# Short-Termist CEO Compensation in Speculative Markets: A Controlled Experiment*

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December 1, 2020

**Abstract**

Bolton, Scheinkman, and Xiong (2006) analyze a setting where investors disagree and short-sale constraints cause pessimistic views of stock prices to be sidelined, which leads to speculative stock prices. A theoretical implication of the model is that existing shareholders can exploit the speculative stock prices by (1) designing managerial compensation contracts that encourage short-term performance and (2) subsequently selling their shares to more optimistic investors. We document empirical support for this theory by finding that an exogenous removal of short-sale constraints curbs the provision of short-term incentives, an effect reflected in longer CEO compensation duration. The effect is concentrated among stocks with high investor disagreement and short-term-oriented institutional ownership. Consistent with prior work, we also find that longer CEO compensation duration leads to longer CEO investment horizons, less over-investment, and less earnings management.

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*We are grateful for the comments of Ferhat Akbas, Fred Bereskin, John Bizjak, Yongqiang Chu, David De Angelis, Robert DeYoung, David Dubofsky, Lee Dunham, Ben Foster, Stuart Gillan, Mina Glambosky, Harrison Hong, Julie Huang, Randy Jorgensen, Paul Koch, Jiacui Li, Tse-Chun Lin, Tingting Liu, Alexander Ljungqvist, David Mauer, Felix Meschke, Sébastien Michenaud, Vassil Mihov, Charlie Moyer, Lars Oxelheim, Chris Stivers, Zexi Wang, Kenneth Washer, Jide Wintoki, Wei Xiong, and seminar participants and discussants at the 2018 CICF, MFA, AsianFA, SFA, TFA, 2017 SFM, PFMC, Creighton University, Fudan University, National Chengchi University, the University of Dayton, Kansas, North Carolina at Charlotte, and Louisville. We thank Vivian Fang and Chao Jiang for data sharing. Chang and Tseng acknowledge research support from the Ministry of Science and Technology, R.O.C. Taiwan (109-2634-F-002-045, 108-2410-H-002-095-MY2), and the Ministry of Education (109L900202).

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# 1. Introduction

Corporate short-termism, or the tendency of firms to sacrifice long-term value for short-term performance, has been a focal point of debates among academics, practitioners, and policy makers over the past several decades. During the 1980s merger wave, evidence suggests that firms distorted earnings to fend off takeover threats (Stein, 1988; Erickson and Wang, 1999). During the dot-com bubble in the early 2000s, the media reported that insiders had sold shares before stock prices crashed.[1] More recently, there are again concerns over firms engaging in excessive share repurchases to meet earnings targets at the expense of long-term investments (Hribar, Jenkins, and Johnson, 2006; Almeida, Fos, and Kronlund, 2016; Edmans, Fang, and Huang, 2018).

One can make two broad observations from the existing literature. First, extreme episodes of corporate short-termism coincide with periods of high asset market speculation (Bolton, Scheinkman, and Xiong, 2005). For example, in July 2007, on the brink of the recent financial crisis, the CEO of Citibank reportedly questioned the enormous liquidity and valuations in the market, but stated that "as long as the music is playing, you've got to get up and dance."[2] Second, anecdotal evidence suggests that short-termism may in fact reflect shareholders' desire. For example, a Moody's Investor Service report pointed to impatient shareholders' penchant for share repurchases with high leverage deteriorated a firm's credit worthiness (Byrd, Hambly, and Watson, 2007).[3] In particular, the report pointed to famous investors such as Carl Icahn forcing Time Warner and Motorola into share repurchases, asset sales, and spinoff programs.[4]

Against this backdrop, we test and find supporting evidence that short-termism is a result of optimal compensation contracting, through which current shareholders incentivize

[1] For example, the *Financial Times* reported that executives and directors from 25 failed U.S. companies during the dot-com bubble grossed about $3.3 billion (see *Financial Times*, 31 July 2002).

[2] https://www.reuters.com/article/financial-crisis-dancing/ex-citi-ceo-defends-dancing-quote-to-u-s-panel-idUSN0819810820100408

[3] https://www.moodys.com/sites/products/AboutMoodysRatingsAttachments/2006600000441326.pdf

[4] Similarly, Larry Fink of BlackRock, in his 2016 letter to CEOs of leading companies, also urged firms to emphasize long-term value creation to avoid the pressures of impatient shareholders (Fink, 2016).

managers to emphasize short-term stock performance in a speculative market. We design a series of tests based on the model of Bolton, Scheinkman, and Xiong (2006) (hereafter, BSX) for equity-based executive compensation. In this model, the only departure from the classical efficient contracting framework of Holmström and Tirole (1993) is to allow for a speculative stock market, due to investor disagreement. In a stock market with short-sale constraints, disagreement among investors results in a speculative component in the stock price because pessimistic views are sidelined (Miller, 1977; Morris, 1996; Hong and Stein, 2007). Consequently, existing shareholders in this model design optimal equity-based compensation contracts that encourage short-term performance, in hopes of increasing the speculative component in the stock price and selling their shares to more optimistic investors in the near future (Scheinkman and Xiong, 2003).

This model yields directly testable implications related to its two key components: short-sale constraints and investor disagreement. First, we expect to observe fewer short-termist incentives in managerial compensation if constraints on short-sales are removed. Because short-selling enables the market to include pessimistic views and decreases the speculative component in stock prices, short-termism becomes less attractive to existing shareholders. The theoretical premise that short-selling promotes price efficiency and market quality can be traced back to at least the classic work of Diamond and Verrecchia (1987), and empirical studies inspired by the recent financial crisis also confirm this view (Beber and Pagano, 2013; Boehmer and Wu, 2013).

Second, when short-sale constraints are removed, firms with high investor disagreement over their fundamental values should reduce short-termist incentives in managerial compensation more than firms with low investor disagreement. Since firms with high investor disagreement have a relatively larger speculative component in their stock prices, there exist high incentives for manipulating stock prices with short-termist incentives. Therefore, the marginal effect of short-selling on short-termism for these firms is high. On the contrary, if investors generally have a consistent view with respect to a firm's value, then little market speculation exists from the onset and, thus, the binding nature of short-sale constraints is less relevant.

Testing these model implications is challenging. First, due to endogeneity concerns, a correlation between a change in executive compensation short-termism and a change in short-selling activities is hard to interpret; omitted variables (e.g., negative private signals) might drive the behavior of both short-sellers and corporate insiders. Also, reverse causality can prove to be a problem, in which case it is actually the structure of executive compensation that drives short-sale activities, which is plausible given media attention on short-termism during periods of asset overvaluation. Second, one would need a measure to quantify short-term incentives in executive compensation.

Our identification strategy to address endogeneity is to exploit a randomized experiment by the Securities and Exchange Commission (SEC) during the Regulation SHO program, which relaxed the short-sale constraints for a group of pilot stocks from 2005 to 2007. Specifically, Rule 202T of Regulation SHO lifted short-sale price tests for every third stock in the Russell 3000 index, as sorted by trading volume within each stock exchange.[5] Recent studies document an increase in short-selling activities for these Regulation SHO pilot firms, which reflects the binding nature and economic significance of short-sale price tests (Diether, Lee, and Werner, 2009; Grullon, Michenaud, and Weston, 2015). Importantly, the Regulation SHO pilot program represents an exogenous shock to short-sale constraints on a randomized set of firms, allowing us to address the endogenous nature of short-selling. Furthermore, the pilot program has clear beginning and ending dates, which allows us to conduct difference-in-difference tests (hereafter, DiD), so we may observe the effect of short-selling on compensation structure both during and after the pilot program.

To quantify the extent of short-termism in executive compensation, we construct a measure to reflect the duration of each CEO's annual compensation grants. Following Gopalan

[5]Before Regulation SHO, the NYSE restricted short-selling prices to be either above the most recent traded price, or at the most recent traded price if that price was above the most recent different price. NASDAQ required short-sale prices to be one cent above the bid price if it was below the previous bid. The SEC eliminated short-sale price tests for all exchanges after the Regulation SHO program.

et al. (2014), we measure CEO compensation duration (*CPD*) as the weighted average vesting periods of the different components in each CEO's annual compensation package, which includes salary, bonus, restricted stocks, and stock options. The weights are then the relative value of each component in the entire compensation package. Consistent with intuition, Gopalan et al. (2014) show that firms with more long-term projects or future growth opportunities have longer *CPD*s.[6,7]

Our DiD estimates show that pilot firms have close to 10% longer *CPD*s relative to non-pilot firms during the Regulation SHO program from 2005 to 2007, when compared to the difference in pre-program years.[8] This difference in *CPD* is not statistically significant after the program concludes, thus providing a validity check on our DiD framework. Our baseline results are consistent with BSX; that is, shareholders use short-term compensation incentives to induce CEOs to focus on short-term performance in a speculative market with investor disagreement and short-sale constraints. Once short-sale constraints are removed, shareholders choose to focus on long-term fundamentals.

Confirming our second model prediction, we show that the compensation duration effect in our baseline regressions is concentrated among firms with pre-existing high investor disagreement. We use two standard measures of investor disagreement to partition our sample: analyst forecast dispersion (Diether, Malloy, and Scherbina, 2002), and abnormal turnover (Chen, Hong, and Stein, 2001). Both measures of disagreement yield similar results. For example, the DiD estimates of the compensation duration effect during the Regulation SHO

[6]Anecdotal media coverage suggets that vesting schedules and provisions strongly influence executive behavior. For example, WhatsApp co-founder Jan Koum announced his intended departure from Facebook in April, 2018. However, he continued to show up at Facebook after the announcement to ensure that his stock grants were fully vested in November, 2018.

[7]There are other measures that characterize different dimensions of CEO compensation. Two widely used correlations, *Delta* and *Vega*, measure CEO pay sensitivity to stock price performance and volatility, respectively. Our objective is to quantify the time dimension in compensation incentives by explicitly accounting for the length of both stock and option grants' vesting schedules. Gopalan et al. (2014) show that *CPD* explains managerial behavior beyond that of *Delta* and *Vega*. In our empirical tests, we show that our results are robust when we control for *Delta* and *Vega*.

[8]It is worth emphasizing that the 10% difference in *CPD* is due to the removal of one aspect of short-sale constraints: short-selling price tests. In practice, other short-sale constraints exist (e.g., explicit borrowing costs, institutional restrictions). For example, individual investors and mutual funds rarely short stocks (Almazan et al., 2004).

program are 15% to 26% for firms with high analyst forecast dispersion. In contrast, the DiD estimates are economically and statistically insignificant for firms with low analyst forecast dispersion.

We next explore the role of existing shareholders, who play a critical role in the model as they are the ones incentivizing CEOs to be short-termist. Large institutional shareholders not only have considerable influence over CEO compensation designs (Shleifer and Vishny, 1986; Black, 1992), but are also better monitors of corporate compensation since they typically hold larger stakes and possess greater governance expertise (Edmans, Gabaix, and Jenter, 2017). Consistent with this view, Hartzell and Starks (2003) find that higher institutional ownership leads to more incentive-compatible executive compensation designs. In other words, influential institutional shareholders align managerial compensation to their preferences. Therefore, if a firm's institutional investors mostly focus on short-term results, then a direct implication from BSX is that these investors will exert more short-term pressure on management to exploit market speculation.

Accordingly, we expect the marginal benefits of encouraging short-termism to drop when short-sale constraints are exogenously removed, especially for firms with more institutional investors that focus on short-term performance. Supporting this hypothesis, we find that firms with pre-existing high levels of ownership by short-term-oriented institutional investors (relative to long-term-oriented investors) have a considerably larger compensation duration effect. Among these firms, the DiD estimates in compensation duration is 16% to 21%. In contrast, we do not find a significant DiD estimate among firms with low levels of relative ownership by short-term-oriented institutional investors.

Finally, we explore the consequences of providing short-term compensation incentives due to speculative motives. If compensation duration increases due to an exogenous shock to short-sale constraints, we should expect less short-termist corporate policies and CEO behavior. To identify these effects, we use two-stage regressions in which we regress the variables of interest on CEO compensation duration, which is in turn instrumented by Regulation

SHO. First, BSX hypothesize that existing shareholders use short-termist compensation to incentivize CEOs to invest more in wasteful "castle-in-the-air" projects in a speculative market. These inferior projects lead to additional investor disagreement and speculation, further boosting stock prices. We follow Polk and Sapienza (2009) and define firms that have above-median industry-year capital expenditures as overinvesting. As expected, we find that longer CEO compensation duration leads to a lower propensity to overinvest. Second, we expect to observe fewer earnings management activities with longer CEO compensation duration. A clear example of short-termist behavior, earnings manipulation benefits existing shareholders and potentially harms future shareholders. More generally, earnings management can manifest in forms of stock price manipulation or delayed investments such as R&D (Graham, Harvey, and Rajgopal, 2005). We examine three different corporate decisions that are intimately linked to earnings management: R&D intensity (Bushee, 1998), repurchases (Hribar, Jenkins, and Johnson, 2006), and meeting analysts' forecasts (Malmendier and Tate, 2009). We show that longer CEO compensation duration leads to a lower propensity to manage earnings in all three dimensions. Third, we expect CEOs to exhibit longer trading horizons in their own companies' stocks. Akbas, Jiang, and Koch (2018) construct an investment horizon measure of company insiders and find that this measure is positively correlated with CEO compensation duration. Intuitively, executives willing to accept longer compensation incentives tend to have longer investment horizons. Consistent with this finding, we show that longer compensation duration leads to longer CEO trading horizons.

