and Variable Definitions

We construct the main dataset from the Center for Research on Security Prices (CRSP). We build the Russell 3000 index based on the rankings of stock market capitalizations as of May 28, 2004 and May 31, 2005.[18] We follow Diether, Lee and Werner (2009) and keep firms that were in the Russell 3000 index in 2004 and 2005 and eliminate firms that are deleted from the index due to acquisitions, mergers or bankruptcies during the year. We merge this list with the list of pilot securities announced on July 28, 2004 by the SEC. Out of the 968 pilot securities in the initial list, 946 pilot securities remain in the sample after the first filter. We merge this data

[16] Recently, Gormley, Matsa and Milbourn (2013) document that firms decrease the convexity of the new equity grants after a shock to liability risk. In contrast, our paper finds that a shock to short-selling risk that is exogenous to growth opportunities leads to an increase in the convexity of executives' new equity grants. See also Cheng, Hong, and Scheinkman (2014) who study the association between risk and total compensation among financial firms during the financial crisis of 2008.

[17] We note that McAnally, Neel and Rees (2010) study the relation of pay for performance sensitivity (PPS) to downside risk and find a negative association. To the extent that PPS is driven by options-based compensation (Hall (1998)) our results can be interpreted as opposite to theirs.

[18] Consistent with the definition of the Russell 3000 at the reconstitution date, we exclude stocks with prices below $1, pink sheet and bulletin board stocks, closed-end mutual funds, limited partnerships, royalty trusts, foreign stocks and American Depositary Receipts (ADRs).

with Compustat, Execucomp, Risk Metrics, and exclude banks and financial firms. Our sample with compensation or anti-takeover information contains 1,442 firms (935 control and 507 pilot firms). For our main tests on the structure of new equity grants, our sample is an unbalanced panel of 4,004 firm-year observations (1,022 firms with 665 control and 357 pilot firms). We define all variables used in the paper in Appendix 1.

Table I provides summary statistics for all the firms in the sample, with a breakdown between pilot and control firms. We find no differences between the two groups, suggesting that our filtering process does not create any obvious sample selection bias to the random selection by the SEC. Both groups of firms have about the same size, compensation levels, equity grants structure, governance characteristics, corporate spending, payout ratios, and capital structure. None of the differences in characteristics are statistically significant. Therefore, the data support the hypothesis that our pilot group firms represent a random draw from our overall sample.

{Insert Table I here}

II. Regulation SHO and Changes in Managerial Incentive Contracts

On July 28, 2004, the SEC announced the removal of restrictions on short sales for a randomly selected sample from the Russell 3000 index. The SEC selected firms from the Russell 3000 index listed on NYSE, NASDAQ and AMEX and ranked them separately for each stock exchange by average daily traded volume. In each stock market, the SEC would then take 3 stocks and pick only the second one to be part of the pilot study. It would then repeat the process by moving down the rankings to ensure representation from the three stock markets, and to get consistent average trading volume between pilot and control firms in each stock market. The objective of the pilot study was to test the impact of removing short sales restrictions induced by the uptick rule on stock market volatility, liquidity, and price efficiency. Figure 1 provides a detailed timeline of the experiment.[19] Previous literature documents an economically significant increase in short-selling activity of about 11% around the announcement date (Grullon et al. (2014)) and 4 to 8% around the implementation date of Reg SHO (SEC's OEA (2007), Diether et al. (2009)).

{Insert Figure 1 here}

To examine the impact of short-selling on the design of executive incentives, we first look at the changes in the structure of new equity grants around the announcement of Reg SHO. We then investigate whether firms change their governance structure around this regulatory change.

A. The Structure of New Equity Grants awarded to the CEO

Our first set of tests examines whether the structure of the new equity grants awarded to the CEO changes around the removal of short-selling constraints. Following the existing literature, we use stock options awards to capture the convexity of the compensation payoff (see, e.g., Hayes et al (2012)). In this paper, we study the change of convexity in the compensation contract by examining the trade-off between awarding stock options and restricted stocks in new CEO equity grants. Everything else equals, granting more stock options relative to restricted stock in new equity grants will lead to higher convexity in the compensation payoff. Our main measure of interest is the portion of options in new equity awards (i.e. option awards scaled by the sum of option and stock awards).[20]

[19] The Securities Exchange Act Release No 48709A first announced on October 28, 2003 the SEC's intention to run the experiment and requested external comments. The Securities Exchange Act Release No 50104 on July 28, 2004 announced the final design of the experiment, the list of all firms in the pilot group, and the group of firms for which all price tests were suspended.

[20] This measure is similar to the one employed in Kadan and Swinkels (2008).

One alternative approach to study the change of convexity in CEO incentives would be to compute the vega of the CEO's total equity portfolio.[21] However, the computation of the portfolio vega relies on the return distribution of the underlying stock. Hence, even absent any changes in compensation practices, this variable could capture a mechanical change in the vega since Reg SHO impacts the return distribution of the underlying stock (see Section III.B). As a consequence, this would not be a reliable measure in our empirical setting.

A.1. The Structure of New Equity Grants in the 2001-2007 period

We first compare, in a simple univariate setting, the evolution of the structure of CEO equity grants for firms in the pilot group and in the control group over time in Figure 2. Panel A plots the difference in the average ratio of the value of stock options granted to the total value of equity grants between pilot and control firms during the period 2001 to 2007. Before the start of the experiment, the difference in the structure of new equity grants between the two groups is very small and statistically insignificant. The difference (in dollars) ranges between -2.3% and 0% before the experiment, increases to +4.5% during the experiment, and goes back to 0.7% when the uptick rule is repealed for all US firms.

{Insert Figure 2 here}

We also study the number of stock options and restricted stocks to verify that our results are not mechanically driven by a relative change in the stock price of pilot firms relative to control firms. This analysis is useful in confirming that we indeed capture a change in contracting behavior, not a mechanical effect on the distribution of stock prices. Panel A also plots the difference in the average ratio of the number of stock options granted to the total

[21] Vega captures the sensitivity of a change in dollar value of a financial claim as a function of a change in the annualized standard deviation of stock returns. Guay (1999) uses vega as a measure of the convexity of the compensation payoff and shows that the vega associated with stock options is considerably larger than the vega associated to restricted stock. As a result, subsequent studies such as Knopf et al (2002) and Coles et al (2006) approximate the total vega of CEOs' stock and option portfolios by the vega of their option portfolio.

number of stock options and shares of restricted stock granted to the CEO over the same period. Consistent with the previous analysis, we find that before the experiment, the difference of the new equity grant structure ranges between -1% and 0.4%. This difference significantly increases during the experiment to reach +3.3% in 2004 and +4.3% in 2006, while it decreases to +2.2% after the repeal of the uptick rule for all US firms in 2007.

In Panel B we plot the yearly difference-in-differences of the structure of new CEO equity grants between pilot firms and control firms over the same period. The difference-in-differences coefficient (DiD) measures the change in the difference of the ratio of stock options granted to total equity grants (in value and in number of shares) between pilot and control firms from year t-1 to year t. This panel shows that there are almost no changes in the difference of the structure of new equity grants between the two groups during all the years covered except in 2005 (the year following the announcement of Reg SHO) and in 2007 (the year of the repeal of the uptick rule for all US stocks). In 2005, the DiD is +5.7% (*Option/Equity($)*) and +4.3% (*Option/Equity(#)*). In 2007, the DiD is -3.8% (*Option/Equity($)*) and -2.2% (*Option/Equity(#)*).

These results suggest that the implementation of Reg SHO causes pilot firms to use more stock options in their new CEO equity grants, and this leads to an increase in the convexity of the CEOs' compensation payoffs.

A.2. Difference-in-Differences Analysis

Our empirical strategy relies on the exogenous shock created by the announcement on July 28, 2004 of the list of firms in the pilot experiment implemented in 2005. We thus employ a difference-in-differences technique in a regression setting to gauge the effect of the treatment (e.g. Reg SHO) on the affected group (e.g. pilot firms). The sample period is from June 2002 to May 2007. The treatment years are fiscal year 2005 and 2006 (so unaffected years are fiscal years 2003 and 2004). Firms in Compustat with a 2005 fiscal year have a fiscal year start date between June 1, 2004 and May, 31 2005. Therefore, considering that equity grants are in general decided at the beginning of the fiscal year (Lie (2005)), we assume that firms' decisions regarding the structure of new equity grants occur either immediately following the announcement date of Reg SHO (July, 28 2004), or up to 12 months after the announcement date.[22] We consider other timing classifications in the robustness tests section and reach similar conclusions. The dependent variable is the ratio of the value of stock options granted to the CEO to the total value of equity grants (*Option/Equity ($)*). Panel A of Table II shows results for OLS, fixed-effect and Tobit regressions (left censored at 0 and right censored at 1).

{Insert Table II here}

In those regressions, the coefficient of *Pilot* (dummy variable equal to one if the firm is in the Pilot Group of Reg SHO) is not significant. This confirms that there is no pre-treatment effect for pilot firms, and that pilot and control firms exhibit similar equity grant structures before exposure to the treatment. The coefficient of *Treatment Years* is negative and significant, suggesting a negative trend in the use of stock options in new CEO equity grants. Firms use fewer stock options across the board due to changes in the expensing and regulation of stock options in CEO compensation (Hayes, Lemmon and Qiu (2012)). Finally, our coefficient of interest, *Treatment Years*Pilot*, is positive and significant. This coefficient indicates that the pilot firms include more stock options in their new CEO equity grants during the experiment than the control firms. We reach similar conclusions using our alternative regression

[22] See, for instance, Core and Guay (1999). In their empirical framework, they assume that the design of executive incentives is decided at the beginning of the fiscal year.

specifications.[23] These results are consistent with our graphical analysis in Figure 2 and suggest that Reg SHO causes pilot firms to use more stock options in new CEO equity grants.

The economic magnitude of our results is large. The point estimates from the first column in Panel A suggest that the change in the proportion of stock options in new equity grants increases by 6 percentage points during the treatment years. This represents an increase of 8% relative to the ex-ante mean proportion of stock options in new equity grants (i.e. in 2003 and 2004 – during the control period before the Reg SHO experiment), or an 18% increase relative to the ex-ante standard deviation of the variable.