A study closely related to ours is De Angelis, Grullon, and Michenaud (2017), who find that Regulation SHO pilot firms grant more options to their CEOs as a protection mechanism against bear raids from short sellers. While both papers study CEO compensation designs, we differ substantially in terms of theoretical motivations and, accordingly, empirical variables of interest. Firstly, our paper focuses on testing the *time* dimension in CEO compensation, which is directly motivated by BSX. Specifically, firms will be less inclined to design short-term CEO compensation contracts when it is easy for short-sellers to incorporate negative views in stock prices; that is, there is less room for firms to manipulate the speculative component in stock prices. Accordingly, our measure of CEO compensation *duration*, which matches tightly to BSX, considers the average vesting period of all types of grants, including stocks, options, salary, and bonus. In addition, a direct implication of BSX is that eliminating short-sale constraints will lead to less investor disagreement. We design our empirical tests and provide supporting evidence for these claims.

De Angelis, Grullon, and Michenaud (2017), on the other hand, focuses on the *risk-taking* dimension in CEO compensation. Expecting elevated volatility from uninformed bear raids by short sellers, their economic mechanism emphasizes additional risk-taking incentives in CEO compensation during Regulation SHO. Accordingly, their empirical tests surround the *convexity* of CEO compensation, which is primarily due to more option grants. In contrast to BSX, the bear raid hypothesis is silent in terms of the *time* dimension across all types of compensation grants. In fact, the bear raid hypothesis implies shorter vesting periods, which is opposite to BSX and our findings, since it is not optimal to encourage risk-taking by granting more options with longer vesting periods when elevated volatility is short-lived. The bear raid hypothesis is also silent regarding the implications of short-selling on investor disagreement.

Recently, Black et al. (2019) and Black et al. (2020) critique studies that use Regulation SHO on the grounds that short-selling activities only increase modestly (if at all) and there is little news coverage around the experiment. In addition, they argue that some findings in this literature are not robust to alternative specifications and thus are products of data-mining. Our view on this critique is threefold. First, short-selling activites do increase, as documented by various studies in this literature (Grullon, Michenaud, and Weston, 2015; Diether, Lee, and Werner, 2009; Alexander and Peterson, 2008). In Section 4.1 of this paper, we also show evidence that short selling increases for the pilot firms in our sample. More importantly, *observed* short selling activities do not necessarily have to increase for the pilot firms. As Fang, Huang, and Karpoff (2019) point out, firm behaviors will change due to *ex* *ante* expectations of elevated short selling, and *ex post* short-selling opportunities may fade.[9] Second, while Regulation SHO did not draw much attention from popular press, we provide anecdotal evidence in Section 3.3.1 that firms lobbied for continued short-selling restrictions at the time. This evidence suggests that company boards were fully aware of and paid close attention to the regulatory change. While this evidence does not explicitly show that, in our context, Regulation SHO lead to compensation redesigns, it points to the plausibility of the theoretical link proposed by BSX. Third, we agree with Black et al. (2019) that data mining is undesirable and leads to false positive results. This is precisely why our test designs are guided by the theoretical predictions of BSX. Theory-motivated tests alleviate concerns with respect to data-mining.

Our findings make three contributions to the literature. First, they speak to the root causes of corporate short-termism. Various theoretical works explain managers' short-termist behavior as detrimental to current shareholders' interests by using imperfections such as manager signal jamming (Narayanan, 1985; Stein, 1989; Goldman and Slezak, 2006; Sun, 2014; Peng and Röell, 2008b, 2014), asymmetric information (Von Thadden, 1995), or heterogeneity in limits to arbitrage (Shleifer and Vishny, 1990). Our paper provides evidence for a complementary view by BSX. In this model, short-termism is consistent with the current shareholders' objective function. This view of managerial compensation reconciles the seemingly contradictory trends of improved corporate governance and persistent short-termism. Our findings suggest that shareholders exploit market speculation by providing short-term incentives, which in turn contributes to corporate short-termism. In Section 2, we review the literature on corporate short-termism and, in particular, the role of executive compensation.

Second, our results offer potential policy implications with respect to short-termism. If we accept the optimal contracting view of managerial compensation and recognize that

[9]We acknowledge the arguement by Black et al. (2020) that the statistical and economic significance of short-selling results in the Regulation SHO studies may vary depending on methods of sample construction, definitions of sample periods, or choices of empirical specifications. Reconciling these differences in the literature is beyond the scope of this paper and we leave this to future research. Instead, we show evidence within our sample and point the readers to other studies in this literature in Section 2.1.

transient institutional investors are the driving forces, then introducing more shareholder activism, strengthening corporate governance, or even increasing institutional ownership may not be ideal policy options. For instance, Cadman and Sunder (2014) show that VC-backed IPO firms use short-term incentives to boost stock prices as venture capital exits, which provides an example of institutional investors exacerbating rather than mitigating managerial short-termism. In a similar vein, Asker, Farre-Mensa, and Ljungqvist (2015) find that stock market-listed firms exhibit more short-termism than private firms, due to pressures from transient institutional shareholders. Accordingly, addressing short-termism may lie in less speculative stock prices, potential restrictions on CEOs unwinding stock holdings, or mechanisms to tie compensation incentives to longer-term performance of company stocks.

Third, this paper contributes to the literature that explores the link between secondary financial markets and their effects on real decision-making (for a review, see Bond, Edmans, and Goldstein, 2012). Stock prices aggregate information in the secondary market and potentially guide real decisions such as corporate investments. This idea can be traced back to Hayak (1945) and Fama and Miller (1972). For example, stock prices can affect corporate decisions through trading between insiders and outside speculators (Fishman and Hagerty, 1992; Leland, 1992; Khanna, Slezak, and Bradley, 1994), securities issuances (Boot and Thaker, 1997; Subrahmanyam and Titman, 1999), and cross-listings (Foucault and Gehrig, 2008). In particular, our paper belongs to a strand of this literature in which real decisions are affected by stock prices through a manager's contract incentives (Holmström and Tirole, 1993). In this regard, we provide a more comprehensive review of the role of short-selling in managerial compensation and real decisions in Section 2.

The rest of the paper is organized as follows. After we review the literature in Section 2, we describe our sample, key variable definitions, and our identification strategy in Section 3. In Section 4, we report our tests of the effects of short-sale constraints and investor disagreement on CEO compensation duration. In Section 5, we provide evidence on mechanism and consequences of short-termist incentives, and we conclude this paper in Section 6.

# 2. Literature Review

## 2.1. Short Selling

Our study contributes to a large body of research examining the role of short selling in stock prices and corporate behavior.[10] Classical theoretical works suggest that short-sale constraints have an adverse effect by limiting the extent to which pessimists can express their views in prices. Diamond and Verrecchia (1987) show that short-sale constraints impede the diffusion of value-relevant negative information into prices. Miller (1977) argues that investor disagreement combined with short-sale constraints lead to overpricing. Building on these models, more recent theoretical works suggest that short-sale constraints contribute to bubbles and excess volatility (Allen, Morris, and Postlewaite, 1993; Abreu and Brunnermeier, 2002, 2003) or market crashes (Hong and Stein, 2003). In particular, dynamic models that incorporate short-sale constraints and investor disagreement can jointly explain asset bubbles and excessive trading (Harrison and Kreps, 1978; Scheinkman and Xiong, 2003; Hong, Schienkman, and Xiong, 2006). Building upon this idea, BSX propose a model in which shareholders incentivize CEOs' short-termist behavior to manipulate earnings and overinvest in inferior projects.

Theoretical works also point out potential negative effects of short selling. For example, predatory short selling forces margin long positions to sell and profits from further depressed stock prices (Brunnermeier and Pedersen, 2005). Similarly, uninformed short selling could induce managers to scrap profitable projects through feedback effects (Goldstein and Guembel, 2008; Khanna and Mathews, 2012; Goldstein, Ozdenoren, and Yuan, 2013). These views are popular among practitioners (Soros, 2009) and provide theoretical support for regulatory short-selling bans, such as the one issued by the SEC during the financial crisis in 2008.[11]

Empirical tests have supported both the pros and cons of short selling. Earlier empirical

[10]Short sellers are typically viewed as sophisticated market participants. For example, prior research suggests that they are more informed than stock analysts (Drake, Rees, and Swanson, 2011) and insiders (Khan and Lu, 2013).

[11]Securities Exchange Act Release No.34-58952.

works typically use short interest as a measure of binding short-sale constraints and relate this measure to future lower stock returns (Figlewski, 1981; D'Avolio, 2002). Dechow et al. (2001) and Aitken et al. (1998) show that short sellers take positions in stocks with high valuations that eventually mean-revert. Similarly, Jones and Lamont (2002), Chang, Cheng, and Yinghui (2007), and Beneish, Lee, and Nichols (2015) provide evidence that short-sale constraints lead to high valuations and low subsequent returns. Pownall and Simko (2005) and Saffi and Sigurdsson (2011) show that short sellers promote price discovery, especially when there is a poor information environment.

While empirical works in general have found support for better price informativeness from short selling, studies have also identified its potential destabilizing effects. Bris, Goetzmann, and Zhu (2007) find in an international sample that short-sale constraints are associated with less negative return skewness. Similarly, Henry and McKenzie (2006) show that short selling leads to elevated volatility in the Hong Kong market. Around earnings announcements, Hong, Kubik, and Fishman (2012) and Savor and Gamboa-Cavazos (2011) both find overreactions to positive earnings shocks for stocks that are heavily shorted. In a similar vein, Henry and Koski (2010) find that manipulative short selling before SEO announcements leads to less efficient stock prices. Finally, Battalio and Schultz (2006), Kaplan, Moskowitz, and Sensoy (2013), and Beber and Pagano (2013) find that short-sale constraints have little impact on prices, contrary to the view that short-selling bans lead to elevated stock prices and volatility.

One challenge in resolving this debate is the endogenous nature of short-selling activities. The Regulation SHO program by the SEC provides an ideal empirical setting to test the role of short-sale constraints on various aspects of capital markets. Alexander and Peterson (2008) and Diether, Lee, and Werner (2009) both document that shorting activities of pilot firms increase more than those of control firms, suggesting that short-sale constraints are binding. Importantly, Grullon, Michenaud, and Weston (2015) show that stock prices of the Regulation SHO pilot firms underperform those of control firms, and that this effect is permanent. This evidence supports the view that short-sale constraints impede pessimistic traders with value-relevant information and induce asset overvaluation.

Our study takes a step further in this direction and studies the impact of Regulation SHO on compensation contract designs. When there is little room to manipulate investor disagreement and the resale option component in stock prices, we find that shareholders lengthen the time dimension in CEO contracts. In addition, this change in incentives then leads to less overinvestment and earnings management. These findings are consistent with those of Fang, Huang, and Karpoff (2016), Grullon, Michenaud, and Weston (2015), and He and Tian (2016). We contribute to this literature by providing an economic mechanism to these findings.

Finally, other recent studies investigate Regulation SHO's impact on other corporate decisions and market participants. Echoing De Angelis, Grullon, and Michenaud (2017), Li and Zhang (2015) find that pilot firms' managers reduce the precision and readability of their bad news disclosures to try and maintain stock price levels. He and Tian (2016) hypothesize that short sellers mitigate managerial myopia and show that pilot firms' innovation efficiency improves. Finally, Hope, Hu, and Zhao (2017) show that auditors charge higher audit fees to pilot firms, supposedly reacting to heightened litigation risks.

## 2.2. Corporate Short-Termism and Executive Compensation

Our paper is also part of an important and growing literature in corporate short-termism. In addition to the theoretical works referenced above, there is much empirical evidence suggesting important consequences of short-termism. Budish, Roin, and Williams (2015) use cancer treatment research data to show that short-termism leads to underinvestment in long-term research. Similarly, Edmans, Fang, and Lewellen (2017) and Edmans, Fang, and Huang (2018) find that corporate short-termist incentives lead to fewer real investments and more value-reducing actions. In their survey of financial executives, Graham, Harvey, and Rajgopal (2005) find that the majority of executives would sacrifice long-term firm values in favor of better earnings.

While there is much consensus on the existence of corporate short-termism, there is a lively debate in the empirical literature on whether equity compensation alleviates short-termism (for a review, see Edmans, Gabaix, and Jenter (2017)). Earlier studies find that managerial stock ownership prevents managers from opportunistically cutting R&D spending (Dechow and Sloan, 1991; Cheng, 2004). Similarly, Chen et al. (2015) show that better contractual protection of CEO compensation eases managerial myopia. On the contrary, other papers show a positive link between executive equity compensation and earnings management (Cheng and Warfield, 2005; Bergstresser and Philippon, 2006; Peng and Röell, 2008b, 2014) and financial misreporting (Burns and Kedia, 2006; Efendi, Srivastava, and Swanson, 2007).

We believe that part of the difficulty in resolving this debate lies in the endogenous nature of executive compensation contracts. It is challenging to find variations in executive compensation contracts due to exogenous shocks. Our study contributes to this literature by exploiting an as-good-as-random Regulation SHO experiment that alleviates this concern. With this empirical approach, we attempt to highlight the potential impact of executive compensation on corporate behavior.[12]

# 3. Data

## 3.1. Sample

On July 28, 2004, the Securities and Exchange Commission (SEC) published a list of 986 stocks that would be relieved of short-sale price tests during the Regulation SHO program.

[12]A paper that shares a similar theoretical motivation as ours is Egger and Radulescu (2014). This study finds a correlation between a firm's shareholders' speculative tendency (measured by institutional shareholders' asset turnover rate) and its CEO's option vesting periods. While this correlation is consistent with BSX, it does not speak to the economic mechanisms in the model. In other words, this correlation can also be consistent with a story with no disagreement and short-sale constraints. For example, the paper's finding is also consistent with the clientele hypothesis in Hartzell and Starks (2003); that is, institutional investors choose the type of firms with compensation structures that are consistent with their preferences.

To determine the firms eligible for the program, the SEC started with the 2004 Russell 3000 index constituents and excluded firms that were not listed on NYSE, Amex, or NASDAQ. The SEC further omitted firms that had IPOs or spin-offs after April 30, 2004. The remaining stocks were then sorted by their average daily dollar volume from June 2003 to May 2004 within each exchange. Every third stock on these rankings was selected as a pilot firm.[13] Our initial sample consists of 986 treatment firms and 1,970 control firms. When we restrict our sample to firms for which we can measure CEO compensation duration (defined below) and other firm characteristics, our final sample (*Full sample*) contains 356 treatment firms and 704 control firms. Alternatively, we also follow De Angelis, Grullon, and Michenaud (2017) and construct a balanced sample, in which we require firms to be in all three years of the pre-treatment period, the first year of the treatment period, and the first year of the post-treatment period (*Balanced sample*). This sample contains 151 treatment firms and 279 control firms.

Throughout this paper, we present results with both the *Full sample* and the *Balanced sample*. To the extent that the experiment is randomized and there is no differential attrition across the pilot and control firms, then the *Full sample* is preferred. With a larger sample, the *Full sample* estimates have better efficiency and generalizability (Baltagi and Li, 1990, 1994). In Section 3.4, we show, in our sample, evidence consistent with random assignment and parallel trends. In addition, sample attrition rates over the experiment in our sample are 25% for both pilot and control firms. In the Regulation SHO literature, Grullon, Michenaud, and Weston (2015) and Hope, Hu, and Zhao (2017) primarily focus on unbalanced samples. Nevertheless, there are studies such as Fang, Huang, and Karpoff (2016) that rely primarily on a balanced-sample approach, which has the advantage that regression estimators are derived from firms that operate through the experiment (Fang, Huang, and Karpoff, 2019). A balanced-sample approach is also preferred when there is differential sample attrition across pilot and control firms. Following De Angelis, Grullon, and Michenaud (2017), we

[13]We thank Vivian Fang for sharing the Ruessell 3000 constituents and pilot firm list with us.

show results with both approaches to emphasize that our results are not sensitive to sample contruction.

### 3.2. CEO Compensation Duration

The main dependent variable in this paper is CEO compensation duration ($CPD$), which is the weighted average vesting period of compensation components including salary, bonus, restricted stocks, and stock options. Specifically, we follow Gopalan et al. (2014) and calculate $CPD$ for each CEO-year as follows:

$$CPD = \frac{(Salary + Bonus) \times 0 + \sum_{i=1}^{n_s} Restricted\ Stock_i \times t_i + \sum_{j=1}^{n_o} Option_j \times t_j}{Salary + Bonus + \sum_{i=1}^{n_s} Restricted\ Stock_i + \sum_{j=1}^{n_o} Option_j}, \quad (1)$$

where $i$ and $j$ index restricted stock grants and option grants, respectively. $Salary$ and $Bonus$ are the dollar values of annual salary and bonus. $Restricted\ Stock_i$ is the dollar value of restricted stock grant $i$ with vesting period $t_i$ (in months), and $Option_j$ is the dollar value of option grant $j$ with vesting period $t_j$ (in months).[14] $n_s$ and $n_o$ are the number of stock and option grants a CEO receives in a year.[15]

To estimate equation 1, we obtain detailed compensation data from the Incentive Lab database by Institutional Shareholder Services (ISS). This database contains detailed grant-by-grant information with respect to equity compensation, such as vesting schedules, vesting periods, and fair values (Bettis et al., 2018; Huang, 2016). The sample of executives covered by the Incentive Lab comes from the S&P500 and a significant portion of the S&P400 (mid-cap firms).

For firms covered by Incentive Lab but with missing CEO compensation information over

[14]We use fiscal year-end stock prices and the Black-Scholes model to estimate the values of restricted stock grants and option grants. In Section 4.3.1, we show that our empirical results are similar if we use grant-day values.

[15]We assume the dollar value and vesting period to be zero for each single grant if either of these variables is missing. This way, we retain as much information as possible when aggregating across grants. However, in cases for which all grants for a CEO have missing information on either dollar value or vesting period, we choose to drop these observations to be conservative.

our sample period, we hand-collect data from corporate filings in SEC's EDGAR system whenever possible. A firm's CEO compensation information is typically in its DEF 14A filing, which provides a Summary Compensation Table that contains annual salary and bonus information. Information on annual grants of restricted stocks and options for a particular year is also typically included in the DEF 14A for that year, or the next couple of years. We use keyword searches within these files to locate value and vesting information for annual equity grants.[16] If the DEF 14A is missing, or if it does not contain sufficient data, we manually search the firm's 10-K or other proxy material filings (DEFC 14A or DEFR 14A) for available compensation information.[17]

We report the summary statistics of *CPD* in the first row of Table 1. Over our sample years, *CPD* has an average and median of 20.14 and 21.18, respectively. The standard deviation is 11.32, showing a healthy cross-sectional variation.

## 3.3. Key Variable Definitions

In this section, we provide the definitions of key variables in our baseline specification. Definitions for all other variables used in this paper are in Appendix A.

### 3.3.1. Regulation SHO

We define three periods of interest for our study. The indicator variable *PRE* denotes firm-years before the Regulation SHO pilot program from 2001 to 2003. The second period, the three years (2005 to 2007) during which pilot firms did not have short-sale price tests, is denoted by the indicator variable *DURING*. We denote the post-Regulation SHO years (2009 to 2011) with an indicator variable *POST*. Finally, we use an indicator variable *PILOT* to denote treatment firms in the pilot program.

[16]For example, the 2003 compensation information of Gregory L. Quesnel, CEO of CNF Inc., can be found in the firm's DEF 14A filing (pp. 16–22).

[17]For example, the 2005 annual compensation information of Cristóbal Conde, CEO of Sungard Data Systems Inc., can be found in item 11 ("Executive Compenation") of its 10-K filing (pp. 85–94).

We follow Fang, Huang, and Karpoff (2016) and skip one year between pre-Regulation SHO years and during-Regulation SHO years because pilot firms were announced on July 28, 2004 and implemented on May 2, 2005. This design also takes into account the time for CEOs to re-negotiate their compensation contracts. In particular, the SEC first announced the list of pilot firms on July 28, 2004, following internal approval by the SEC board on June 23, 2004 (Grullon, Michenaud, and Weston, 2015). Therefore, the earlist time a CEO could start contract negotiation was on June 23, 2004.

While the negotiation process of CEO compensation is not observable, it is plausible that firms started CEO contract re-negotiations immediately for the following reasons. First, listed firms were clearly paying much attention to the potential move to relax short-sale constraints before the Regulation SHO program. For example, Darla C. Stucky of the NYSE wrote an open letter to the SEC in 2004 stating that "the Exchange, its members and its listed companies strongly support continued price restrictions for short sales in all securities."[18] In another incident, David Humphreville of the Specialist Association also wrote an open letter to the SEC in 2004 stating that "managements of listed companies correctly believe that the protections afforded by existing rule 10a-1 are an enormously important attribute of exchange listing."[19] This anecdotal evidence suggests that Regulation SHO is an important regulatory change for pilot firms.

Second, Shue and Townsend (2017) use realized option grants and infer that 61.14% of the compensation cycles last for two years. In other words, 3 out of 5 CEOs renegotiate their compensation packages every two years. Therefore, our design allows for considerably more time than the majority of CEO contracting cycles. The compensation cycle serves as an upper-bound for the time of contract renegotiation without market changes. Since the listed firms were clearly paying attention to Regulation SHO, a CEO would plausibly start contract renegotiations immediately after the announcement of the pilot program without letting the full compensation cycle run its course. For example, Saly (1994) studies the October 1987

[18]https://www.sec.gov/rules/proposed/s72303/dstuckey03012004.htm
[19]https://www.sec.gov/rules/proposed/s72303/s72303-374.pdf

stock market crash and finds that firms renegotiated their executive compensations as early as late 1987 and early 1988.

### 3.3.2. Firm Characteristics Controls

We use data from the Center for Research in Security Prices (CRSP) and Compustat to compute all firm-year characteristic measures. *SIZE* is the natural logarithm of total assets. *LEV* is total debt scaled by total assets. *MB* is the market-to-book ratio of firm assets. *LTASSET* is long-term asset, defined as property, plant, and equipment (PPE) plus goodwill, scaled by non-cash total assets. *R&D* is research and development expenditure divided by the book value of total assets. *SPREAD* is the average difference in a stock's daily bid and ask prices scaled by the mid-quote price in a year. *RET* is annual stock return. *VOLATILITY* is annualized stock return volatility calculated with daily stock returns during the year. *S.D. CF* is the standard deviation of the ratio of cash flows, scaled by lagged total assets over the previous five years. *S.D. SALES* is the standard deviation of a firm's annual sales growth over the previous five years.

We report the mean, median, and standard deviation of these firm characteristics in Table 1.

## 3.4. Identification Strategy and Validity Checks

Our identification strategy is to use the random assignment of pilot firms to be relieved of short-sale price test constraints. To the extent that Regulation SHO pilot firms are randomly assigned, we have an ideal setting of pilot and control firms to study changes in short-sale constraints and CEO compensation during the Regulation SHO program years, relative to the pre-Regulation SHO years. In addition, the ending time of the Regulation SHO program allows us to observe if the effects we document during the program years revert during the post-Regulation SHO years. Specifically, we examine changes in *CPD* differences between the pilot and control firms both during and after the Regulation SHO program.

In addition to random assignment, the validity of a DiD analysis also relies on a parallel trend assumption; that is, in the absence of any treatment, the pilot and control groups should exhibit parallel trends. In Table 2, we check the random assignment and parallel trends assumptions by comparing the level and growth rates of firm characteristics between our pilot and control firms before Regulation SHO. While a number of studies verify the validity of a DiD analysis using Regulation SHO (Grullon, Michenaud, and Weston, 2015; Fang, Huang, and Karpoff, 2016), it is important to confirm this validity with our sample. Specifically, for our key variables of interest, we report their average levels in 2003 and average growth rates from 2001 to 2003 for the pilot and control firms, respectively. We report our results when we use the *Full sample* in Panel A and those when we use the *Balanced sample* in Panel B. Overall, the pilot and control firms exhibit similar average levels and growth rates before Regulation SHO across all firm characteristics that we consider. The two-sample difference tests are small in magnitude and statistically insignificant. Overall, the pre-event tests support the validity of a DiD analysis.

# 4. CEO Compensation Duration, Short-Sale Constraints, and Investor Disagreement

## 4.1. Regulation SHO's Impact on Short-Selling Activity and Investor Disagreement

Before we explore CEO compensation duration, we first examine assumptions behind our empirical setting and BSX. First, we should observe a relatively larger increase in average short-selling activities during the Regulation SHO program for the pilot firms, compared to those of the control firms. This evidence will imply that short-sale constraints are indeed binding for the pilot firms and speaks to the validity of our identification strategy. Second, investor disagreement should decrease more for the pilot firms relative to that of the control firms during the Regulation SHO program. One crucial theoretical premise behind BSX and Scheinkman and Xiong (2003) is that short-selling prevents short-termist firms from increasing investor disagreement to manipulate stock prices.[20] Finally, the previous two assumptions also imply that short-selling activities should increase more during the Regulation SHO program for firms with pre-existing high investor disagreement.

We test these assumptions and report our results in Table 3. Short-selling activity is measured by *SHORT RATIO*, defined as monthly short interest divided by the number of shares outstanding, in percentages. We use two investor disagreement measures: *DISPERSION* is the standard deviation in monthly I/B/E/S analysts' EPS forecasts, scaled by the mean forecast (Diether, Malloy, and Scherbina, 2002), and *TURNOVER* is abnormal dollar trading value scaled by the previous one-year average (Chen, Hong, and Stein, 2001; Barber and Odean, 2008).

In Panel A, we show the average short-selling activity before and during the Regulation SHO program for the pilot and control firms, respectively. While both pilot and control firms' short-selling activities increase during Regulation SHO, pilot firms see a larger increase. The last two columns show the average difference-in-difference estimates and t-statistics. As expected, pilot firms have a relatively larger increase in *SHORT RATIO* by 0.21% with a t-statistic of 2.5. The economic magnitude is equivalent to 6.5% (12.38%) of the sample mean (median) of *SHORT RATIO*. Panel B suggests that pilot firms, compared to the

[20]Following Hong and Stein (2007), there are two potential mechanisms through which short selling can affect investor disagreement. First, recent literature shows that investors' information processing power is limited, as they consistently ignore less salient signals (for a review, see Hirshleifer (2015)). Consistent with this view, empirical studies find that media affects trading (e.g., Engelberg and Parsons (2011), Dougal et al. (2012)), and, in particular, has a penchant for negative news (Niessner and So, 2018). Accordingly, optimistic investors with limited attention will be more aware of negative information and revise their views to the extent that elevated short selling is reported by the media. Recent studies have found support for this claim (Fox, Glosten, and Tetlock, 2009; Bushman and Pinto, 2019). Essentially, this mechanism implies that short selling decreases investor disagreement. Second, limited attention can be viewed as a special case of gradual information flow. In Hong and Stein (1999), investor segmentation (e.g., limited attention) or information dissemination constraints lead to each investor group observing only pieces of value-relevant information in each period. Investor disagreement arises naturally in this market structure. In this context, short sellers can decrease investor disagreement by inducing more negative media coverage as argued above. Short sellers can also promote the diffusion of negative news through trading (Diamond and Verrecchia, 1987; Duffie, Gârleanu, and Pedersen, 2002). As negative information is incorporated more quickly, investor disagreement will decrease.

control firms, also have larger decreases in investor disagreement measures during the Regulation SHO program. The DiD estimate using *DISPERSION* is -0.015 with a t-statistic of -2.22, equivalent to 6.32% (15.02%) of the sample mean (median) of *DISPERSION*. Using *TURNOVER* yields a DiD estimate equivalent to 6.16% (7.86%) of the sample mean (median). These economic magnitudes are comparable to the 8% increase in CEO compensation duration that we document in our later tests (Table 4). Note that we expect the total economic significance of short-sale constraints to be larger because Regulation SHO only removes one aspect of short-sale constraints: short-selling price tests. Additional short-sale constraints, such as explicit borrowing costs and institutional restrictions, prevent individual and institutional investors from shorting stocks.