We also replicate our analysis using the ratio of the number of stock options granted to the total number of stock options and shares of restricted stock granted to the CEO (*Option/Equity (#)*) as a dependent variable. We find similar results, thus confirming that we capture a change in contracting behavior that is not driven by changes in stock prices.

In Panel B of Table II, we extend the sample period by including fiscal years 2001, 2002 and 2007. We create dummy variables for each fiscal year separately and interact these with our pilot dummy to precisely identify when changes in the equity grant structure occur. Consistent with the previous analyses, we find that the difference in the equity grant structure between pilot and control firms is only significant in 2005 and 2006. These results confirm that there is no pre-treatment effect (i.e. both groups are similar before the experiment), that pilot firms use more stock options during the treatment period, and that this difference disappears at the end of the experiment around the time of the repeal of the uptick rule for all US stocks.[24]

[23] As exposed in Puhani (2012), the interacted term *Treatment Years*Pilot* in the Tobit regression correctly identifies the sign of the treatment effect in a difference-in-differences model, even though Tobit is a non-linear model.

[24] We note here that although the difference disappears in 2007, the difference-in-differences test using the repeal of the experiment is not statistically significant at usual levels. This lack of significance may be attributable to the lack of clarity of the SEC on the initial end date of the experiment. In addition, given the fact that the pilot firms had

The economic magnitude of these results is similar to the one measured in Panel A. Using the point estimates from the first column in Panel B, the change in the proportion of stock options in new equity grants increases by 6 percentage points in 2005. This represents an increase of 8% relative to the ex-ante mean proportion of stock options in new equity grants (i.e. in 2004 – the benchmark year in this regression), or a 16% increase relative to the ex-ante standard deviation of the variable.

We also investigate the change in the structure of new equity grants awarded to all top executives present in the Execucomp database. In addition to using OLS, firm fixed-effect and Tobit specifications, we also use an executive fixed–effect specification. Table III presents the results.

{Insert Table III here}

The results are similar to the ones for the CEO. In all regression specifications, we find a significant increase in the proportion of stock options in new equity grants for the Pilot firms relative to the Control firms (Panel A). In addition, when extending the sample period and including dummy variables for each fiscal year, we find that the difference in the structure of new equity grants is only significant in 2005 and 2006, i.e. during the experiment (Panel B). The coefficient of the interaction of the Pilot dummy and the 2007 fiscal year dummy term is not significant, consistent with the results for the CEO equity awards. This last result confirms that the difference in the structure of new equity grants disappears at the end of the experiment.

A.3. Additional Results regarding the Design of Executive Incentives

We also study changes in other pecuniary and non-pecuniary forms of incentives in response to the implementation of Reg SHO. More precisely, we investigate changes in the

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been treated for a period of two years, it is not clear whether treated and control firms represent a random sample of the population anymore and thus whether difference-in-differences tests can be used for causal inferences.

provision of severance package and in anti-takeover provisions. We examine three specific anti-takeover provisions: if the board of the company is classified (cboard), if the firm has a blank check preferred provision (blankcheck), and if the firm requires supermajority to approve a merger (supermajor). We employ logit regressions and report the results in Table IV.

{Insert Table IV here}

The coefficient for Treatment Years*Pilot is positive for all provisions, although only significant at the usual significance level for classified board and blank check. Lower power is expected given that we only have one observation per firm every other year. We extend this analysis by creating an index measuring the extent of the use of severance package and the three anti-takeover provisions. Anti+Sev represents the sum of the four provisions (i.e. it is a discrete variable ranging from zero to four) and I_Anti+Sev is a dummy variable indicating whether Anti+Sev is positive. Using logit and ordered logit regressions, we find that the coefficient for Treatment Years*Pilot is positive and significant for both specifications. These results suggest that firms insure their top executives against the adverse effects associated with short-selling activity. These results also complement the results related to the changes in the structure of new equity grants and confirm that firms react to a change in the firm's contracting environment by redesigning managerial incentives.

III. Changes in the Contractual Environment

In this section we examine the impact of Reg SHO on the contractual environment to determine the channel(s) through which short-selling activity affects compensation contracts. We focus on two dimensions: downside risk and stock price informativeness. We assess the effect of Reg SHO on downside risk by examining changes in the sensitivity of stock prices to realized and anticipated negative news. To study the effect of Reg SHO on stock price informativeness, we use the probability of informed trading (PIN) (Easley, O'Hara, and Hvidkjaer (2002)) and 1-R^2 (Roll (1988)).

Regarding the sensitivity to realized negative news, we follow the methodology in Grullon et al. (2014), who focus on event windows around the announcement date. They use this approach because under rational expectations, investors should incorporate the future impact of the change in short sales regulation at the time of the announcement (see, for example, Allen, Morris, and Postlewaite (1993) and Scheikman and Xiong (2003)).[25,26] Moreover, the Reg SHO experiment could increase stock price sensitivity to negative news subsequent to the announcement date because of the increased incentives of bear raiders to manipulate the value of those firms that will face weaker short-selling constraints in the future (Goldstein and Guembel (2008)).

Taken together, these theories suggest that short-sellers and existing shareholders of the firms in the pilot group should sell their stocks more aggressively when these firms are subject to negative news, even before the implementation of the pilot test. Increased short-selling is rational as long as the benefits from doing so do not overweigh the costs of short-selling the stocks that are still subject to the uptick rule. Consistent with this argument, Grullon et al. (2014), find that short interest increases around the announcement of the pilot program on July 28, 2004. Moreover, the SEC's Office of Economic Analysis (OEA, 2007), Alexander and

[25] Allen, Morris, and Postlewaite (1993) show that stock price bubbles may arise if investors face short sale constraints either now or in the future, in spite of all agents being rational and fully informed about future dividends. In their model, the belief that investors will be able to sell the stock at a high price in the future causes the bubble. In this setting, the announcement of the removal of short-selling constraints in the future should immediately lead to an increase in selling activity by existing shareholders and possibly short sellers because investors realize that they will not be able to sell the stocks at inflated prices to other investors in the future.

[26] Scheinkman and Xiong (2003) show that stock prices should incorporate the option value of reselling to optimistic investors in the presence of short-selling constraints. The expected removal of short-selling constraints should therefore lead to an increase in selling and short-selling activity after the announcement.

Peterson (2008), Diether et al. (2009) document an increase in short sales after the implementation of the pilot experiment on May 2, 2005.

A. *Sensitivity to Negative News*

We test whether stock prices for the firms in the pilot group become more sensitive to bad news. If the removal of short-selling constraints increases the trading activity of pessimistic investors in the stock market, whether they already own the stock (existing shareholders) or not (short-sellers), then stock prices of the pilot firms should become more sensitive to realized or anticipated bad news after the announcement of the Reg SHO experiment. To test this hypothesis, we examine the behavior of daily returns of both pilot and control firms during bearish and bullish stock market days. We also examine the impact of Reg SHO on the volatility skew of options to determine whether the options markets anticipate the effects of the removal of short-sales constraints. The objective of these tests is to provide evidence that Reg SHO generates an asymmetric shock to stock price risk. By becoming more sensitive to negative news, we argue that stocks become more risky on the downside, a feature that will expose stock and put option investors to more risk, but not call options investors.

We first test firms' stock price reactions to bad market-wide news. We resort to difference-in-differences analyses in which we sort daily market-wide returns into five quintiles to test whether the returns of firms in the pilot group become more negative in the worst market days (first quintile of market returns) after the announcement of the pilot program.

{Insert Table V here}

Panel A of Table V presents the results of this analysis. The two groups of firms do not display different returns on bad market days before the announcement of Reg SHO. However, after the announcement, firms in the pilot group display more negative returns than the control firms during the worst market days (lowest quintile). The difference-in-differences coefficient is statistically significant at the 1% level. In auxiliary tests not reported in a table, we also study changes in the sensitivity of pilot stock returns to firm-specific news (earnings announcements) and find similar results.

B. Implied Volatility Skew

To further investigate whether stock prices become more sensitive to bad news after Reg SHO, we also examine changes in the volatility skew of put and call options on the stocks of pilot and control firms. The volatility skew of puts (calls) has been shown to proxy for large expected negative (positive) jumps in individual stocks (Xing, Zhang and Zhao (2010)) and in indices (Bollen and Whaley (2004), Bates (2003), and Gârleanu, Pedersen, and Poteshman (2007)). We define volatility skew of put options as the difference between the implied volatility of out of the money put options (strike price to stock price ratio between 0.7 and 0.9) and at the money put options (strike price to stock price ratio between 0.95 and 1.05).[27] The volatility skew of call options is the difference between the implied volatility of out of the money call options (strike price to stock price ratio between 1.1 and 1.3) and at the money call options (strike price to stock price ratio between 0.95 and 1.05).

Our estimation window covers the two-month period before and after July 28, 2004 (i.e. the Reg SHO announcement).[28] The volatility-skew of puts captures the anticipation of large negative jumps in prices. As illustrated in Figure 3, we observe that the volatility skew of put options is similar across both groups of firms before the experiment while it increases by around

[27] We define volatility skew as the difference between the implied volatility of out-of-the-money put (call) options and that of at-the-money put (call) options following Xing, Zhang and Zhao (2010), except that we separate out the negative and positive components of volatility skew.
[28] Due to data limitations, we use a restricted subsample of firms that have options traded on options market with a strictly positive trading volume. Only 490 such firms (pilot and control) meet our requirements, thus resulting in a sample that is about one third of the size of our original sample.

10% after the announcement for firms in the pilot group relative to the ones in the control group. In addition, the statistical tests in Panel B of Table V show that the increase in the volatility skew of the puts is significant. We also perform the same exercise using call options (see Figure 3 and Panel C of Table V) and find no significant changes in the difference of volatility skew between the two groups. These results confirm that the change in the risk profile of the firm is asymmetric: only the downside component of equity risk is affected by the relaxation of short-selling constraints. In addition, we also find that the realized negative semi-volatility of stock returns increases while the positive semi-volatility does not increase in a period of one year around the announcement date of Reg SHO. These results are untabulated in the interest of space.