In Panel C, we report short-selling activities when all firms are first partitioned by investor disagreement in the pre-Regulation SHO years. Specifically, we compute the average investor disagreement in the *PRE* period for all CRSP stocks and partition our sample into those that fall into the bottom 30%, middle 40%, and top 30% investor disagreement groups. We report results for the bottom and top groups only for brevity. Our results show that short-selling activities increase more for firms in the high disagreement partition. For example, *SHORT RATIO* increases by 1.69% more for high *TURNOVER* firms than low *TURNOVER* firms.

While BSX, and the investor disagreement literature in general, focus on investor disagreement and excessive trading as two important features of market speculation, we take a further step and explore other dimensions of market speculation and market quality. In Appendix B, we discuss and show Regulation SHO's effect on return volatility, effective spreads, and fraction of zero-return days. Consistent with our results in Table 3, we find pilot firms have lower values in all of the above measures compared to those of control firms during the program.

## 4.2. Short-Sale Constraints and CEO Compensation Duration

We first investigate the effect of removing short-sale constraints on CEO compensation duration. It is important to first note that both short-sale constraints and investor disagreement are necessary conditions in BSX for existing shareholders to engage in corporate short-termism. In other words, the level of *aggregate* investor disagreement during the Regulation SHO program should be reasonably high for a change in short-sale constraints to impact average CEO compensation incentives.

In Figure 1, we plot the monthly average stock-level disagreement (in red) from 1990 to 2011 for all CRSP common stocks, except for penny shares and microcaps. Specifically, we measure stock-level disagreement by the standard deviation of analysts' long-term EPS growth forecasts. In Figure 1, we show that investor disagreement is at a relatively elevated level around the time of Regulation SHO (in grey), compared to the 1990s. Thus, this evidence provides a validity check for testing BSX with our empirical setting.

We begin our baseline DiD analysis by running the following OLS regression on the firm-year level during the nine years of our sample period:

$$\begin{aligned} Log(CPD)_{i,t} = &\beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \\ &\beta_3 PILOT_i + \mathbf{X}_{i,t}\boldsymbol{\beta} + \delta_t + \lambda_i + \epsilon_{i,t}, \end{aligned} \tag{2}$$

where $\delta_t$ are year fixed effects, $\lambda_i$ are industry fixed effects, and $\mathbf{X}_{i,t}$ are firm characteristics control variables.[21,22] We winsorize by dropping *CPD* in the top 1 percentile of each year to avoid outliers. All other variables are defined in Section 3 and Appendix A. The key coefficients of interest are $\beta_1$ and $\beta_2$. If shareholders give short-term incentives to their

[21] The importance of industry affiliation in compensation has been emphasized in the literature (e.g., Krueger and Summers (1988), Baker, Jensen, and Murphy (1988)). Following this literature, we define industry fixed effects by using SIC three-digit codes.

[22] A Hausman test to determine the necessity to include firm fixed effects yields a p-value of 0.28, suggesting that unobservable time-invariant firm characteristics are not correlated with Regulation SHO. This test suggests that the gain of adopting a fixed effect model may not outweigh the cost of power and efficiency. In unreported tests (available upon request), we use a random effects model and obtain results consistent with our baseline regressions.

CEOs in a speculative market as in BSX, then we expect to observe a longer *CPD* if a firm is designated as a pilot firm, relative to firms in the control group and in the pre-Regulation SHO years. Therefore, we expect to find a positive $\beta_1$. After the pilot program expires, we expect this difference-in-difference to revert, resulting in a smaller and statistically weaker $\beta_2$. In addition, we expect to find insignificant estimates of $\beta_3$, to the extent that the Regulation SHO pilot firms are randomly assigned.

We report our baseline OLS estimates in Table 4. We use the *Full sample* in columns (1)–(3). In column (1) without any controls, we find that $\beta_1$ is a statistically significant 8.4%. The economic magnitude is equivalent to 15% of a standard deviation in *CPD*. Next, $\beta_2$ is statistically insignificant and considerably smaller in magnitude than $\beta_1$, which implies that the difference in *CPD* between the pilot and control firms in the *POST* period is similar to that in the *PRE* period. Finally, $\beta_3$ is statistically and economically insignificant, suggesting that pre-Regulation SHO differences in *CPD* between pilot and control firms are insignificant.

In columns (2) and (3), we follow Gopalan et al. (2014) and control for factors known to correlate with compensation duration: basic firm characteristics (*SIZE*, *MB*, *LEV*, *RET*, *SPREAD*), project duration (*LTASSET*, *R&D*), and firm risk (*VOLATILITY*, *S.D. CF*, *S.D. SALES*). There is only a minimal effect on the coefficients of interest when we control for these firm-level variables. The estimates of $\beta_1$ are both above 8% with t-statistics over 2.3. Consistent with column (1), we also find statistically insignificant estimates of $\beta_2$ and $\beta_3$. In addition, the signs of the coefficients in front of the firm characteristic variables are consistent with the prior literature. CEO compensation duration is positively correlated with firm size, market-to-book, and R&D expenditures, but negatively correlated with leverage and bid-ask spread (Gopalan et al., 2014). As we would expect, firms grant compensation packages with longer duration when they have longer-term assets, greater future growth opportunities, and more R&D projects. In columns (4)–(6), we repeat these tests with the *Balanced sample* and find consistent results. The magnitudes of the $\beta_1$ estimates are around

10% with statistical significance at the 5% level. As expected, estimates of $\beta_2$ and $\beta_3$ are all statistically insignificant.[23]

To better illustrate these findings, we first compute the average difference in *CPD* between the pilot and control firms for each of the three-year periods (*PRE*, *DURING*, and *POST*) in our sample. We then compute the period-to-period changes of this difference and plot them in Figure 2. This figure shows that the change in *CPD* difference during the *PRE* period is minimal. In contrast, the change in *CPD* difference in the *DURING* period is visibly large at 8%, and then reverses in the *POST* period.

Our findings support BSX. Given reasonably high aggregate investor disagreement, removing short-sale constraints diminishes the speculative component in pilot firms' stock prices and thus induces shareholders to grant longer-term incentives to their CEOs. After Regulation SHO, which results in the SEC eliminating short-sale price tests for all firms, the differences between the pilot and control firms significantly decrease.

## 4.3. Alternative Explanations and Robustness Tests

### 4.3.1. Alternative Specifications

In this section, we discuss alternative explanations and continue to explore the robustness of our findings by including additional control variables. However, it is worth emphasizing that controlling for time-varying firm characteristics may in fact bias our regression estimates because the controls themselves may be affected by the empirical shock (Angrist and Pischke, 2009; Gormley and Matsa, 2011; Gormley, Matsa, and Milbourn, 2013). To the extent that the treatment and control groups are as good as randomly assigned, our preferred specification is without controls.

[23] We conduct two additional joint tests. First, pilot firms should have longer compensation duration than control firms in the *DURING* period. For this test, the p-values from the null hypothesis $\beta_1 + \beta_3 \leq 0$ range from 0.03 to 0.06 with the *Full sample* specifications. We interpret these results as consistent with expectation. The p-values using the *Balanced sample* are weaker and range from 0.17 to 0.24. Second, pilot and control firms should have similar compensation duration in the *POST* period. For this test, the p-values from the null hypothesis $\beta_2 + \beta_3 = 0$ range from 0.69 to 0.88 across all six specifications, consistent with our expectation.

An alternative interpretation for the effect of Regulation SHO on corporate policies may be that short sellers simply impose a market disciplining effect on managers. In other words, short selling provides a complementary mechanism to internal corporate governance in reining in managers. Accordingly, the usual negative incentives associated with short-term compensation contracts, such as managerial myopia, are now less of a concern. This line of reasoning is broadly consistent with Fang, Huang, and Karpoff (2016). While we do not dispute the potential disciplining effects on managers by short sellers, this argument biases against us from finding results in compensation duration. If short selling provides an additional safeguard against managerial myopia, we should find Regulation SHO leading to *shorter* rather than longer compensation duration, which we do find in this paper.

On the contrary, the disciplining effect from short selling can be muted following De Angelis, Grullon, and Michenaud (2017), in which they find that Regulation SHO pilot firms convexify their CEOs' compensation contracts (i.e., more option grants) to protect them from potential bear raids. We control for this effect by including *Log(Vega)* as an additional control variable.[24] For completeness, we also control for pay-performance sensitivity *Log(Delta)*, another important dimension in compensation structure (Coles, Daniel, and Naveen, 2006).

In addition, CEO turnover may be correlated with short-selling activities, and our results may be driven by newly appointed CEOs receiving longer compensation duration. Gibbons and Murphy (1992) hypothesize that CEOs in the final years of their tenure have weak career concerns and thus choose to focus on short-term incentives. More recently, Marnovic and Varas (2019) show that the horizon problem exists even with endogenous incentives. In an empirical study, Dechow and Sloan (1991) find that CEOs spend less on R&D in the final years of their tenure. Accordingly, we control for CEO turnover with an indicator variable

[24]In Appendix C, we decompose our *CPD* measure into *CPD Stock* and *CPD Option* (i.e., value-weighted vesting periods of stock and option grants), respectively. We show in Table A2 that the CEO compensation duration effect we document in Table 4 is primarily due to restricted stock grants instead of option grants. In other words, increasing convexity by granting more options does not contribute to longer overall compensation duration. The preference for using stocks for duration adjustment is most likely due to the adoption of FAS 123-R in 2005. Prior to this policy, firms were not required to expense at-the-money options in their financial statements, and the majority of firms granted employees such options to avoid changes. After FAS 123-R, firms reduced their use of option grants.

*CEO TURN* in our regressions.

We control for these additional variables and report the regression results with the *Full sample* in column (1) of Table 5. The DiD estimate of the compensation duration effect $\beta_1$ is 8.6% with a t-statistic of 2.31. Using the *Balanced sample* in column (4) yields similar results, with a $\beta_1$ estimate of 10% and a t-statistic of 2.09. In both specifications, the estimates of $\beta_2$ and $\beta_3$ are generally statistically insignificant.

Furthermore, an alternative approach for measuring *CPD* in equation 1 is to use grant-date values instead of fiscal year-end values. Using grant-date values has the advantage of measuring the intent of shareholders on the dates of different grants throughout the year. At the same time, using fiscal year-end values has the advantage of measuring the vesting periods of all grants at a specific point in time for a given year. One would expect both approaches to yield similar results. Indeed, we find in column (2) with the *Full sample* that the DiD estimate $\beta_1$ is a statistically significant 9.9%. The estimates of $\beta_2$ and $\beta_3$ are statististically insignificant. Using the *Balanced sample* in column (5) yields similar results, with the exception that the $\beta_3$ coefficient in column (5) is statistically significant. This peculiar finding suggests a difference in *CPD* between the pilot and control firms before the experiment. However, this effect is not observed with the *Full sample* or specifications without controls, echoing our discussion regarding the advantages of the full-sample approach and our preference for the no-control specifications.

Finally, we include group-year fixed effects in columns (3) and (6). This allows the pilot and control firms to have different year effects. We find that the DiD estimate of $\beta_1$ is positive and statistically significant in column (3). The statistical significance of $\beta_1$ is slightly weaker in column (6), but the economic magnitude is almost identical. Finally, we find the $\beta_2$ coefficient estimates to be insignificant in both columns.[25]

[25]Similar to Section 4.2, we conduct two additional joint tests. First, pilot firms should have longer compensation duration than control firms in the *DURING* period. For this test, the p-values from the null hypothesis $\beta_1 + \beta_3 \leq 0$ range from 0.02 to 0.11. We interpret these results as consistent with expectation. Second, pilot and control firms should have similar compensation duration in the *POST* period. For this test, the p-values from the null hypothesis $\beta_2 + \beta_3 = 0$ range from 0.68 to 0.81, consistent with our expectation.

### 4.3.2. Placebo Tests

We run two placebo tests to further ensure robustness. First, we randomly assign our sample firms into treatment and control groups using the number of firms in the respective groups as in Table 4. We then repeat the same DiD estimation and report the results using the *Full sample* in columns (1) and (2) of Table 6. The results with the *Balanced sample* are reported in columns (5) and (6). As expected, the estimates of $\beta_1$, $\beta_2$, and $\beta_3$ are all insignificantly different from zero in all four specifications.