{Insert Figure 3 here}

C. Stock Price Informativeness

The removal of short-sales constraints may have improved the incorporation of negative information into stock prices for pilot firms. We use two different measures to estimate the changes in stock price informativeness around Reg SHO. The first one is $1\text{-}R^2$ (Roll (1988)) and the second one is the probability of informed trades (PIN) (Easley, O'Hara, and Hvidkjaer (2002)).[29] Because these measures are computed on an annual-based, we compare their estimates in 2003 and 2005 and thus drop 2004 (because about half of the 2004 period is treated). The results are reported in Panel D and E of Table V respectively. Overall, our results do not support the hypothesis that unrestrained short-selling due to the removal of the uptick rule improves stock price informativeness. The difference-in-differences estimators indicate a non-significant decrease in $1\text{-}R^2$ (Panel A) and a decrease in PIN (Panel B).

[29] We collect the PIN measure from Stephen Brown's website (see Brown and Hillegeist (2007)).

All our results in this section point to a significant increase in downside risk for the firms in the pilot group. In addition, we do not find evidence supporting the alternative hypothesis that stock price informativeness increases.

D. Difference-in-Difference-in-Differences Analysis

We use a difference-in-difference-in-differences (DiDiD) technique to explore how cross-sectional variation in risk, informativeness and governance relate to our documented effect on the structure of new equity awards. We first apply the DiDiD technique to study how changes in downside risk can be related to our results. For that purpose, we create a dummy variable equal to one if the firm is in the top quintile of changes in stock price returns sensitivity to negative market returns around the announcement date (*High Downside Risk*). We measure changes in stock price returns sensitivity to negative market returns as changes in firms' stock returns when the daily stock market returns fall into the lowest quintile of stock market return days (as shown in Panel A of Table V).[30] The change is measured over a one-year period before and after the Reg SHO announcement date. The results are reported in Panel A of Table VI.

{Insert Table VI here}

The coefficient for *High Downside Risk*Treatment Years*Pilot* is positive and significant in all specifications. Changes in the structure of new equity grants are more pronounced for the pilot firms with the largest increases in the sensitivity of their stock prices to negative market-wide news. The economic magnitude is quite large. The point estimates from the first column in Table VI indicate that the change in the proportion of stock options in new equity grants increases by 9.5 percentage points more for the pilot firms that are most affected

[30] If we use the volatility skew of put options as an alternative downside risk measure (see Panel B of Table III), we obtain qualitatively similar results. The level of significance of the differences is lower due to a very small sample size.

(in comparison to pilot firms that are less affected). According to the point estimates, the total effect for these firms is on average 13 percentage points (i.e. 9.5 plus 3.6), which represents an increase of 17% relative to the ex-ante mean proportion of stock options in new equity grants. These results suggest that changes in downside equity risk are driving the effects on the changes in the structure of new CEO option grants.

In Panel B and C, we apply the DiDiD technique to study how potential increases in stock price informativeness can be related to our results. We use measure the changes in $1-R^2$ and PIN between 2003 and 2005, and create a dummy variable equal to one if the firm is in top quintile in the increase of stock price informativeness (*High* $1-R^2$ and *High PIN*). Overall we find that our results are not significantly related to changes in stock price informativeness.

Finally, in Panel D, we also explore whether our results are related to weaker ex-ante governance measure using the G-Index of the firm in 2004 (Gompers, Ishii, Metrick (2003)). We create a dummy variable equal to one if the G-Index is in the top quintile in 2004 (*High Gindex*). We find that our results are not driven by firms with ex-ante weaker governance measures.

In general, the results in this section are consistent with a risk-based explanation. Stock options, with their floor at zero, are less sensitive to changes in downside risk than restricted stocks. Hence, by shifting toward stock options and away from restricted stocks, firms appear to decrease managerial exposure to the increase in downside risks associated with short-selling. On the other hand, our results do not support an informativeness-based argument. If firms were changing CEO incentives contracts to take advantage of the negative information impounded into stock prices, they should use more restricted stock and less stock options, which decrease managerial exposure to negative outcomes. Moreover, a risk-based story is also consistent with our findings that firms insure their top executives against the adverse effects associated with increased probability of hostile takeovers and dismissal by providing severance packages and anti-takeover provisions (classified boards, blank check preferred provisions, supermajority rules to approve a merger).

E. New Incentive Contracts and Investment Outcomes

Given that our evidence indicates that Reg SHO leads firms to reduce managerial exposure to downside risk by granting relatively more stock options, we investigate whether these changes affect firms' investment policies. In particular, we explore whether pilot firms that change the structure of their equity grants the most also tend to invest more. The motivation for this test comes from Grullon et al. (2014) who find that pilot firms exhibit a decrease in their investment following Reg SHO.

To proxy for firms that exhibit a large change in grant structure, we create a dummy variable equal to 1 if the increase in *Option/Equity ($)* from the 2003-2004 to the 2005-2006 period falls in the top decile of the sample distribution (*High Equity Change*). For this part of the analysis, the sample firms are restricted to non-utilities firms in the Pilot group. We use two different measures of investment: one based on capital expenditure (*CAPX*) and another one including research and development expenses (*CAPX+R&D*). The results are reported in Table VII.

{Insert Table VII here}

The coefficient for *Treatment Years* High Equity Change* is positive and significant for both specifications. In other words, pilot firms that responded the most to changes in downside risk by increasing stock option grants also increase investment in capital expenditures and research and development expenses the most. These results provide suggestive evidence of the interplay between the design of CEO incentives and investment outcomes.

IV. Robustness Tests

We first run placebo regressions to check the validity of our results. The results are reported in Table VIII. The sample period is fiscal year 2001 to 2004. The placebo treatment years are 2003 and 2004. Confirming that our results are not spurious, we find that the coefficient of *Placebo Treatment Years*Pilot* is not significant.

{Insert Table VIII here}

We also examine whether our results are robust to a different classification of the treatment period. In our empirical framework, we assume that the decision regarding the structure of the equity awards is made at the beginning of the fiscal year (see, e.g., Core and Guay, 1999). Yet, since the Reg SHO experiment was announced on July 28, 2004, it is possible that some firms already re-contracted in fiscal year 2004 if the design of CEO incentives contracts occurs at the end of the fiscal year. This potential measurement error would reduce our ability to find a significant effect of the regulation or reduce the economic magnitude of the impact of Reg SHO on the change in the equity grant structure.

To address this concern, we re-run our main regressions in Table IX using only firms with fiscal-year month ending after the month of July (Panel A) or excluding fiscal year 2004 (Panel B). We find similar results to the ones presented in our main analysis in both specifications. In addition, the point estimates in Panel A are larger than in our main regressions, confirming that the potential measurement error works against us finding a significant effect. It is therefore unlikely that a timing mismatch affects our conclusions.

{Insert Table IX here}

An alternative channel that may potentially explain our results is related to a drop in stock prices. Since stock prices of firms in the pilot might be negatively affected by the experiment (Grullon et al (2014)), it is possible that the pilot firms could simply be reloading managers' incentives. First, one should note that if the changes in the equity grant structure were related to reloading motives, the differences in equity grant structure between the two groups would most likely not persist over a two-year period. Treated firms would reload the incentives in the first year, and the difference between the two groups would disappear after one year. Nevertheless, we test this alternative explanation by examining whether the firms that exhibit a large negative announcement returns around the announcement date (i.e. firms more impacted by a change in stock price – variable *Low CAR*) also exhibit a larger change in the structure of new equity grants. The results are reported in Panel C of Table IX. The coefficient for *LowCAR* TreatmentYears*Pilot* has the wrong sign and is not statistically significant, suggesting that a drop in stock prices is not the driving force behind our results.

Our final robustness test is related to the randomized nature of our experimental framework. As mentioned earlier, endogeneity is unlikely to be an issue since firms cannot possibly have *caused* their inclusion in the pilot program. Nevertheless, we test whether our results could have been the result of chance. We randomize inclusion of firms in the pilot group and bootstrap an empirical distribution of our main results. Table X shows the empirical distribution we get out of 5,000 simulations. We cannot find a single sample exhibiting a joint increase in the sensitivity to negative news, and in the proportion of options in new equity grants that are independently statistically significant at the 10% level. Thus, it is unlikely that the results we document are generated by methodology choices or sample selection.

{Insert Table X here}

In addition, this robustness test validates the level of significance of our main tests. In Table X, we provide the bootstrapped distribution of T-statistics from the randomized samples for our main tests. According to this bootstrapped distribution, the change in the structure of new CEO equity grants is significant at the 1% level. In addition, the change in the antitakeover provisions classified board and blank check is significant at the 5% level.

V. Conclusion

We investigate whether short-selling threats affect the design of executive incentives. We use a randomized natural experiment that relaxes short-selling constraints on a random sample of US stocks (Reg SHO). Using difference-in-differences tests around the pilot program, we find evidence that Reg SHO affects firms' contracting environment by increasing downside risk and that treated firms reduce managerial exposure to this risk by increasing the proportion of stock options granted in new equity grants. We also find that the change in equity grant structure is significantly more pronounced for firms with larger changes in the sensitivity of their stock prices to negative news. Our evidence also indicates that firms redesign the contracts of the other top executives as well as adopt anti-takeover provisions during the experiment. Finally, we find suggestive evidence that these changes in incentive contracts influence corporate investment.

Overall, our results contribute to the literature on executive incentives by pointing to a causal effect of risk on the design of executive incentive contracts and by providing evidence consistent with models that rationalize the extensive use of stock options to incentivize managers that are more sensitive to losses than gains (Dittman, Maug, and Spalt (2010)).

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Appendix 1
Definition of Main Variables

1-R2	One minus the yearly R^2 from regressing firm stock returns on market and industry index returns (Roll (1988))
blankcheck	Dummy variable equal to 1 if the firm has a blank check preferred provision (blankcheck)
Cash flow	Net income before extraordinary Items (IB) + depreciation and amortization expenses (*DP*) scaled by start-of-year total assets x 100
Cash Holdings	Cash and Short Term Investment (*CHE*) scaled by start-of-year total assets (*AT*) x 100
CAPX	Capital expenditures (Compustat *CAPX*) scaled by start-of-year total assets (*AT*) x 100
CAPX+R&D	Capital expenditures (*CAPX*) plus Research and Development Expenses (*XRD*) scaled by start-of-year total assets (*AT*) x 100
cboard	Dummy variable equal to 1 if the board of the company is classified (RiskMetrics: cboard)
CEO Tenure	The difference between fiscal year and the year in which the CEO became the CEO
Control	Dummy variable equal to 1 if the company is not in the Pilot Group of REG SHO
Dividends	Common Shares Dividends (*DVC*) plus Preferred Shares Dividends (*DVP*) scaled by start-of-year total assets (*AT*) x100
Equity Grant	Total value of equity grants (Options plus Restricted Stocks)
G Index	Governance index developed by Gompers, Ishii, and Metrick (2003)
High Downside Risk	Dummy variable equal to 1 if the firm is in the top quintile of changes in stock price returns sensitivity to negative market returns around the announcement date. We measure changes in stock price returns sensitivity to negative market returns as changes in firms' stock returns when the daily stock market returns fall into the lowest quintile of stock market return days (as shown in Table V). The change is measured over a one-year period before and after the Reg SHO announcement date.
High Equity Change	Dummy variable equal to 1 if the increase in *Option/Equity ($)* from the 2003-2004 to the 2005-2006 period is in the top decile of the sample distribution
Leverage	Long term debt (*DLTT*) plus debt in current liabilities (*DLC*) scaled by the sum of long term debt, debt in current liabilities, and total stockholders' equity (*SEQ*) x 100
Low CAR	Dummy variable equal to 1 if firm's CAR around the SHO announcement is below the median

Market-to-Book ratio	Market value of equity (*PRCC* x *CSHO*) plus book value of assets minus book value of equity minus deferred taxes (when available) (*AT-CEQ-TXDB*), scaled by book value of total assets (*AT*). Variable is lagged one year
Options ($)	The value of stock options granted to the CEO (Execucomp – before 2006: option_awards_blk_value – starting 2006: option_awards_fv)
Options (#)	The number of stock options granted to the CEO (option_awards_num)
Options/Equity ($)	Ratio of the value of stock options granted to the total value of equity grants in % (100 x *Options ($)/(Options ($)+Restricted Stock ($))*
Options/Equity (#)	Ratio of the number of stock options granted to the total number of stock options and shares of restricted stock granted in% (100 x *Options (#)/(Options (#)+Restricted Stock (#))*
Past profitability	Ratio of operating income before depreciation and amortization (*OIBDP*) to start-of-year total assets (AT) x 100. Variable is lagged one year
Pilot	Dummy variable equal to 1 if the company is in the Pilot Group of REG SHO
PIN	Yearly probability of informed trading as computed by Brown and Hillegeist (2007)
Placebo Treatment Years	Dummy variable equal to 1 if fiscal year is 2003 or 2004
Restricted Stock ($)	The value of restricted stock granted to the CEO (before 2006: rstkgrnt – starting 2006: stock_awards_fv)
Restricted Stock (#)	The number of shares of restricted stock granted to the CEO (*Restricted Stock ($)*/prcc_f)
severance	Dummy variable equal to 1 if the firm uses severance packages (severance)
supermajor	Dummy variable equal to 1 if the firm requires supermajority to approve a merger (supermajor)
Total assets	Start-of-year total assets (*AT*) (in million USD)
Treatment Years	Dummy variable equal to 1 if fiscal year is 2005 or 2006

Table I
Summary Statistics

Data are collected from the merged CRSP/Compustat Industrial database, Execucomp, and RiskMetrics in the fiscal year that is the closest to July 28, 2004, the announcement date of the SHO pilot test. We exclude firms that are not in the Russell 3000 index in 2004 and 2005, and financial services firms (SIC code 6000-6999). All variables are described in Appendix 1.

	Pilot group				Control group					
	N	Mean	Median	Std. Dev	N	Mean	Median	Std. Dev	Diff.	T-stat
Total assets	471	4,635	1,132	13,064	878	5,263	1,199	14,738	-629	-0.78
Market-to-Book ratio	471	2.11	1.80	1.06	878	2.13	1.75	1.08	-0.02	-0.25
CAPX	471	5.85	3.99	6.18	877	5.33	3.50	5.61	0.52	1.56
CAPX+R&D	466	9.82	7.06	8.58	872	9.60	7.23	10.37	0.23	0.48
Cash flow	470	10.79	10.48	10.01	877	10.80	10.72	12.17	0.56	0.72
Leverage	469	29.08	28.22	24.61	873	29.29	26.96	27.12	-0.21	-0.14
Dividends	471	1.01	0.00	1.70	876	0.97	0.00	1.73	0.04	0.42
Cash Holdings	471	20.92	13.35	23.15	877	22.81	13.48	23.92	-1.90	-1.41
Past profitability	470	12.66	13.07	9.71	874	12.39	12.44	10.86	0.27	0.46
Equity Grant ($)	442	2,400	1,338	2,966	807	2,590	1,386	3,156	-190	-1.04
Options/Equity ($)	353	73.23	100	35.20	660	75.50	100	34.58	-2.27	-0.99
Options/Equity (#)	357	80.47	100	31.65	667	81.44	100	31.27	-0.96	-0.47
cboard	473	0.57	1.00	0.50	860	0.60	1.00	0.49	-0.03	-0.92
blankcheck	473	0.90	1.00	0.30	860	0.91	1.00	0.29	-0.01	-0.45
supermajor	473	0.16	0.00	0.37	860	0.15	0.00	0.36	0.01	0.66
severance	473	0.06	0.00	0.24	860	0.07	0.00	0.25	0.01	0.28
G Index	473	9.12	9.00	2.66	860	9.17	9.00	2.45	-0.05	-0.37
CEO Tenure	438	6.92	5.00	6.30	804	6.46	4.00	5.76	0.47	1.32

Table II
The Impact of the Threat of Short-Selling on the Structure of Equity Grants awarded to the CEO

This table shows results of OLS, fixed-effect (FE) and Tobit regressions. Tobit regressions are left censored at 0 and right censored at 1. The sample period is fiscal year 2003 to 2006 for Panel A, and fiscal year 2001 to 2007 for Panel B. The dependent variables are the ratio of the value of stock options granted to the CEO to the total value of equity grants (*Option/Equity ($)*), and the ratio of the number of stock options granted to the total number of stock options and shares of restricted stock granted to the CEO (*Option/Equity (#)*). *Pilot* is a dummy variable equal to 1 if the company is in the Pilot Group of REG SHO. *Treatment Years* is a dummy variable equal to 1 if fiscal year is 2005 or 2006. Standard errors are clustered at the firm level. T-statistics are reported in parenthesis. [c], [b], [a] indicate a significance level of less than 10%, 5%, and 1% respectively.

Panel A: DiD Analysis (2003-2006)

VARIABLES	OLS Option/ Equity ($)	OLS Option/ Equity (#)	FE Option/ Equity ($)	FE Option/ Equity (#)	Tobit Option/ Equity ($)	Tobit Option/ Equity (#)
Pilot	-1.96	-0.86			-5.46	-3.46
	(-1.01)	(-0.52)			(-0.93)	(-0.66)
Treatment Years	-20.17[a]	-17.05[a]	-18.74[a]	-15.57[a]	-53.84[a]	-47.38[a]
	(-15.71)	(-13.51)	(-14.58)	(-12.34)	(-14.00)	(-13.12)
Treatment Years*Pilot	**5.95[a]**	**4.68[b]**	**4.78[b]**	**3.50[c]**	**14.34[b]**	**11.60[b]**
	(2.91)	**(2.38)**	**(2.40)**	**(1.84)**	**(2.55)**	**(2.26)**
Constant	78.32[a]	84.14[a]	77.14[a]	83.30[a]	122.79[a]	126.20[a]
	(69.60)	(85.79)	(154.34)	(172.01)	(31.03)	(36.82)
Observations	4,004	4,036	4,004	4,036	4,004	4,036
Adjusted R^2 / Pseudo R^2	0.058	0.049	0.477	0.478	0.012	0.011

Panel B: DiD Analysis By Year and Extended Sample Period (2001-2007)						
VARIABLES	OLS Option/ Equity ($)	OLS Option/ Equity (#)	FE Option/ Equity ($)	FE Option/ Equity (#)	Tobit Option/ Equity ($)	Tobit Option/ Equity (#)
Pilot	-2.27	-0.96			-6.16	-3.75
	(-0.98)	(-0.47)			(-0.97)	(-0.65)
Year 2001	13.26[a]	10.67[a]	14.75[a]	11.73[a]	43.80[a]	39.30[a]
	(9.21)	(8.28)	(10.09)	(8.82)	(8.91)	(8.74)
Year 2002	10.49[a]	8.01[a]	11.71[a]	8.68[a]	34.46[a]	30.56[a]
	(7.54)	(6.44)	(8.30)	(6.76)	(7.58)	(7.34)
Year 2003	5.68[a]	5.46[a]	5.40[a]	4.98[a]	18.29[a]	17.62[a]
	(4.47)	(4.66)	(4.52)	(4.47)	(4.85)	(5.09)
Year 2005	-9.61[a]	-8.05[a]	-9.87[a]	-8.13[a]	-25.48[a]	-22.56[a]
	(-6.69)	(-5.72)	(-7.19)	(-6.00)	(-6.72)	(-6.27)
Year 2006	-25.24[a]	-20.77[a]	-23.57[a]	-19.16[a]	-61.98[a]	-53.75[a]
	(-13.65)	(-11.72)	(-12.90)	(-11.08)	(-12.53)	(-11.64)
Year 2007	-29.22[a]	-24.89[a]	-27.71[a]	-23.56[a]	-69.83[a]	-60.76[a]
	(-15.57)	(-13.60)	(-14.97)	(-13.17)	(-13.81)	(-12.80)
Year 2001 * Pilot	1.87	1.35	1.52	1.43	4.87	3.96
	(0.78)	(0.62)	(0.65)	(0.66)	(0.61)	(0.54)
Year 2002 * Pilot	2.33	1.12	3.78	2.91	5.41	2.41
	(0.97)	(0.50)	(1.60)	(1.32)	(0.69)	(0.34)
Year 2003 * Pilot	0.90	0.44	2.11	1.95	2.69	1.51
	(0.39)	(0.21)	(1.00)	(0.98)	(0.40)	(0.24)
Year 2005 * Pilot	**5.69**[b]	**4.25**[b]	**5.65**[a]	**4.10**[b]	**13.27**[b]	**10.45**[c]
	(2.56)	**(1.97)**	**(2.78)**	**(2.07)**	**(2.30)**	**(1.93)**
Year 2006 * Pilot	**6.79**[b]	**5.30**[c]	**6.17**[b]	**5.07**[c]	**16.44**[b]	**13.11**[c]
	(2.23)	**(1.85)**	**(2.10)**	**(1.88)**	**(2.16)**	**(1.88)**
Year 2007 * Pilot	2.97	3.15	3.36	3.49	7.15	6.87
	(0.95)	(1.03)	(1.10)	(1.19)	(0.92)	(0.95)
Constant	75.50[a]	81.44[a]	73.89[a]	80.37[a]	112.38[a]	116.91[a]
	(56.06)	(67.22)	(99.24)	(114.91)	(27.84)	(32.56)
Observations	6,809	6,883	6,809	6,883	6,809	6,883
Adjusted R^2 / Pseudo R^2	0.163	0.129	0.488	0.465	0.033	0.031