For our second placebo test, we use the true pilot and control firms but also use pseudo-timings of the Regulation SHO program. Although the ideal setting would be a time span of nine years that completely avoids our sample period, this is not possible because our *CPD* measure only goes as far back as 1999. Accordingly, the second placebo test defines *PRE* as 1999 to 2001, *DURING* as 2003 to 2005, and *POST* as 2007 to 2009. We report our results with the *Full sample* in columns (3) and (4), and those with the *Balanced sample* in columns (7) and (8). Again, we find insignificant results for all coefficients of interest.[26]

## 4.4. Investor Disagreement, Short-Sale Constraints, and CEO Compensation Duration

Next, we explore the role of investor disagreement in conjunction with that of short-sale constraints with respect to CEO compensation incentives. Investor disagreement, along with short-sale constraints, is one of the key necessary conditions in BSX. All else equal, the marginal effect of removing short-sale constraints should be larger among firms with higher market speculation due to investor disagreement. In other words, we should expect the effect in Table 4 to be stronger for this type of firm. Therefore, we partition our sample firms by

[26] Similar to Section 4.2 and Section 4.3.1, we conduct two additional joint tests; that is, pilot firms should have longer compensation duration than control firms in the *DURING* period, and pilot and control firms should have similar compensation duration in the *POST* period. As expected from placebo tests, we do not find support for any of these hypotheses. These test results are available upon request and not reported to conserve space.

measures of pre-Regulation SHO investor disagreement relative to the market and re-run our DiD test.

Our first measure of investor disagreement is analyst forecast dispersion (*DISPERSION*). We follow Diether, Malloy, and Scherbina (2002) and use the dispersion of analysts' EPS forecasts to proxy for investor disagreement. Using this measure, Diether, Malloy, and Scherbina (2002) find that stocks with high analyst disagreement have subsequently low returns, which supports the hypothesis that prices reflect optimistic views in a market with short-sale constraints (Miller, 1977). To the extent that analysts' opinions reflect investors' views, this is a direct measure of disagreement. This measure is widely used in the disagreement literature; see, for example, Sadka and Scherbina (2007), Moeller, Schlingemann, and Stulz (2007), Berkman et al. (2009), Yu (2011), and Hong and Sraer (2016), among others. Following this line of literature, we compute the monthly standard deviation of analysts' EPS forecasts, scaled by the consensus forecast to allow for cross-sectional comparisons. For each firm, we average *DISPERSION* over the five years before the Regulation SHO program. In Figure 1, we plot the average stock-level analyst dispersion for the high (in green) and low (in blue) investor disagreement partitions. Our plots show that there is a wide cross-sectional variation in investor disagreement, and the high disagreement group indeed consists of firms with high levels of disagreement relative to the market.

We present our DiD regression results in Panel A of Table 7. The *Full sample* results are in columns (1) to (4), and the *Balanced sample* results are in columns (5) to (8). Focusing first on the low *DISPERSION* group in columns (1) and (2), we find that the DiD estimates for $\beta_1$ are both statistically and economically weak. For example, the $\beta_1$ coefficient in column (1) is 5.5% with a t-statistic of 1.55. For the high *DISPERSION* group in columns (3) and (4), the $\beta_1$ estimates are both statistically significant with economic magnitudes over 15%. Using the *Balanced sample* yields consistent results.

Next, we use an alternative measure of investor disagreement: abnormal turnover (*TURNOVER*). Investor disagreement is intimately related to trading in this line of literature. Kandel and Pearson (1995) and Harris and Raviv (1993) both model investors who engage in excessive trading using common information, but who nonetheless hold divergent opinions. More closely related to our context of speculative trading, Scheinkman and Xiong (2003) propose that overconfident investors in a market with short-sale constraints actively trade with each other, leading to bubbles. Hong and Stein (1999) assume slow information diffusion between investor groups and show that this mechanism also leads to disagreement and trading. Specifically, we follow Chen, Hong, and Stein (2001) and measure disagreement by abnormal turnover. Every month, we compute the monthly stock turnover of each firm, scaled by the average in the previous one year. We compute abnormal turnover for all common stocks in the CRSP universe and take averages over the five years before Regulation SHO.

We report the DiD regression results in Panel B of Table 7. With the *Full sample*, we find that the DiD estimates for $\beta_1$ among the high turnover firms are both statistically and economically significant. For example, the $\beta_1$ coefficient in column (3) is 14.2% with a t-statistic of 2.12. On the contrary, the low abnormal turnover counterpart specifications in columns (1) and (2) yield no results. Using the *Balanced sample* in columns (5)–(8) yields consistent results. Overall, we find supporting evidence that the effect on compensation duration from removing short-sale constraints is stronger for firms with high investor disagreement.[27]

To assess the statistical significance of the differences in the DiD estimates for $\beta_1$ between the high disagreement firms and low disagreement firms, we run Wald tests to assess the equality of $\beta_1$ between the two partitions. Using *DISPERSION* (Panel A) and the *Full sample*, we find weak statistical support with p-values of 17% (columns (1) vs. (3)) and 35% (columns (2) vs. (4)). However, we restore statistical significance with the *Balanced sample* with p-values of 1% (column (5) vs. (7)) and 11% (column (6) vs. (8)). Using *TURNOVER*

[27]While information asymmetry is not the emphasis in BSX, we discuss in Appendix D the role of information asymmetry, another factor that promotes market speculation. Using the information asymmetry measure by Pástor and Stambaugh (2003), we show that firms with pre-existing high levels of information asymmetry have stronger effects on CEO compensation duration from Regulation SHO. We present our results in Table A3.

(Panel B) and the *Full sample*, we find that the p-values are 3% (column (1) vs. (3)) and 5% (column (2) vs. (4)). The statistical significance of the same tests remains strong when we use the *Balanced sample*, with p-values of 0.2% (column (5) vs. (7)) and 1% (column (6) vs. (8)).

Alternatively, instead of using the level of investor disagreement in the pre-Regulation SHO years, we could partition our sample by the change of investor disagreement during Regulation SHO. Since short-sale constraints are more binding for firms with larger drops in investor disagreement, these firms will see a larger increase in CEO compensation duration during Regulation SHO, compared to those with lesser drops (or increases) in investor disagreement. Specifically, we partition the sample by the change in average investor disagreement from the *PRE* period to the *DURING* period and re-run the DiD regressions.[28]

We present our results in Table 8. In Panel A, we use *DISPERSION* as the investor disagreement measure. With the *Full sample*, the DiD estimates for $\beta_1$ from the low dispersion change group in columns (1) and (2) are both above 13% with strong statistical significance. The main coefficients of interest from the high dispersion change group are statistically insignificant. The results using the *Balanced sample* are similar in economic magnitude with slightly weaker statistical significance. In Panel B, we use *TURNOVER* as the investor disagreement measure. With the *Full sample*, the DiD estimates from the low turnover change group in columns (1) and (2) are both above 16%, while the high turnover change group in columns (3) and (4) does not have significant results. We find consistent results using the *Balanced sample* in columns (5) to (8).

Similar to Table 7, we also run Wald tests to assess the equality of $\beta_1$ between the two partitions. While the differences in economic magnitudes are apparent, the Wald tests are generally insignificant with p-values that range from 1% to 76%. We find the strongest

[28]The changes in investor disagreement in the low (high) group are generally negative (positive). Using analyst dispersion, the median change of investor disagreement in the *Full sample* within the low (high) group is -0.063 (0.029). In the *Balanced sample*, the medians are -0.050 and 0.026, respectively. With abnormal turnover, the medians are -0.133 and 0.184 in the *Full sample*, respectively. In the *Balanced sample*, the medians are -0.100 and 0.162, respectively.

statistical significance in Panel B with the *Balanced sample*, where the p-values are both at 1% for column (5) vs. (7), and column (6) vs. (8). We attribute the weaker statistical significance to dropping sample observations in the *POST* period for this test, resulting in much smaller sample sizes and thus weaker statistical power.

# 5. Mechanism and Consequences

## 5.1. Mechanism: Short-Term-Oriented Institutional Ownership

Presumably, the design of grants in CEO compensation packages are heavily influenced by the preferences of large institutional shareholders and blockholders (Hartzell and Starks, 2003; Edmans, Gabaix, and Jenter, 2017). Accordingly, we explore the role of short-term-oriented institutional shareholders in promoting corporate short-termism. Specifically, we expect the marginal benefit of removing short-sale constraints to be stronger among firms with more short-term-oriented institutional shareholders; that is, we should observe a stronger effect on CEO compensation duration for these firms.

We first classify institutional shareholders into short-term-oriented investors and long-term-oriented investors. Bushee (1998) measures institutional investors' portfolio turnover and defines three types of institutional investors: transient, quasi-indexers, and dedicated. Transient institutional investors are short-term-oriented, while the other two types are long-term-oriented. For each firm-year, we follow this classification and measure the influence of short-term-oriented shareholders by the relative ownership between short-term-oriented institutional investors and long-term-oriented institutional investors (*STIO/LTIO*). As we do with our sub-sample tests in Section 4, we partition our sample firms by *STIO/LTIO* before the Regulation SHO program and re-run the DiD regressions.

We report our results in Table 9. We find that the compensation duration effect is concentrated among firms with a pre-existing larger influence by short-term-oriented institutional investors. With the *Full sample*, the $\beta_1$ estimates in columns (3) and (4) for the high *STIO/LTIO* partition are 17.1% and 16.7% with strong statistical significance, respectively. On the contrary, firms with low pre-existing levels of short-term-oriented institutional investors have insignificant coefficient estimates for $\beta_1$ in columns (1) and (2), as expected. The p-values from Wald tests for the equality between columns (1) and (3), and between columns (2) and (4), are 2% and 1%, respectively. Using the *Balanced sample* in columns (5)–(8) yields similar results. The $\beta_1$ estimates from the high *STIO/LTIO* partition are 21.3% and 19.1% with strong statistical significance, respectively. Similarly, we do not find significant results in the low *STIO/LTIO* partition.The p-values from Wald tests for the equality between columns (5) and (7), and between columns (6) and (8), are 7% and 8%, respectively.

Overall, we find evidence of a mechanism for the empirical results we present in Section 4. The empirical results we show here suggest that one of the driving forces behind corporate short-termism is the preference of short-term-oriented institutional shareholders. Accordingly, removing short-sale constraints induces firms dominated by short-term-oriented institutional shareholders to lengthen their CEO compensation duration more than other comparable firms.

## 5.2. Consequences

If changes in compensation duration are effective, then we should observe changes in both corporate policies and CEO behavior. In this section, we consider three implications on corporate policies and CEO behavior explored in prior literature: overinvestment, earnings management, and CEO investment horizon. Revisiting these results in the literature is helpful in further alleviating concerns that unobservable biases in the sample selection process might drive our empirical findings.[29] To conserve space in the main text, we refer the readers to Appendix E for detailed variable definitions and empirical designs.

[29] We thank the editor for pointing out this aspect of our tests.

### 5.2.1. Overinvestment

One direct implication of BSX is that shareholders use short-termist compensation incentives to induce CEOs to devote more managerial efforts to "castle-in-the-air" projects. These projects have inferior long-term average value, but have the potential to be overvalued in a speculative market with overconfident investors disagreeing among themselves. Therefore, we should observe less inefficient investments when CEO compensation duration increases due to short selling. More broadly, this hypothesis is consistent with prior studies that find firms overinvest when stock prices are overvalued (see, among others, Morck, Shleifer, and Vishny, 1990; Gilchrist, Himmelberg, and Huberman, 2005; Polk and Sapienza, 2009).

We report the results in Table 10. In Panel A, we use the *Full sample* and report the 2SLS results in column (1). We find that longer compensation duration due to the removal of short-sale constraints indeed leads to less propensity to overinvest. The second-stage coefficient is -0.907 with a t-statistic of -2.66. The 2SLS result in column (1) of Panel B with the *Balanced sample* yields similar results.

We also report the reduced-form regression results in column (2) of both panels. The results are consistent with its two-stage counterparts; that is, removing short-sale constraints induces pilot firms' CEOs to reduce overinvestment more than the control firms during the Regulation SHO program. Overall, we find results in our sample consistent with prior literature with regard to overinvestment.

### 5.2.2. Earnings Management

Next, we expect that longer CEO compensation duration should lead to fewer earnings management activities. BSX suggest that earnings management will be driven by shareholders' incentives to exploit market speculation. Similarly, Peng and Röell (2008a) show that fast-vesting equity compensation creates incentives for managers to manipulate earnings.

Cutting R&D expenditure has been linked to earnings management. For example, Graham, Harvey, and Rajgopal (2005) survey financial executives and find that the majority would decrease discretionary spendings such as R&D to meet earnings targets. Bushee (1998) finds that this tendency to sacrifice R&D in favor of short-term earnings is strongest among firms with more short-term-oriented institutional shareholders. Accordingly, we expect CEOs with longer-term incentives in their compensation contracts to focus more on R&D investments.

The two-stage and reduced-form regression results with the *Full sample* are in columns (3) and (4) of Table 10 Panel A. In column (3), the second-stage coefficient shows that a longer compensation duration indeed leads to higher R&D intensity with marginal statistical significance. The reduced-form regression in column (4) is not statistically significant, though the signs of the coefficients are consistent with our expectation. The *Balanced sample* results in Panel B are not statistically significant.

One corporate behavior that has also been linked to earnings management is stock repurchases. While stock repurchase is a form of payout to shareholders, it can also be used to manage earnings (Hribar, Jenkins, and Johnson, 2006; Almeida, Fos, and Kronlund, 2016). While the rise of activist investors over the past several decades has been credited for corporate governance improvements, they are also impatient investors who push for quick performance improvements and payouts (Strine, 2010; Lazonick, 2014). In a speculative market as in BSX, managers incentivized by short-termist compensation will conduct more repurchases to manage earnings and boost stock prices (Ikenberry, Lakonishok, and Vermaelen, 1995). Closely related to our context, Edmans, Fang, and Huang (2018) find that short-term considerations in CEO compensation are linked to more stock repurchases. Accordingly, longer CEO compensation duration should lead to fewer stock repurchases for earnings management.