Table III
The Impact of the Threat of Short-Selling on the Structure of Equity Grants awarded to all Firm Executives

This table shows results of OLS, firm fixed-effect (Firm FE), executive fixed-effect (Exec FE) and Tobit regressions. Tobit regressions are left censored at 0 and right censored at 1. The sample period is fiscal year 2003 to 2006 for Panel A, and fiscal year 2001 to 2007 for Panel B. The dependent variables are the ratio of the value of stock options granted to the CEO to the total value of equity grants (*Option/Equity ($)*), and the ratio of the number of stock options granted to the total number of stock options and shares of restricted stock granted to the CEO (*Option/Equity (#)*). *Pilot* is a dummy variable equal to 1 if the company is in the Pilot Group of REG SHO. *Treatment Years* is a dummy variable equal to 1 if fiscal year is 2005 or 2006. Standard errors are clustered at the firm level. T-statistics are reported in parenthesis. [c], [b], [a] indicate a significance level of less than 10%, 5%, and 1% respectively.

Panel A: DiD Analysis (2003-2006)

VARIABLES	OLS Option/ Equity ($)	OLS Option/ Equity (#)	Firm FE Option/ Equity ($)	Firm FE Option/ Equity (#)	Exec FE Option/ Equity ($)	Exec FE Option/ Equity (#)	Tobit Option/ Equity ($)	Tobit Option/ Equity (#)
Pilot	-0.43	0.15					-1.41	-0.28
	(-0.25)	(0.11)					(-0.25)	(-0.06)
Treatment Years	-20.48[a]	-18.40[a]	-19.99[a]	-17.60[a]	-18.16[a]	-16.56[a]	-55.78[a]	-50.97[a]
	(-18.83)	(-17.74)	(-18.30)	(-16.76)	(-16.77)	(-15.94)	(-16.30)	(-16.10)
Treatment Years*Pilot	**5.57[a]**	**4.99[a]**	**4.98[a]**	**4.55[a]**	**5.08[a]**	**4.45[a]**	**12.79[b]**	**10.89[b]**
	(3.14)	**(3.01)**	**(2.86)**	**(2.79)**	**(2.98)**	**(2.82)**	**(2.51)**	**(2.44)**
Constant	77.80[a]	84.90[a]	77.51[a]	84.66[a]	76.59[a]	84.20[a]	123.82[a]	129.07[a]
	(74.53)	(102.28)	(184.05)	(227.80)	(184.38)	(231.34)	(33.50)	(41.57)
Observations	22,322	24,549	22,322	24,549	22,322	24,549	22,322	24,549
Adjusted R^2 / Pseudo R^2	0.062	0.060	0.559	0.547	0.492	0.479	0.012	0.014

Panel B: DiD Analysis By Year and Extended Sample Period (2001-2007)								
VARIABLES	OLS Option/ Equity ($)	OLS Option/ Equity (#)	Firm FE Option/ Equity ($)	Firm FE Option/ Equity (#)	Exec FE Option/ Equity ($)	Exec FE Option/ Equity (#)	Tobit Option/ Equity ($)	Tobit Option/ Equity (#)
Pilot	-0.88	0.08					-2.61	-1.25
	(-0.41)	(0.04)					(-0.43)	(-0.24)
Year 2001	14.32[a]	11.15[a]	15.54[a]	11.92[a]	14.31[a]	11.80[a]	49.10[a]	43.92[a]
	(10.88)	(10.18)	(12.12)	(10.77)	(11.28)	(10.72)	(10.58)	(11.07)
Year 2002	11.79[a]	9.01[a]	12.67[a]	9.34[a]	12.22[a]	9.40[a]	41.23[a]	37.76[a]
	(9.38)	(8.62)	(10.30)	(8.82)	(10.17)	(8.97)	(9.56)	(10.15)
Year 2003	7.01[a]	6.58[a]	7.09[a]	6.79[a]	6.60[a]	6.69[a]	24.13[a]	22.82[a]
	(6.16)	(6.77)	(6.74)	(7.31)	(6.35)	(7.24)	(6.86)	(7.64)
Year 2005	-9.00[a]	-8.57[a]	-9.88[a]	-8.98[a]	-9.54[a]	-9.07[a]	-24.15[a]	-23.25[a]
	(-7.72)	(-7.85)	(-8.89)	(-8.50)	(-8.54)	(-8.57)	(-7.55)	(-8.05)
Year 2006	-24.88[a]	-21.55[a]	-23.90[a]	-20.13[a]	-23.40[a]	-19.94[a]	-61.90[a]	-55.31[a]
	(-15.72)	(-14.74)	(-15.13)	(-13.87)	(-14.67)	(-13.63)	(-14.08)	(-13.91)
Year 2007	-27.99[a]	-25.18[a]	-27.50[a]	-24.24[a]	-26.75[a]	-24.06[a]	-68.60[a]	-61.10[a]
	(-16.94)	(-16.04)	(-16.97)	(-15.79)	(-16.03)	(-15.35)	(-14.97)	(-14.67)
Year 2001 * Pilot	0.82	0.10	1.00	0.63	1.46	0.63	3.90	2.43
	(0.36)	(0.05)	(0.47)	(0.34)	(0.69)	(0.34)	(0.50)	(0.37)
Year 2002 * Pilot	0.38	-0.47	0.88	0.76	0.61	0.70	-1.29	-3.69
	(0.17)	(-0.25)	(0.42)	(0.41)	(0.29)	(0.37)	(-0.17)	(-0.60)
Year 2003 * Pilot	0.93	0.22	1.02	0.24	1.37	0.37	2.94	2.60
	(0.46)	(0.13)	(0.55)	(0.15)	(0.75)	(0.22)	(0.48)	(0.50)
Year 2005 * Pilot	**5.22[a]**	**4.47[a]**	**5.74[a]**	**4.78[a]**	**5.76[a]**	**4.88[a]**	**11.57[b]**	**9.99[b]**
	(2.81)	**(2.65)**	**(3.34)**	**(3.02)**	**(3.33)**	**(3.10)**	**(2.28)**	**(2.25)**
Year 2006 * Pilot	**6.73[b]**	**5.62[b]**	**6.25[b]**	**5.14[b]**	**6.56[b]**	**5.32[b]**	**15.84[b]**	**13.45[b]**
	(2.54)	**(2.33)**	**(2.40)**	**(2.19)**	**(2.51)**	**(2.28)**	**(2.28)**	**(2.22)**
Year 2007 * Pilot	2.44	3.24	2.91	3.15	2.20	2.39	5.12	5.85
	(0.90)	(1.27)	(1.10)	(1.27)	(0.80)	(0.93)	(0.73)	(0.95)
Constant	74.30[a]	81.52[a]	73.56[a]	81.02[a]	73.62[a]	81.02[a]	111.35[a]	117.58[a]
	(59.23)	(76.79)	(112.83)	(142.33)	(113.94)	(142.97)	(29.47)	(36.68)
Observations	38,156	43,184	38,156	43,184	38,156	43,184	38,156	43,184
Adjusted R^2 / Pseudo R^2	0.163	0.143	0.536	0.501	0.527	0.492	0.034	0.035

Table IV
The Impact of the Threat of Short-Selling on Antitakeover Provisions and Severance Packages

This table shows results of Logit and Ordered Logit regressions. The dependent variables are dummy variables equal to 1 if the board of the company is classified (*cboard*), the firm has a blank check preferred provision (*blankcheck*), the firm requires supermajority to approve a merger (*supermajor*), and the firm uses severance packages (*severance*). *Anti+Sev* represents the sum of the four dummy variables (*cboard, blankcheck, supermajor*, and *severance*) *I_Anti+Sev* is a dummy variable equal to one if *Index* is positive. *Pilot* is a dummy variable equal to 1 if the company is in the Pilot Group of REG SHO. *Treatment Years* is a dummy variable equal to 1 if fiscal year is 2005 or 2006. The cut-off estimates of the Ordered Logit regression are not reported. Standard errors are clustered at the firm level. T-statistics are reported in parenthesis. [c], [b], [a] indicate a significance level of less than 10%, 5%, and 1% respectively.