We report the two-stage and reduced-form regressions in columns (5) and (6) in Table 10. With the *Full sample* in Panel A, the second-stage coefficient implies that longer compensation duration leads to fewer accretive repurchases with marginal statistical significance. The reduced-form regression also implies that removing short-sale constraints leads to fewer accretive repurchases for the pilot firms compared to those of control firms. It is worth noting that if the Regulation SHO program leads to lower stock prices for the pilot firms, then firms in fact have *more* incentive to buy back cheap shares. Nevertheless, the evidence is more consistent with our prediction. The results with the *Balanced sample* in Panel B are similar. The second-stage coefficient is negative with similar economic magnitude, though with low statistical significance. The reduced-form results are similar with strong statistical significance.

Another measure of earnings management is the propensity to exactly meet analysts' forecasts, or zero earnings surprises (Malmendier and Tate, 2009). We report the two-stage and reduced-form regressions in columns (7) and (8) in Table 10. With the *Full sample* in Panel A, the second-stage results imply that longer compensation duration leads to less propensity of earnings management. In addition, the reduced-form regressions also suggest that removing short-sale constraints decreases a firm's earnings management activities. The *Balanced sample* in Panel B yields similar results.

Overall, we find results with regard to earnings management in our sample that are consistent with existing literature.

#### 5.2.3. CEO Investment Horizon

One potential consequence of granting short-termist incentives to CEOs is the effect on the trading horizons in their own respective company stocks. Longer CEO compensation duration incentivizes CEOs to take a longer view in their own trades. In addition, CEOs willing to accept longer vesting periods tend to have longer stock trading horizons. Therefore, we expect that longer compensation duration due to the removal of short-sale constraints will lead to longer CEO trading horizons.

The two-stage and reduced-form regression results with the *Full sample* are in columns (9) and (10) of Table 10 Panel A. Here, column (9) shows that longer compensation duration leads to longer CEO trading horizons. The coefficient is -0.129 with a t-statistic of -2.48.

We also report the reduced-form regression results in column (10). This inference is consistent with its two-stage counterpart. Using the *Balanced sample* in Panel B does not yield statistically significant results. This is most likely a result of weaker power due to limited sample size.

# 6. Conclusion

In this paper, we exploit a randomized experiment to understand corporate short-termism. We design our tests around the executive compensation model of BSX. In this model, the major deviations from the fully efficient contracting environment are the existence of short-sale constraints in the stock market and of investors with divergent opinions regarding firm value. As such, existing shareholders design optimal contracts that encourage short-term stock performance, so they may hopefully sell their shares to more optimistic investors. In contrast to prior studies, which focus on conflicts of interests between current shareholders and managers, the conflict in BSX and our study is between current shareholders and future shareholders.

Our identification strategy is the implementation of Regulation SHO by the SEC that removes short-sale constraints for a randomly selected group of pilot stocks. Using a difference-in-difference approach, we show that the Regulation SHO pilot firms increase their CEO compensation duration more than that of control firms during the program, and this difference is not significant after the program. Consistent with BSX, we show that this effect is stronger among firms with higher investor disagreement, which fosters market speculation. We identify a plausible mechanism that short-term-oriented institutional investors are the driving force behind our findings. In addition, we provide additional supporting evidence that pilot firms engage in less inefficient investments and earnings management, and that their CEOs have longer trading horizons. Overall, our findings not only support the speculative trading motivation in BSX as one important cause for corporate short-termism, but also offer potential policy implications. The solution to the problem of short-termism may lie in more efficient and less speculative stock prices.

# Appendix A Additional Variable Definitions

**TURNOVER** Monthly dollar volume scaled by the previous 12-month average.

**DISPERSION** Monthly standard deviation of I/B/E/S analysts' EPS forecasts, scaled by the consensus analyst forecast.

**CEO TURN** Indicator variable equals one if a firm has a change of CEO, and zero otherwise.

**Log(DELTA)** Log of the change in dollar value of a CEO's wealth for a 1% change in stock price.

**Log(VEGA)** Log of the change in dollar value of a CEO's wealth for a 1% change in stock return volatility.

**STIO/LTIO** The ratio of short-term-oriented institutional ownership to long-term-oriented institutional ownership. Institutional shareholders are classified following Bushee (1998). Transient institutional investors are short-term-oriented. Quasi-indexers and dedicated investors are long-term-oriented.

**HOR** Average absolute annual net order flow multiplied by -1 in the current year (Akbas, Jiang, and Koch, 2018). Annual net order flow is defined as the difference between the number of purchases and sales, scaled by the total number of trades.

**OVERINVEST** Indicator variable that equals one if a firm's investment is above the industry-year median, where investment is defined as capital expenditure scaled by lagged PPE, and zero otherwise (Polk and Sapienza, 2009).

**ARP** Dollar amount of accretive repurchases, scaled by lagged market capitalization. Accretive repurchases are defined by the method in Hribar, Jenkins, and Johnson (2006).

**EM** Indicator variable that equals one if a firm's EPS from the current fiscal year is the same as the median of all analysts' last forecasts, and zero otherwise.

# Appendix B Market Speculation and Quality

In this section, we first explore another characteristic of speculative markets around Regulation SHO: return volatility. It has been widely documented that asset overvaluation accompanies high volatility (Cochrane, 2002; Ofek and Richardson, 2003; Hong and Sraer, 2013). In an investor disagreement framework similar to BSX, Scheinkman and Xiong (2003) imply that short sales lead to a convergence of disagreement and lower volatility. With more short selling and less investor disagreement, we expect the Regulation SHO pilot firms to exhibit lower return volatility. In addition, we explore if additional short selling leads to better market quality as measured by liquidity. Short sellers are contrarians; that is, they sell more (less) when prices are high (low). Therefore, short sellers' trading potentially provides liquidity and stability to market prices (Boehmer and Wu, 2013). Accordingly, we expect Regulation SHO pilot firms' liquidity to improve relative to that of control firms.

Return volatility (*VOLATILITY*) is defined as the annualized standard deviation of daily stock returns. We use two liquidity measures: effective spread (*EFF. TICK*) by Goyenko, Holden, and Trzcinka (2009), and fraction of zero-return days (*RET FRACTION*) by Lesmond, Ogden, and Trzcinka (1999). Goyenko, Holden, and Trzcinka (2009) find that among the liquidity measured based on daily data, *EFF. TICK* and *RET FRACTION* perform the best in measuring liquidity. Specifically, *EFF. TICK* is the probability-weighted average effective spread deflated by price, based on daily CRSP data and end-of-day price clustering. *RET FRACTION* is the fraction of trading days with zero returns and non-zero volume in a year.

We report our test results in Table A1. Consistent with our expectations, we find that the difference-in-difference estimates of these three measures are all negative; that is, Regulation SHO pilot firms have lower volatility, effective spreads, and fraction of zero-return days during the experiment, relative to control firms. These results are all statistically significant at the 5% level.

# Appendix C Stocks vs. Options

In Section 4, we find that CEO compensation duration increases when short-sale constraints are removed from Regulation SHO. To determine if our findings are primarily due to option grants, we decompose our CEO compensation duration measure into stock duration (*CPD Stock*) and option duration (*CPD Option*). Specifically, for each firm-year in our sample, we compute the value-weighted duration of stock grants and option grants, respectively. We take the logarithms of these two duration measures and use them as our dependent variables in our baseline DiD framework.

We report our regression estimates in Table A2, where columns (1)-(4) use the *Full sample* and columns (5)-(8) use the *Balanced sample*. In columns (1) and (2) with the *Full sample*, we use stock compensation duration *Log(CPD Stock)* as the dependent variable. The coefficient estimates of $\beta_1$ are both above 12%, larger than our pooled estimates in Table 4. These estimates imply that when short-sale constraints are removed, firms increase their CEO compensation duration by offering stocks with longer vesting periods. The economic magnitudes in columns (5) and (6) with the *Balanced sample* are stronger at 14%, though with weaker statistical significance. Similar to our baseline results in Table 4, we do not find statistical significant estimates of $\beta_2$ and $\beta_3$.

On the contrary, we do not find significant estimates of $\beta_1$ in columns (3), (4), (7), and (8), in which we use option compensation duration *Log(CPD Option)* as the dependent variable. This implies that there is no difference in option compensation duration between the Regulation SHO pilot and control firms. Likewise, we do not find statistically significant estimates of $\beta_2$ and $\beta_3$. The p-values from Wald tests for the equality between columns (1) and (3), and between columns (2) and (4), are 6% and 3%, respectively. Using the *Balanced sample*, the p-values are 17% between columns (5) and (7), and 10% between columns (6) and (8). Overall, Table A2 suggests that our empirical findings in the main text are primarily driven by stock grants with longer vesting periods and not by option grants.

# Appendix D Information Asymmetry

While not the emphasis of BSX, information asymmetry also promotes market speculation and thus could play a role in the relationship between short-sale constraints and compensation duration. The smaller the information gap that exists between investors, the harder it becomes for asset bubbles to persist (Allen, Morris, and Postlewaite, 1993; Abreu and Brunnermeier, 2003). Therefore, we expect to observe the effect of removing short-sale constraints on *CPD* to be more pronounced among firms with high information asymmetry.

We measure information asymmetry by Pástor and Stambaugh's (2003) liquidity measure ($\gamma$). We follow their methodology and run the following regression for stock $i$ in month $t$:

$$R^e_{i,d+1,t} = \alpha_{i,t} + \beta_{i,t}R_{i,d,t} + \gamma_{i,t}\text{sign}(R^e_{i,d,t}) \times v_{i,d,t} + \epsilon_{i,d+1,t}, \tag{3}$$

where $d$ indexes each trading day in month $t$, $R_{i,d,t}$ is stock $i$'s daily return on day $d$, $R^e_{i,d,t}$ is the market-adjusted daily return, and $v_{i,d,t}$ is the dollar volume. If a firm has severe information asymmetry (or high illiquidity), then one expects to observe a large contemporaneous price impact on the day with large dollar volume, as well as observe strong price reversal on the next trading day. The estimate of $\gamma_{i,t}$ in this case is expected to be a negative number with a large absolute magnitude. Firms with low information asymmetry would generally yield a $\gamma$ estimate with a small absolute value.

As we do in Section 4, we first partition firms into high and low information asymmetry groups according to the CRSP universe median and rerun our DiD regressions. We present our results in Table A3. These results are consistent with our expectation. With the *Full sample*, the DiD estimates for $\beta_1$ in columns (3) and (4) are positive and statistically significant for firms with low $\gamma$ (high information asymmetry). On the contrary, the high $\gamma$ (low information asymmetry) firms exhibit no significant effects. For example, column (3) shows that firms with low $\gamma$ yield a $\beta_1$ estimate of 12.2% with a t-statistic of 2.41. The post-event DiD estimate, $\beta_2$, is insignificant, consistent with our prior results. In column

(1), the $\beta_1$ estimate is both economically and statistically insignificant for firms with high $\gamma$ (low information asymmetry). The results using the *Balanced sample* are consistent with stronger economic and statistical significance.

We also run Wald tests to assess the equality of $\beta_1$ between the two information asymmetry partitions. The p-values from Wald tests with the *Balanced sample* are 6% (column (5) vs. (7)) and 16% (column (6) vs. (8)). The Wald tests are insignificant with the *Full sample*.

# Appendix E Consequences

In this appendix we provide details of empirical design and variable construction relating to Section 5.2. To investigate Regulation SHO's impact, through its effect on executive compensation duration, on corporate policies and CEO behavior, we use two-stage least squares (2SLS). Specifically, the first-stage regression is the baseline regression as in Table 4, and the second-stage regression is the outcome variable of interest on the fitted values of compensation duration. We report our second-stage results in Table 10 for each outcome variable. In particular, columns (1), (3), (5), (7), and (9) are the second-stage results for each outcome variable, respectively. We report results with the *Full sample* in Panel A, and those with the *Balanced sample* in Panel B.

We also report the reduced-form regression results where we run DiD regressions with each outcome variable of interest as the dependent variable. In particular, we report the reduced-form regression results in columns (2), (4), (6), (8), and (10) for each outcome variable, respectively. All regressions in Table 10 contain the full set of control variables as in Table 4 and are omitted for brevity.

In Section 5.2, we explore three aspects of corporate policies and CEO behavior that the prior literature has shown to be affected by compensation designs: overinvestment, earnings management, and CEO investment horizon. In the rest of this appendix, we provide details of variable construction with regard to these outcome variables of interest.

## E.1 Overinvestment

We measure firm overinvestment following Polk and Sapienza (2009). Firm investment is defined as the sum of capital expenditure, research and development expense, and advertising expense, scaled by lagged property, plant, and equipment (PPE). We define an indicator variable *OVERINVEST* that equals one if a firm's annual investment level is above the industry-year median, and zero otherwise. Industry classifications are based on Fama-French

48 industries.

## E.2 Earnings Management

Our first measure of earnings management is R&D intensity. We define R&D intensity by R&D expenditure scaled by lagged PPE.

Our second measure of earnings management is stock repurchasing activities. We first follow Hribar, Jenkins, and Johnson (2006) to determine the amount of accretive repurchases (i.e., repurchases that increase a firm's EPS). Specifically, we first compute the "as-if" EPS without repurchases following equation 3 in Hribar, Jenkins, and Johnson (2006).[30] Repurchases in a quarter are accretive if actual EPS exceeds "as-if" EPS by more than one cent. The dollar amount of quarterly repurchases are scaled by previous quarter-end market capitalization and then averaged across four quarters to obtain an annual measure of accretive repurchase ($ARP$).

Our third measure of earnings management is the propensity to exactly meet analysts' forecasts, or zero earnings surprises (Malmendier and Tate, 2009). We define an indicator variable $EM$ that equals one if a firm's EPS is identical to the median of analysts' forecasts, and zero otherwise.

## E.3 CEO Investment Horizon

To measure CEOs' trading horizons in their own company stocks, we use the CEO trading horizon measure by Akbas, Jiang, and Koch (2018).[31,32] Intuitively, if CEOs engage in mostly one-sided trades (e.g., all buys, all sales), then they tend to have longer trading horizons. When CEOs have long-term goals, their trading patterns are typically persistent. For example, persistent selling most likely reflects ongoing diversification or liquidity needs

[30] "As-if" EPS is defined as $NI_t/(\textit{Shares Outstanding}_{t-1} + 0.5 \times \textit{Shares Issued}_t)$, where $NI_t$ is reported quarterly earnings.

[31] We thank Chao Jiang for sharing the trading horizon data.

[32] While Akbas, Jiang, and Koch (2018) examine trades by all company insiders, we focus on trades made only by CEOs.

from accumulated equity grants. In contrast, CEOs that trade frequently on both sides are more likely trading for short-term profits.

We measure a CEO's investment horizon in a given year with absolute annual net order flow. Annual net order flow is defined as the difference between the number of purchases and sales in a year, scaled by the total number of trades. Accordingly, absolute net order flow ranges between 0 and 1, and a CEO that only buys or only sells will have a value of 1. We follow Akbas, Jiang, and Koch (2018) and define a CEO's trading horizon, *HOR*, as the average absolute annual net order flow multiplied by -1. In other words, a CEO with the longest (shortest) trading horizon will have *HOR* equal to -1 (0).

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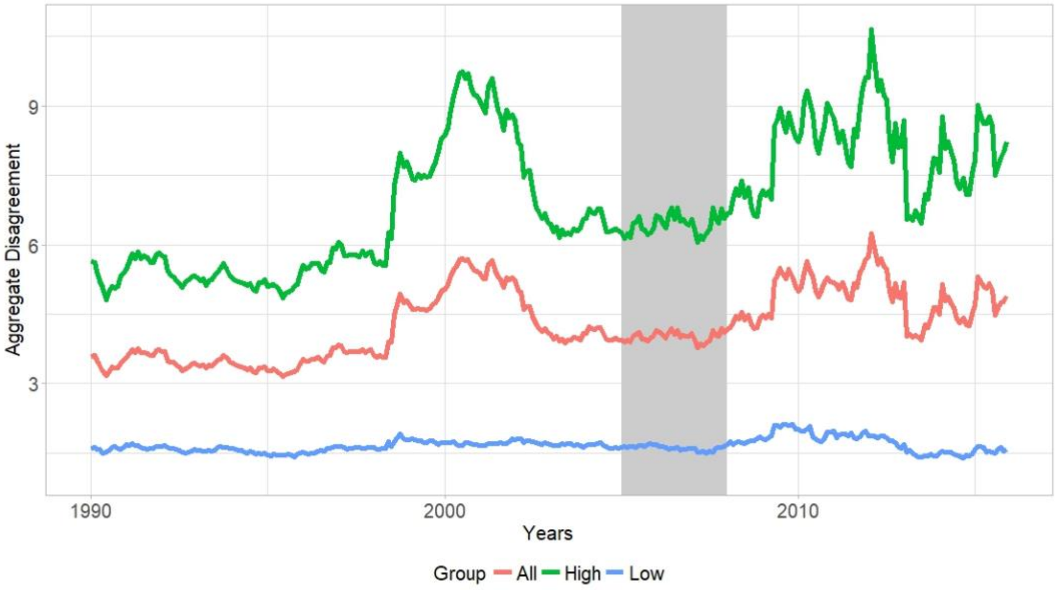


Figure 1
Aggregate Disagreement

We plot the equal-weighted monthly average stock-level disagreement from 1990 to 2011. The sample includes all CRSP common stocks, excluding penny stocks (price less than five dollars) and the bottom two deciles in the monthly NYSE market capitalization deciles. Stock-level disagreement is measured by the standard deviation of long-term EPS growth forecasts by analysts. The graph plots the average disagreement of stocks for the entire sample (in red), the high disagreement sample (in green), and the low disagreement sample (in blue). The partition of High and Low disagreement groups is determined by the monthly median in stock-level disagreement. Regulation SHO program years (2005-2007) are marked in grey.

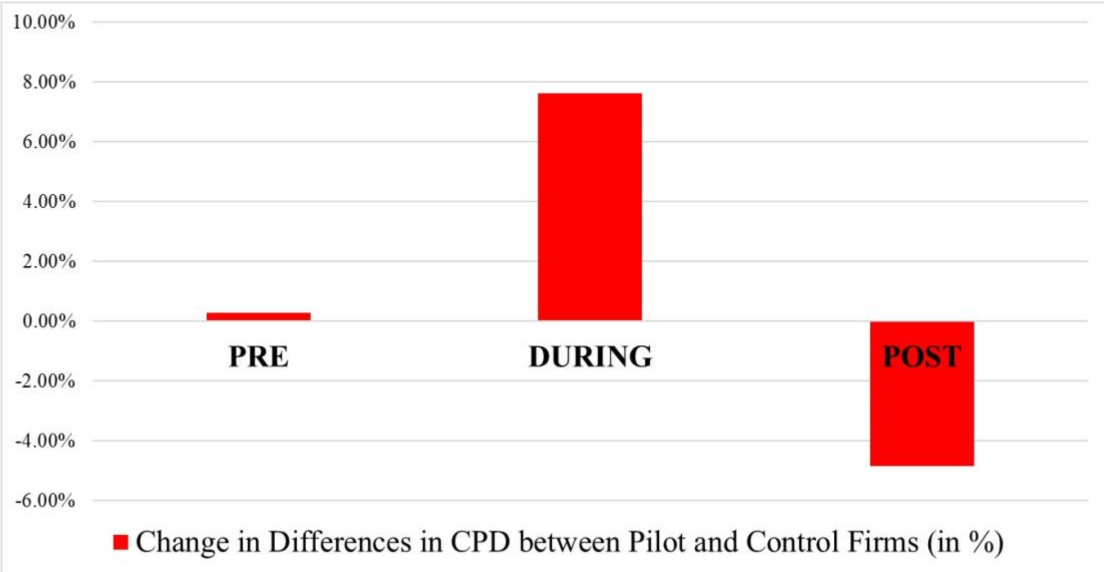


Figure 2

Change in CEO Compensation Duration: Pilot versus Control Groups

We plot the evolution of the difference in CEO compensation duration (*CPD*) between the Regulation SHO experiment pilot and control firms. We first compute the average difference in *CPD* between the pilot and control firms during four three-year periods: 1998 to 2000, *PRE* (2001-2003), *DURING* (2005-2007), and *POST* (2009-2011). We graph the period-to-period changes in these differences for the *PRE*, *DURING*, and *POST* periods.

## Table 1: Summary Statistics

This table presents summary statistics of the firm characteristics for our sample. *CPD* is CEO compensation duration (in months) defined in the text and Gopalan et al. (2014). *SIZE* is the natural logarithm of total assets. *LEV* is total debt scaled by total assets. *MB* is the market-to-book ratio. *LTASSET* is property, plant, and equipment plus goodwill, scaled by non-cash total assets. *R&D* is research and development expenditure divided by the book value of total assets. *ROA* is operating income before depreciation and amortization divided by the beginning of period total assets. *SPREAD* is the average daily stock bid-ask spread, scaled by the bid-ask midpoint, in a year. *VOLATILITY* is annualized stock return volatility calculated with daily stock returns during the year. *S.D. CF* is the standard deviation of the ratio of cash flows over lagged total assets over the previous five years. *S.D. SALES* is the standard deviation of the firm's annual sales growth over the previous five years. *VEGA* ($000s) is the dollar change in a CEO's wealth for a 0.01 change in standard deviation of returns. *DELTA* ($000s) is the dollar change in a CEO's wealth for a 1% change in stock price. *CEO TURN* is an indicator variable that equals one if there is a change in CEO from the previous year. *RET* is the annual stock return. The sample period consists of 2001-2003 (pre-pilot period), 2005-2007 (during-pilot period), and 2009-2011 (post-pilot period).

| | Mean | Stdev | Min | 25% | Median | 75% | Max |
|---|---|---|---|---|---|---|---|
| *CPD* | 20.14 | 11.32 | 0.00 | 13.23 | 21.18 | 26.81 | 82.01 |
| *SIZE* | 8.28 | 1.45 | 2.85 | 7.32 | 8.14 | 9.22 | 13.93 |
| *LEV* | 0.29 | 0.49 | 0.00 | 0.11 | 0.25 | 0.38 | 23.53 |
| *MB* | 1.92 | 1.81 | 0.02 | 0.91 | 1.41 | 2.30 | 32.94 |
| *LTASSET* | 0.73 | 0.37 | 0.00 | 0.48 | 0.71 | 0.98 | 2.81 |
| *R&D* | 0.03 | 0.07 | 0.00 | 0.00 | 0.00 | 0.04 | 1.26 |
| *ROA* | 0.15 | 0.12 | -1.14 | 0.09 | 0.14 | 0.21 | 1.66 |
| *SPREAD* | 0.33 | 0.52 | -0.05 | 0.07 | 0.12 | 0.34 | 8.09 |
| *VOLATILITY* | 2.51 | 1.40 | 0.38 | 1.62 | 2.16 | 2.99 | 24.55 |
| *S.D. CF* | 0.05 | 0.09 | 0.00 | 0.02 | 0.03 | 0.06 | 4.11 |
| *S.D. SALES* | 0.26 | 0.97 | 0.00 | 0.07 | 0.14 | 0.25 | 41.56 |
| *VEGA* | 172.1 | 241.3 | 0.00 | 43.0 | 124.9 | 198.5 | 3,286 |
| *DELTA* | 1,233 | 8,029 | 0.00 | 176.0 | 487.7 | 1,163 | 388,908 |
| *CEO TURN* | 0.17 | 0.38 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| *RET* | 0.15 | 0.73 | -0.98 | -0.19 | 0.07 | 0.33 | 17.74 |

Table 2: Pre-Event Firm Characteristics

This table reports the average firm characteristics for the year before Regulation SHO announcement (2003) and growth rates in the pre-Regulation SHO years (2001-2003). The treatment group consists of firms designated as pilot stocks during the Regulation SHO program, and the remaining firms are classified into the control group. Panel A uses the full sample, and Panel B uses a balanced sample in which are required to be in all three years in the *PRE* period, and the first year in both the *DURING* and *POST* periods. All variables are defined in Section 3.3 and Appendix A.

Panel A: Full Sample

| | Pilot Group | | Control Group | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Level (03') | Growth Rate (01'-03') | Level (03') | Growth Rate (01'-03') | Diff | T-stat | Diff | T-stat |
| | (1) | (2) | (3) | (4) | (1)-(3) | | (2)-(4) | |
| *CPD* | 17.73 | 0.17 | 17.61 | 0.18 | 0.13 | 0.14 | -0.01 | -0.05 |
| *SIZE* | 8.17 | 0.02 | 8.15 | 0.02 | 0.02 | 0.22 | 0.00 | 0.18 |
| *LEV* | 0.29 | 4.02 | 0.38 | 1.72 | -0.08 | -0.90 | 2.30 | 0.85 |
| *MB* | 2.15 | 0.09 | 1.90 | 0.06 | 0.25 | 1.40 | 0.03 | 0.44 |
| *LTASSET* | 0.76 | 0.40 | 0.75 | 0.36 | 0.01 | 0.42 | 0.04 | 0.32 |
| *R&D* | 0.03 | 0.03 | 0.03 | 0.00 | 0.00 | 0.29 | 0.02 | 0.32 |
| *ROA* | 0.15 | -0.04 | 0.15 | -0.98 | 0.01 | 1.09 | 0.93 | 0.79 |
| *SPREAD* | 0.40 | -0.55 | 0.39 | -0.56 | 0.01 | 0.32 | 0.01 | 0.38 |
| *VOLATILITY* | 2.35 | -0.23 | 2.38 | -0.25 | -0.03 | -0.37 | 0.02 | 0.93 |
| *S.D. CF* | 0.06 | 0.35 | 0.06 | 0.44 | -0.01 | -1.05 | -0.09 | -0.68 |
| *S.D. SALES* | 0.30 | 0.48 | 0.27 | 0.38 | 0.03 | 0.70 | 0.11 | 1.07 |
| *VEGA* | 203.7 | 2461 | 196.1 | 32.4 | 7.62 | 0.38 | 2,429 | 1.35 |
| *DELTA* | 1051 | 0.68 | 905.6 | 0.50 | 145.8 | 0.83 | 0.18 | 0.71 |
| *CEO TURN* | 0.16 | -0.79 | 0.16 | -0.79 | 0.01 | 0.18 | 0.00 | 0.00 |
| *RET* | -0.05 | -3.47 | -0.04 | -1.03 | -0.01 | -0.16 | -2.43 | -0.85 |

| Panel B: Balanced Sample | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Pilot Group | | Control Group | | | | | |
| | Level (03') | Growth Rate (01'-03') | Level (03') | Growth Rate (01'-03') | Diff | T-stat | Diff | T-stat |
| | (1) | (2) | (3) | (4) | (1)-(3) | | (2)-(4) | |
| *CPD* | 16.69 | 0.16 | 17.17 | 0.23 | -0.47 | -0.43 | -0.06 | -0.29 |
| *SIZE* | 8.23 | 0.02 | 8.23 | 0.02 | -0.00 | -0.03 | 0.00 | 0.77 |
| *LEV* | 0.28 | 5.20 | 0.28 | 0.13 | 0.01 | 0.37 | 5.06 | 1.40 |
| *MB* | 2.03 | 0.10 | 1.76 | 0.07 | 0.27 | 1.55 | 0.03 | 0.31 |
| *LTASSET* | 0.75 | 0.24 | 0.75 | 0.32 | -0.01 | -0.17 | -0.08 | -0.85 |
| *R&D* | 0.03 | 0.17 | 0.03 | 0.09 | 0.01 | 0.90 | 0.07 | 1.45 |
| *ROA* | 0.15 | -0.11 | 0.14 | -0.04 | 0.01 | 1.18 | -0.07 | -0.27 |
| *SPREAD* | 0.38 | -0.53 | 0.38 | -0.56 | -0.01 | -0.19 | 0.03 | 0.65 |
| *VOLATILITY* | 2.34 | -0.22 | 2.28 | -0.23 | 0.06 | 0.55 | 0.01 | 0.38 |
| *S.D. CF* | 0.06 | 0.43 | 0.06 | 0.43 | -0.00 | -0.15 | 0.00 | 0.00 |
| *S.D. SALES* | 0.25 | 0.44 | 0.27 | 0.40 | -0.02 | -0.53 | 0.05 | 0.37 |
| *VEGA* | 220.5 | 3838 | 177.5 | 49.1 | 43.1 | 1.64 | 3788 | 1.37 |
| *DELTA* | 891.3 | 0.84 | 726.1 | 0.54 | 165.2 | 1.16 | 0.30 | 0.83 |
| *CEO TURN* | 0.09 | -0.79 | 0.11 | -0.90 | -0.02 | -0.50 | 0.11 | 1.24 |
| *RET* | -0.04 | -0.15 | 0.01 | -1.59 | -0.05 | -0.94 | 1.44 | 1.57 |