VARIABLES	Logit cboard	Logit blankcheck	Logit supermajor	Logit severance	Logit I_Anti+Sev	Ordered Logit Anti+Sev
Pilot	-0.11	-0.09	0.10	-0.07	-0.20	-0.08
	(-0.92)	(-0.45)	(0.66)	(-0.28)	(-0.75)	(-0.74)
Treatment Years	-0.16[a]	0.02	-0.06	-0.75[a]	-0.08	-0.20[a]
	(-3.51)	(0.27)	(-1.18)	(-3.80)	(-0.62)	(-4.59)
Treatment Years*Pilot	**0.14[b]**	**0.19[c]**	**0.09**	**0.40**	**0.42[b]**	**0.18[b]**
	(1.99)	**(1.74)**	**(1.27)**	**(1.35)**	**(2.26)**	**(2.56)**
Constant	0.40[a]	2.29[a]	-1.73[a]	-2.63[a]	3.13[a]	
	(5.76)	(19.40)	(-18.12)	(-19.31)	(18.38)	
Observations	2,616	2,616	2,616	2,616	2,616	2,616
Pseudo R^2	0.0008	0.0006	0.0009	0.012	0.0013	0.0008

Table V
Changes in the Contractual Environment

Panel A presents the mean daily raw returns for all firms in the sample that were part of the pilot experiment, and firms that were part of the control group. We sort the observations by quintiles based on the value-weighted daily market returns (from CRSP), and then compute the average daily market returns for the pilot and control firms for each quintile. Quintile 1 of the value-weighted daily market returns is the lowest quintile of market daily returns while quintile 5 is the largest. The difference-in-differences measures the change in mean daily returns after the announcement of the Pilot (versus before the announcement of the Pilot) for the pilot group relative to the control group. Point estimates are based on OLS regressions where the daily returns are regressed on a dummy for firms in the Pilot, a dummy variable equal to 1 for the one-year period after the experiment is announced (July 28, 2004) and the interaction term of these two variables. Before is the one-year period before July 28, 2004. Panel B reports the average daily volatility skew of put options on stocks for all firms in the Pilot Group and the Control Group. Volatility Skew is computed as the difference between the implied volatility of out of the money puts (strike price to stock price ratio is less than .9 and more than .7) and at the money puts (strike price to stock price ratio is less than 1.05 and more than .95). Before is the two-month period before July 28, 2004. After is the two-month period after July 28, 2004. Panel D and E reports measures of stock price informativeness: $1\text{-}R^2$ and the probability of informed trading (PIN). In Panel D and E, before is 2003 and after is 2005. Standard errors are clustered at the firm- and date-level in Panel A, B and C, and at the firm-level in Panel D and E. [c], [b], [a] indicate a significance level of less than 10%, 5%, and 1% respectively.

Panel A: Sensitivity to Daily Market Returns

	Before				After					
Quintile	Pilot	Control	Diff.	T-stat	Pilot	Control	Diff.	T-stat	Diff.-in-Diff.	T-stat
1 (Lowest)	-1.46	-1.48	0.03	(1.56)	-1.38	-1.33	-0.05	(-1.44)	-0.07[a]	(-2.63)
3 (Medium)	0.20	0.19	-0.01	(-0.80)	0.11	0.11	-0.00	(-0.09)	-0.01	(-0.66)
5 (Highest)	1.61	1.61	-0.00	(-0.06)	1.34	1.29	0.05	(1.05)	0.05	(1.56)

Panel B: Volatility Skew on Put Options

	Before	After	Difference	T-stat
Pilot Group	7.30[a]	8.85[a]	1.55[a]	(4.29)
Control Group	7.25[a]	8.05[a]	0.80[a]	(2.38)
Difference	0.05	0.80		
T-stat	(0.12)	(1.64)		
Difference-in-differences			0.75[b]	(2.16)

Panel C: Volatility Skew on Call Options

	Before	After	Difference	T-stat
Pilot Group	0.14	0.08	-0.06	(-0.19)
Control Group	-0.16	0.07	0.22	(0.66)
Difference	0.27	0.01		
T-stat	(1.19)	(0.03)		
Difference-in-differences			-0.29	(-1.18)

Panel D: Informativeness 1-R^2				
	Before	After	Difference	T-stat
Pilot Group	0.622[a]	0.665[a]	0.043[a]	(4.17)
Control Group	0.620[a]	0.670[a]	0.050[a]	(6.41)
Difference	0.002	-0.005		
T-stat	(0.20)	(-0.44)		
Difference-in-differences			-0.007	(-0.58)

Panel E: Informativeness PIN				
	Before	After	Difference	T-stat
Pilot Group	0.138[a]	0.102[a]	-0.036[a]	(-17.59)
Control Group	0.135[a]	0.118[a]	-0.016[a]	(-11.45)
Difference	0.003	-0.017[a]		
T-stat	(1.12)	(-7.41)		
Difference-in-differences			-0.020[a]	(-7.99)

Table VI

Difference-in-Difference-in-Differences Analysis

This table shows results of OLS, fixed-effect (FE) and Tobit regressions (left censored at 0 and right censored at 1). The sample period is fiscal year 2003 to 2006. The dependent variables are the ratio of the value of stock options granted to the CEO to the total value of equity grants (*Option/Equity ($)*), and the ratio of the number of stock options granted to the total number of stock options and shares of restricted stock granted to the CEO (*Option/Equity (#)*). *Pilot* is a dummy variable equal to 1 if the company is in the Pilot Group of REG SHO. *Treatment Years* is a dummy variable equal to 1 if fiscal year is 2005 or 2006. *High Downside Risk* is a dummy variable equal to 1 if the firm is in the top quintile of changes in stock price returns sensitivity to negative market returns (measured as changes in firms' stock returns when the daily stock market returns fall into the lowest quintile of stock market return days, as shown in Table V). The change is measured over a one-year period before and after the Reg SHO announcement date. In Panel B and C, *High 1-R^2* and *High PIN* are dummy variables equal to 1 if the difference in these measures between 2005 and 2003 is in top quintile (i.e. highest increase). *High Gindex* is a dummy variable equal to 1 if the G-index in 2004 is in the top quintile. Standard errors are clustered at the firm level. T-statistics are reported in parenthesis. [c], [b], [a] indicate a significance level of less than 10%, 5%, and 1% respectively.

Panel A: High Increase in Downside Risk

VARIABLES	OLS Option/ Equity ($)	OLS Option/ Equity (#)	FE Option/ Equity ($)	FE Option/ Equity (#)	Tobit Option/ Equity ($)	Tobit Option/ Equity (#)
Pilot	-0.94	-0.60			-2.21	-1.44
	(-0.42)	(-0.31)			(-0.32)	(-0.24)
Treatment Years	-19.68[a]	-15.99[a]	-18.77[a]	-15.22[a]	-51.29[a]	-44.35[a]
	(-13.42)	(-11.14)	(-12.95)	(-10.81)	(-12.30)	(-11.37)
Treatment Years*Pilot	3.64	2.52	2.72	1.55	7.01	4.97
	(1.54)	(1.10)	(1.19)	(0.71)	(1.10)	(0.86)
High Downside Risk	0.27	-0.41			4.16	2.48
	(0.09)	(-0.16)			(0.45)	(0.31)
High Downside Risk *Pilot	-2.18	0.21			-9.24	-5.53
	(-0.46)	(0.05)			(-0.65)	(-0.44)
High Downside Risk *Treatment Years	0.16	-0.43	1.07	0.62	-2.04	-2.06
	(0.05)	(-0.12)	(0.31)	(0.18)	(-0.22)	(-0.24)
High Downside Risk *Treat. Years*Pilot	**9.54[c]**	**8.18[c]**	**9.86[c]**	**8.75[c]**	**28.84[b]**	**25.67[b]**
	(1.84)	**(1.67)**	**(1.90)**	**(1.80)**	**(2.06)**	**(2.02)**
Constant	79.43[a]	85.00[a]	78.60[a]	84.39[a]	123.08[a]	126.06[a]
	(63.59)	(77.40)	(155.76)	(173.39)	(28.75)	(33.80)
Observations	3,654	3,682	3,654	3,682	3,654	3,682
Adjusted R^2 / Pseudo R^2	0.059	0.047	0.468	0.468	0.012	0.011

Panel B: High Increase in Price Informativeness – 1 – R^2						
VARIABLES	OLS Option/ Equity ($)	OLS Option/ Equity (#)	FE Option/ Equity ($)	FE Option/ Equity (#)	Tobit Option/ Equity ($)	Tobit Option/ Equity (#)
Pilot	-0.20	0.46			-0.52	0.49
	(-0.09)	(0.24)			(-0.07)	(0.08)
Treatment Years	-20.80[a]	-17.53[a]	-19.06[a]	-15.73[a]	-55.25[a]	-48.54[a]
	(-13.27)	(-11.43)	(-12.14)	(-10.26)	(-11.90)	(-11.20)
Treatment Years*Pilot	6.16[b]	4.46[c]	5.87[b]	4.06[c]	13.57[b]	10.59[c]
	(2.52)	(1.91)	(2.46)	(1.78)	(1.98)	(1.70)
High 1 – R^2	-0.18	-0.60			-1.44	-2.41
	(-0.06)	(-0.24)			(-0.16)	(-0.31)
High 1 – R^2 *Pilot	-6.46	-5.58			-20.44	-18.00
	(-1.28)	(-1.25)			(-1.39)	(-1.38)
High 1 – R^2*Treatment Years	2.75	3.34	2.93	3.41	7.21	8.04
	(0.94)	(1.15)	(0.98)	(1.15)	(0.90)	(1.09)
High 1 – R^2*Treat. Years*Pilot	**-2.44**	**-0.01**	**-5.20**	**-3.16**	**-2.08**	**0.40**
	(-0.49)	**(-0.00)**	**(-1.04)**	**(-0.67)**	**(-0.16)**	**(0.03)**
Constant	79.77[a]	85.33[a]	78.49[a]	84.25[a]	126.03[a]	128.85[a]
	(60.10)	(74.33)	(147.37)	(163.68)	(26.60)	(31.29)
Observations	3,501	3,528	3,501	3,528	3,501	3,528
Adjusted R^2 / Pseudo R^2	0.0620	0.0502	0.4607	0.4620	0.0128	0.0121

Panel C: High Increase in Price Informativeness – PIN						
	OLS	OLS	FE	FE	Tobit	Tobit
VARIABLES	Option/ Equity ($)	Option/ Equity (#)	Option/ Equity ($)	Option/ Equity (#)	Option/ Equity ($)	Option/ Equity (#)
Pilot	-1.68	-0.31			-4.30	-2.03
	(-0.72)	(-0.16)			(-0.62)	(-0.33)
Treatment Years	-21.09[a]	-18.05[a]	-19.94[a]	-16.76[a]	-55.01[a]	-48.30[a]
	(-12.79)	(-11.13)	(-12.05)	(-10.27)	(-11.42)	(-10.71)
Treatment Years*Pilot	6.31[b]	4.83[b]	5.61[b]	4.00[c]	14.80[b]	11.36[c]
	(2.57)	(2.03)	(2.34)	(1.72)	(2.22)	(1.86)
High PIN	7.78[a]	6.43[a]			25.29[a]	22.92[a]
	(3.15)	(3.10)			(2.87)	(2.91)
High PIN *Pilot	3.58	-0.17			8.71	3.44
	(0.71)	(-0.04)			(0.45)	(0.20)
High PIN *Treatment Years	3.30	3.83	3.93	4.37[c]	0.91	0.32
	(1.20)	(1.45)	(1.42)	(1.65)	(0.11)	(0.04)
High PIN *Treat. Years*Pilot	**-5.29**	**-3.86**	**-9.55[c]**	**-7.50**	**-13.99**	**-8.87**
	(-0.94)	**(-0.71)**	**(-1.79)**	**(-1.48)**	**(-0.78)**	**(-0.54)**
Constant	76.73[a]	82.72[a]	77.36[a]	83.36[a]	118.30[a]	122.21[a]
	(53.25)	(64.96)	(144.96)	(160.70)	(24.82)	(29.35)
Observations	3,500	3,528	3,500	3,528	3,500	3,528
Adjusted R^2 / Pseudo R^2	0.0686	0.0562	0.4757	0.4782	0.0139	0.0131

Panel D: High ex-ante Gindex (2004)						
VARIABLES	OLS Option/ Equity ($)	OLS Option/ Equity (#)	FE Option/ Equity ($)	FE Option/ Equity (#)	Tobit Option/ Equity ($)	Tobit Option/ Equity (#)
Pilot	-1.53	-0.34			-5.19	-3.08
	(-0.63)	(-0.16)			(-0.69)	(-0.46)
Treatment Years	-20.52[a]	-17.65[a]	-18.53[a]	-15.89[a]	-55.36[a]	-49.40[a]
	(-13.00)	(-11.47)	(-11.68)	(-10.32)	(-11.93)	(-11.41)
Treatment Years*Pilot	4.68[c]	4.47[c]	3.03	3.08	11.67[c]	10.57
	(1.84)	(1.82)	(1.23)	(1.29)	(1.66)	(1.65)
High Gindex	-7.10[a]	-4.64[b]			-22.89[a]	-19.31[a]
	(-2.71)	(-2.00)			(-3.08)	(-2.90)
High Gindex *Pilot	1.62	1.09			6.86	5.57
	(0.38)	(0.30)			(0.57)	(0.52)
High Gindex *Treatment Years	1.07	3.05	-0.94	1.12	9.46	11.77[c]
	(0.37)	(1.05)	(-0.33)	(0.40)	(1.28)	(1.72)
High Gindex *Treat. Years*Pilot	**2.75**	**-0.78**	**3.96**	**0.06**	**2.89**	**-1.72**
	(0.61)	**(-0.18)**	**(0.89)**	**(0.01)**	**(0.25)**	**(-0.16)**
Constant	79.99[a]	85.23[a]	76.89[a]	83.36[a]	127.08[a]	129.79[a]
	(57.78)	(70.99)	(153.20)	(171.48)	(26.42)	(30.80)
Observations	3,650	3,678	3,650	3,678	3,650	3,678
Adjusted R^2 / Pseudo R^2	0.0632	0.0487	0.4802	0.4792	0.0130	0.0126

Table VII
Contracting and Investment Outcomes

This table shows results of OLS regressions. The sample period is fiscal year 2003 to 2006 and the sample firms are restricted to non-utilities firms in the Pilot Group of REG SHO. The dependent variables are the ratio of capital expenditures to start-of-year total assets multiplied by 100 (*CAPX*), and the ratio of the sum of capital expenditures and research and development expenses to start-of-year total assets multiplied by 100 (*CAPX+R&D*). High Equity Change is a dummy variable equal to 1 if the increase in *Option/Equity ($)* from the 2003-2004 to the 2005-2006 period is in the top decile of the sample distribution. *Option/Equity ($)* is the ratio of the value of stock options granted to the CEO to the total value of equity grants. *Treatment Years* is a dummy variable equal to 1 if fiscal year is 2005 or 2006. Standard errors are clustered at the firm level. T-statistics are reported in parenthesis. [c], [b], [a] indicate a significance level of less than 10%, 5%, and 1% respectively.

VARIABLES	OLS CAPX	OLS CAPX+R&D
High Equity Change	0.97	-0.64
	(0.84)	(-0.48)
Treatment Years	0.08	0.35
	(0.39)	(1.19)
Treat. Years*High Equity Change	**1.90[b]**	**2.59[b]**
	(2.04)	**(2.18)**
Constant	5.62[a]	9.41[a]
	(16.33)	(17.87)
Observations	760	760
R-squared	0.02	0.01

Table VIII
Placebo Tests

This table shows results of OLS, fixed-effect (FE) and Tobit regressions. Tobit regressions are left censored at 0 and right censored at 1. The sample period is fiscal year 2001 to 2004. The dependent variables are the ratio of the value of stock options granted to the CEO to the total value of equity grants (*Option/Equity ($)*), and the ratio of the number of stock options granted to the total number of stock options and shares of restricted stock granted to the CEO (*Option/Equity (#)*). *Pilot* is a dummy variable equal to 1 if the company is in the Pilot Group of REG SHO. *Placebo Treatment Years* is a dummy variable equal to 1 if fiscal year is 2003 or 2004. Standard errors are clustered at the firm level. T-statistics are reported in parenthesis. [c], [b], [a] indicate a significance level of less than 10%, 5%, and 1% respectively.

VARIABLES	OLS Option/ Equity ($)	OLS Option/ Equity (#)	FE Option/ Equity ($)	FE Option/ Equity (#)	Tobit Option/ Equity ($)	Tobit Option/ Equity (#)
Pilot	-0.13	0.31			-0.87	-0.37
	(-0.08)	(0.25)			(-0.14)	(-0.07)
Placebo Treatment Years	-9.03[a]	-6.62[a]	-10.51[a]	-7.70[a]	-30.65[a]	-24.86[a]
	(-8.54)	(-7.16)	(-9.70)	(-8.12)	(-8.08)	(-7.68)
Placebo Treatment Years*Pilot	**-1.83**	**-1.17**	**-1.55**	**-1.13**	**-4.56**	**-2.83**
	(-1.04)	**(-0.73)**	**(-0.86)**	**(-0.70)**	**(-0.75)**	**(-0.55)**
Constant	87.35[a]	90.75[a]	88.03[a]	91.42[a]	152.89[a]	146.82[a]
	(96.85)	(120.08)	(194.43)	(228.05)	(32.28)	(36.66)
Observations	3,793	3,829	3,793	3,829	3,793	3,829
Adjusted R^2 / Pseudo R^2	0.025	0.017	0.482	0.439	0.006	0.005

Table IX
Tests of Alternative Timing and Channels

This table shows results of OLS regressions. The dependent variables are the ratio of the value of stock options granted to the CEO to the total value of equity grants (*Option/Equity ($)*), and the ratio of the number of stock options granted to the total number of stock options and shares of restricted stock granted to the CEO (*Option/Equity (#)*). *Pilot* is a dummy variable equal to 1 if the company is in the Pilot Group of REG SHO. *Treatment Years* is a dummy variable equal to 1 if fiscal year is 2005 or 2006. *Low CAR* is a dummy variable equal to 1 if firm's CAR around the SHO announcement is below the median. Panel A shows results for a restricted sample of firms with fiscal end month ending after the month of July (*Fiscal month end>July*) and a sample period from fiscal year 2003 to 2006. Panel B shows results for a restricted sample period: fiscal year 2003, 2005 and 2006 (*Drop fiscal year 2004*). In Panel C, the sample period is fiscal year 2003 to 2006. Standard errors are clustered at the firm level. T-statistics are reported in parenthesis. [c], [b], [a] indicate a significance level of less than 10%, 5%, and 1% respectively.

	Panel A: Alternative Timing Fiscal month end>July		Panel B: Alternative Timing Drop fiscal year 2004		Panel C: Pricing	
VARIABLES	OLS Option/ Equity ($)	OLS Option/ Equity (#)	OLS Option/ Equity ($)	OLS Option/ Equity (#)	OLS Option/ Equity ($)	OLS Option/ Equity (#)
Pilot	-2.36	-1.01	-1.37	-0.52	-5.20[c]	-3.68
	(-1.04)	(-0.51)	(-0.63)	(-0.28)	(-1.77)	(-1.45)
Treatment Years	-20.87[a]	-17.89[a]	-23.03[a]	-19.81[a]	-20.77[a]	-17.49[a]
	(-14.06)	(-12.23)	(-15.84)	(-14.10)	(-11.23)	(-9.65)
Treatment Years*Pilot	**7.30[a]**	**5.93[a]**	**5.36[b]**	**4.34[c]**	**9.07[a]**	**6.65[b]**
	(3.13)	**(2.61)**	**(2.18)**	**(1.86)**	**(3.02)**	**(2.30)**
Low CAR					0.75	-0.05
					(0.33)	(-0.03)
Low CAR*Pilot					7.59[c]	6.29[c]
					(1.95)	(1.88)
Low CAR*Treatment Years					1.70	1.89
					(0.65)	(0.73)
Low CAR*Treatment Years*Pilot					**-6.25**	**-4.01**
					(-1.48)	**(-1.00)**
Constant	76.69[a]	82.94[a]	81.18[a]	86.89[a]	79.12[a]	84.94[a]
	(57.36)	(70.31)	(65.77)	(81.50)	(47.90)	(58.74)
Observations	3,129	3,154	2,991	3,012	3,739	3,769
Adjusted R^2	0.058	0.049	0.067	0.057	0.062	0.050

Table X
Bootstrapped Distribution of T-statistics for Randomized Samples

This table presents the distribution of t-stats of the OLS regressions when we randomize the selection of firms in the Pilot and Control Group using 5,000 simulations. The t-stats correspond to the DiD coefficient or the interaction variable between the Treatment dummy variable and the Pilot dummy variable in all the differences-in-differences analyses.

	Sensitivity to Daily Market Returns	Option/ Equity ($)	Option/ Equity (#)	Logit cboard	Logit blankcheck
Percentiles	1	2	3	4	5
1%	-2.34	-2.33	-2.32	-2.31	-2.47
5%	-1.72	-1.66	-1.67	-1.61	-1.74
10%	-1.33	-1.29	-1.30	-1.27	-1.33
50%	-0.01	+0.03	+0.04	-0.02	+0.00
90%	+1.33	+1.32	+1.32	+1.29	+1.26
95%	+1.71	+1.70	+1.70	+1.65	+1.62
99%	+2.45	+2.30	+2.34	+2.40	+2.29
Coefficient	DiD	Treatment Years*Pilot	Treatment Years*Pilot	Treatment Years*Pilot	Treatment Years*Pilot
Location	Table V.A.Q.1	Table II.A.1	Table II.A.2	Table IV.1	Table IV.2
Reported T-stat	2.63	2.91	2.38	1.99	1.74
Significance level	1%	1%	1%	5%	5%

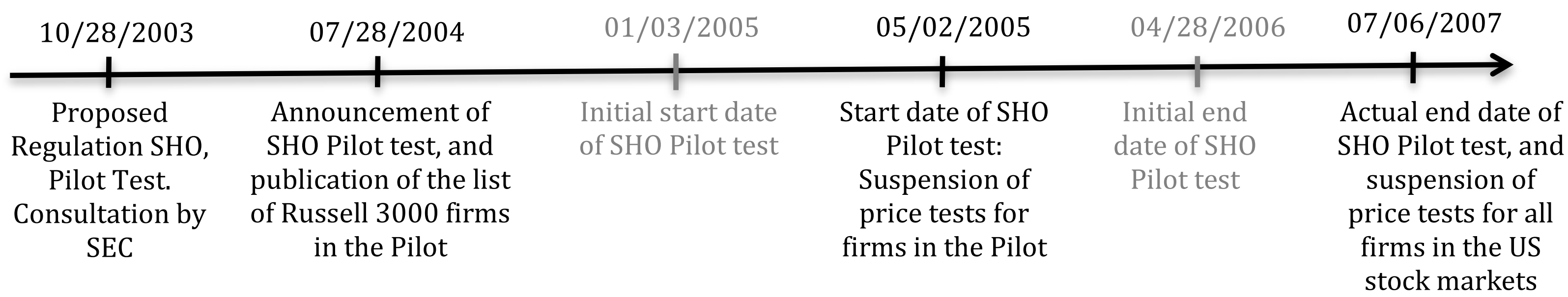

Figure 1. Timeline of the Reg SHO Experiment.

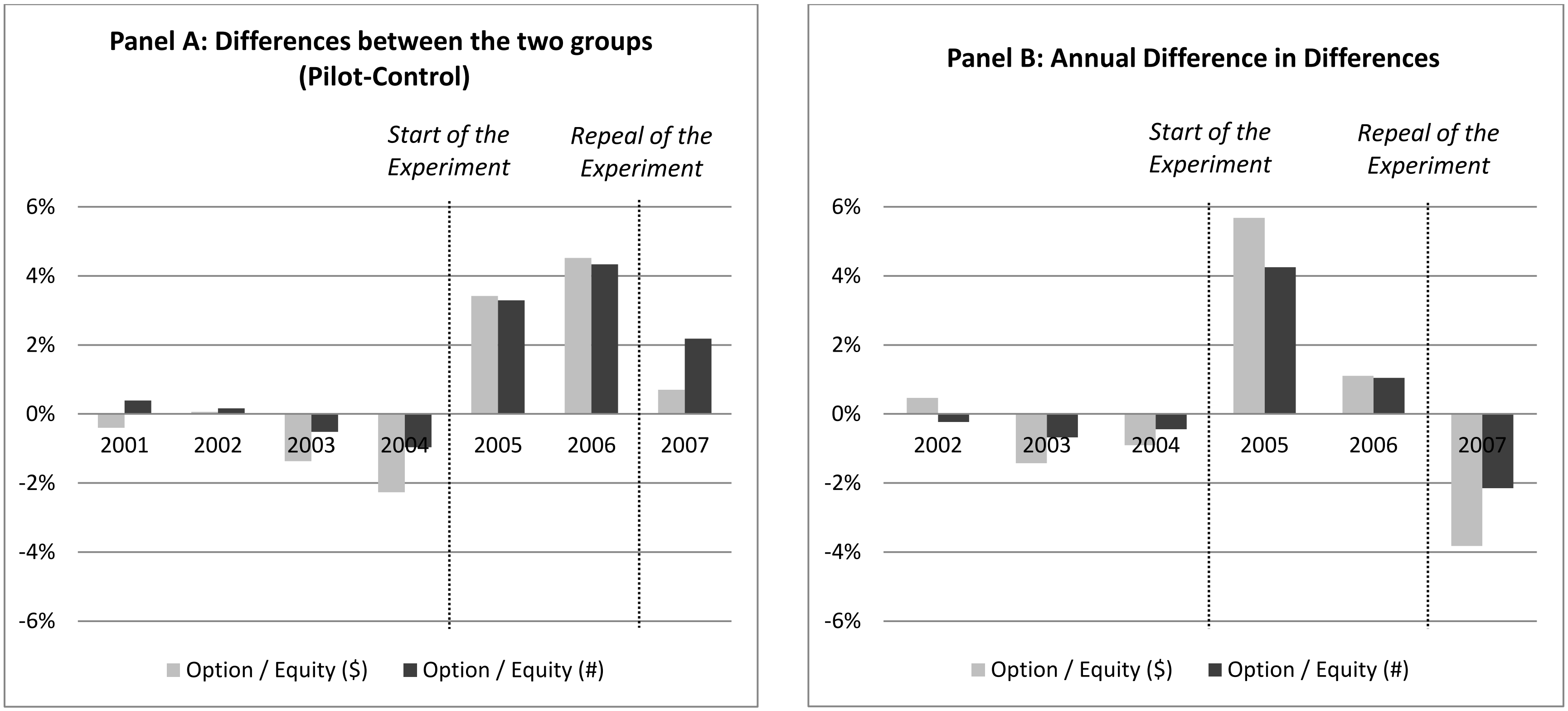

Figure 2. The Structure of Equity Grants: Pilot versus Control Group. Figure 2 compares the evolution of the difference in the structure of CEO equity grants measured by the ratio of stock options granted to total equity grants (in value and in number of shares) between firms in the pilot group and in the control group. Panel A plots the difference of the structure of CEO equity grants between the pilot firms and the control firms in any given year. Panel B plots the annual difference in differences of the structure of CEO equity grants between the pilot firms and the control firms between year t-1 and year t.

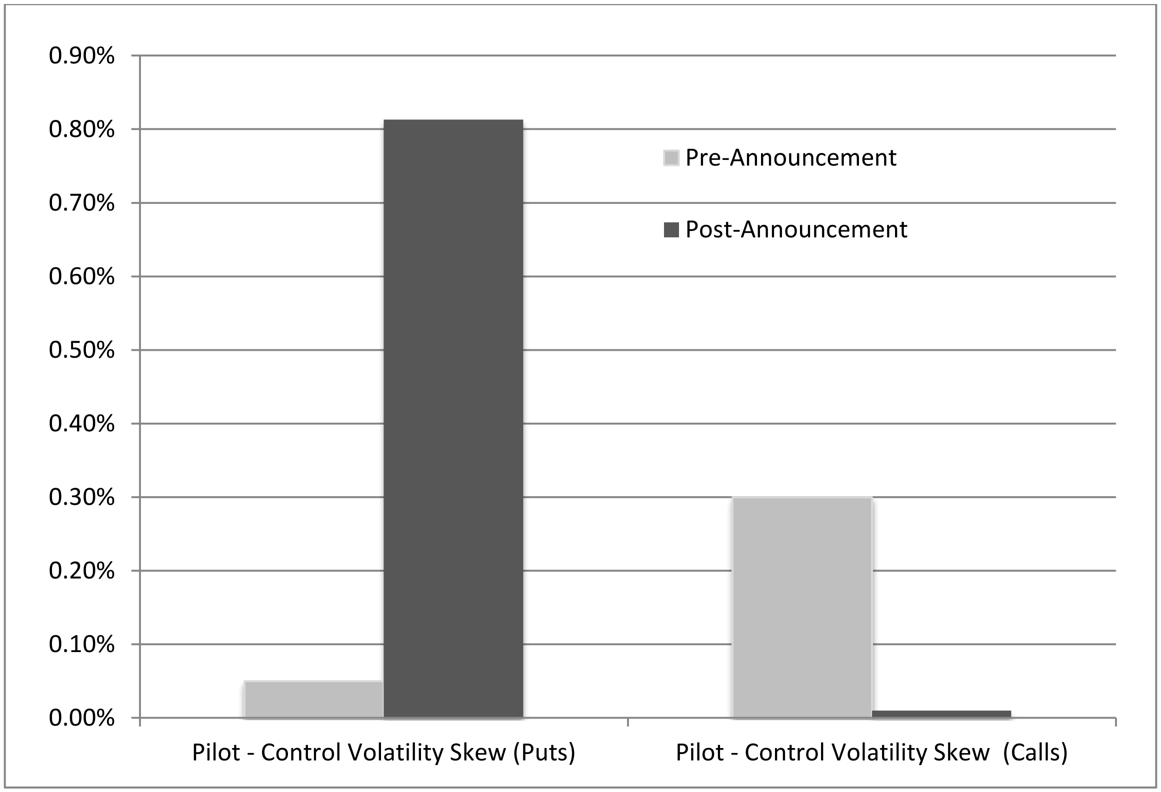

Figure 3. The Increase in Downside Risk in the Options Markets. This figure plots the average difference in implied volatility skew between pilot firms and control firms, both for puts and calls options. The implied volatility skew is defined as the difference between the implied volatility of out-of-the-money puts (calls) on the stock of a firm and the implied volatility of in-the-money puts (calls) on the stock of a firm and is measured at the daily level. We calculate the mean implied volatility skew for the one-month period before the announcement of the Reg SHO experiment on July 28, 2004 (Pre-announcement), and the one-month period following the announcement.