## Table 3: Short-Selling and Investor Disagreement around Regulation SHO

This table reports short-selling activities and investor disagreement around Regulation SHO. In Panels A and B, we report the average of monthly short-selling activity, and investor disagreement for the Regulation SHO pilot and control firms before and during the program, respectively. Short-selling activities are measured with *SHORT RATIO* (monthly short interest divided by current-month number of shares outstanding, in percentages). Investor disagreement is measured with *DISPERSION* (standard deviation in monthly I/B/E/S analyst EPS forecasts, scaled by the mean forecast) and *TURNOVER* (firm trading value scaled by the previous 12-month average). In Panel C, we report the average *SHORT RATIO* when the sample is first partitioned into high and low disagreement by the CRSP universe median before the program.

| Panel A: Short Selling | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Pilot | | | | Control | | | | Pilot-Control | |
| | Before | During | Diff | T-Stat | Before | During | Diff | T-Stat | Diff-Diff | T-Stat |
| *SHORT RATIO* | 3.080 | 4.474 | 1.395 | 22.94 | 3.242 | 4.429 | 1.187 | 23.72 | 0.208 | 2.50 |
| **Panel B: Investor Disagreement** | | | | | | | | | | |
| | Pilot | | | | Control | | | | Pilot-Control | |
| | Before | During | Diff | T-Stat | Before | During | Diff | T-Stat | Diff-Diff | T-Stat |
| *DISPERSION* | 0.231 | 0.214 | -0.017 | -3.70 | 0.241 | 0.239 | -0.002 | -0.47 | -0.015 | -2.22 |
| *TURNOVER* | 1.923 | 1.483 | -0.440 | -13.40 | 1.819 | 1.455 | -0.364 | -17.40 | -0.076 | -2.02 |
| **Panel C: Short Selling and Investor Disagreement** | | | | | | | | | | |
| | High Disagreement (*DISPERSION*) | | | | Low Disagreement (*DISPERSION*) | | | | High-Low | |
| | Before | During | Diff | T-Stat | Before | During | Diff | T-Stat | Diff-Diff | T-Stat |
| *SHORT RATIO* | 3.719 | 6.573 | 2.854 | 45.43 | 2.872 | 5.164 | 2.293 | 42.84 | 0.562 | 6.84 |
| | High Disagreement (*TURNOVER*) | | | | Low Disagreement (*TURNOVER*) | | | | High-Low | |
| | Before | During | Diff | T-Stat | Before | During | Diff | T-Stat | Diff-Diff | T-Stat |
| *SHORT RATIO* | 3.810 | 7.610 | 3.801 | 48.71 | 3.226 | 5.333 | 2.107 | 31.18 | 1.694 | 14.87 |

Table 4: The Effect of Regulation SHO on CEO Compensation Duration

This table reports OLS regression results of differences in CEO compensation duration (*CPD*) between Regulation SHO pilot and control firms before, during, and after the program with the full sample and the balanced sample. All variables are defined in Section 3.3 and Appendix A. Industry fixed effects are based on SIC three-digit codes. T-statistics are reported in parentheses. * and ** denote significance at the 5% and 1% levels, respectively.

| | Full Sample | | | Balanced Sample | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *PILOT×DURING* | 0.084* | 0.089* | 0.087* | 0.096* | 0.101* | 0.101* |
| | (2.21) | (2.40) | (2.35) | (1.97) | (2.12) | (2.12) |
| *PILOT×POST* | 0.048 | 0.061 | 0.057 | 0.079 | 0.083 | 0.082 |
| | (1.16) | (1.50) | (1.41) | (1.50) | (1.59) | (1.59) |
| *PILOT* | -0.030 | -0.049 | -0.046 | -0.057 | -0.067 | -0.065 |
| | (-1.02) | (-1.70) | (-1.59) | (-1.53) | (-1.81) | (-1.76) |
| *SIZE* | | 0.073** | 0.077** | | 0.068** | 0.073** |
| | | (10.44) | (10.74) | | (6.46) | (6.78) |
| *MB* | | 0.045** | 0.044** | | 0.051** | 0.050** |
| | | (8.02) | (7.80) | | (5.68) | (5.60) |
| *LEV* | | -0.023 | -0.024 | | -0.086 | -0.091 |
| | | (-0.63) | (-0.67) | | (-1.59) | (-1.67) |
| *RET* | | -0.017 | -0.017 | | -0.058** | -0.059** |
| | | (-1.45) | (-1.45) | | (-3.36) | (-3.38) |
| *SPREAD* | | -0.122** | -0.131** | | -0.097** | -0.110** |
| | | (-6.04) | (-6.26) | | (-3.35) | (-3.69) |
| *LTASSET* | | 0.037 | 0.043 | | 0.020 | 0.030 |
| | | (1.13) | (1.33) | | (0.43) | (0.63) |
| *R&D* | | 0.224 | 0.181 | | 0.808** | 0.717** |
| | | (1.51) | (1.22) | | (2.98) | (2.62) |
| *VOLATILITY* | | | 0.012 | | | 0.017 |
| | | | (1.55) | | | (1.56) |
| *S.D. CF* | | | 0.213* | | | 0.228 |
| | | | (2.30) | | | (1.08) |
| *S.D. SALES* | | | -0.009 | | | -0.002 |
| | | | (-1.11) | | | (-0.16) |
| *INTERCEPT* | 3.047** | 2.333** | 2.259** | 2.744** | 2.221** | 2.087** |
| | (10.70) | (8.04) | (7.75) | (18.60) | (12.34) | (10.96) |
| Year Effect | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 5,600 | 5,600 | 5,600 | 2,999 | 2,999 | 2,999 |
| Adjusted $R^2$ | 0.11 | 0.15 | 0.15 | 0.16 | 0.19 | 0.19 |

Table 5: The Effect of Regulation SHO on CEO Compensation Duration—Alternative Specifications

This table reports OLS regression results of differences in CEO compensation duration (*CPD*) between Regulation SHO pilot and control firms before, during, and after the program with the full sample and the balanced sample. All variables are defined in Section 3.3 and Appendix A. Industry fixed effects are based on SIC three-digit codes. T-statistics are reported in parentheses. * and ** denote significance at the 5% and 1% levels, respectively.

| | Full Sample | | | Balanced Sample | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *PILOT×DURING* | 0.086* | 0.099** | 0.160* | 0.100* | 0.119* | 0.159 |
| | (2.31) | (2.65) | (2.44) | (2.09) | (2.47) | (1.91) |
| *PILOT×POST* | 0.056 | 0.062 | 0.109 | 0.084 | 0.069 | 0.063 |
| | (1.38) | (1.51) | (0.83) | (1.63) | (1.32) | (0.38) |
| *PILOT* | -0.046 | -0.054 | | -0.067 | -0.083* | |
| | (-1.59) | (-1.88) | | (-1.80) | (-2.25) | |
| *SIZE* | 0.072** | 0.083** | 0.083** | 0.068** | 0.073** | 0.073** |
| | (9.68) | (11.01) | (11.00) | (6.12) | (6.53) | (6.52) |
| *MB* | 0.042** | 0.034** | 0.034** | 0.048** | 0.034** | 0.034** |
| | (7.23) | (5.82) | (5.82) | (5.25) | (3.67) | (3.66) |
| *LEV* | -0.021 | -0.047 | -0.045 | -0.091 | -0.107* | -0.106 |
| | (-0.57) | (-1.28) | (-1.22) | (-1.67) | (-1.96) | (-1.94) |
| *RET* | -0.017 | 0.004 | 0.004 | -0.056** | -0.032 | -0.032 |
| | (-1.45) | (0.34) | (0.33) | (-3.22) | (-1.82) | (-1.83) |
| *SPREAD* | -0.133** | -0.157** | -0.157** | -0.109** | -0.107** | -0.107** |
| | (-6.35) | (-7.40) | (-7.41) | (-3.65) | (-3.53) | (-3.55) |
| *LTASSET* | 0.045 | 0.034 | 0.035 | 0.028 | 0.013 | 0.013 |
| | (1.39) | (1.04) | (1.05) | (0.58) | (0.27) | (0.26) |
| *R&D* | 0.179 | 0.307* | 0.311* | 0.751** | 0.862** | 0.866** |
| | (1.19) | (2.04) | (2.07) | (2.73) | (3.14) | (3.15) |
| *VOLATILITY* | 0.011 | 0.005 | 0.005 | 0.016 | 0.017 | 0.017 |
| | (1.50) | (0.63) | (0.65) | (1.46) | (1.55) | (1.55) |
| *S.D. CF* | 0.213* | 0.221* | 0.217* | 0.214 | 0.242 | 0.241 |
| | (2.29) | (2.36) | (2.32) | (1.01) | (1.14) | (1.13) |
| *S.D. SALES* | -0.010 | -0.014 | -0.014 | -0.002 | -0.008 | -0.008 |
| | (-1.18) | (-1.65) | (-1.63) | (-0.15) | (-0.87) | (-0.87) |
| *CEO TURN* | 0.041* | 0.035 | 0.034 | 0.060* | 0.066* | 0.065* |
| | (2.00) | (1.71) | (1.65) | (1.99) | (2.20) | (2.17) |
| *Log(DELTA)* | 0.014* | 0.001 | 0.001 | 0.012 | 0.006 | 0.006 |
| | (2.21) | (0.18) | (0.14) | (1.36) | (0.67) | (0.64) |
| *Log(VEGA)* | 0.002 | 0.002 | 0.002 | -0.000 | 0.001 | 0.001 |
| | (1.24) | (1.13) | (1.16) | (-0.04) | (0.21) | (0.26) |
| *INTERCEPT* | 2.198** | 2.295** | 2.291** | 2.057** | 2.022** | 2.019** |
| | (7.51) | (7.80) | (7.77) | (10.73) | (10.56) | (10.51) |
| Year Effect | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 5,600 | 5,568 | 5,568 | 2,999 | 2,994 | 2,994 |
| Adjusted $R^2$ | 0.15 | 0.14 | 0.14 | 0.19 | 0.17 | 0.17 |

## Table 6: Placebo Tests

This table reports OLS regression results of differences in CEO compensation duration (*CPD*) between Regulation SHO pilot and control firms before, during, and after the program with the full sample and the balanced sample. In specifications (1), (2), (5), and (6), we use a random number generator to select the same number of firms as in the Regulation SHO pilot group. In specifications (3), (4), (7), and (8), we define alternative timings for the pilot program: before-treatment is 1999 to 2001, during-treatment is 2003 to 2005, and post-treatment is 2007 to 2009. All other variables are defined in Section 3.3 and Appendix A. Industry fixed effects are based on SIC three-digit codes. T-statistics are reported in parentheses. * and ** denote significance at the 5% and 1% levels, respectively.

| | Full Sample | | | | Balanced Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | Random Treatment Firms | | Alt. Treatment Timing | | Random Treatment Firms | | Alt. Treatment Timing | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *PILOT×DURING* | -0.041 | -0.027 | -0.052 | -0.050 | -0.048 | -0.037 | -0.045 | -0.046 |
| | (-1.10) | (-0.73) | (-1.30) | (-1.28) | (-0.99) | (-0.79) | (-0.87) | (-0.90) |
| *PILOT×POST* | -0.051 | -0.039 | 0.044 | 0.051 | -0.007 | -0.001 | 0.078 | 0.077 |
| | (-1.27) | (-0.97) | (1.12) | (1.34) | (-0.13) | (-0.03) | (1.52) | (1.51) |
| *PILOT* | 0.037 | 0.021 | 0.023 | 0.010 | -0.005 | -0.017 | -0.006 | -0.011 |
| | (1.30) | (0.74) | (0.98) | (0.44) | (-0.13) | (-0.47) | (-0.18) | (-0.36) |
| *SIZE* | | 0.077** | | 0.072** | | 0.073** | | 0.061** |
| | | (10.75) | | (10.20) | | (6.79) | | (5.84) |
| *MB* | | 0.044** | | 0.027** | | 0.049** | | 0.022** |
| | | (7.74) | | (6.64) | | (5.41) | | (3.61) |
| *LEV* | | -0.023 | | -0.005 | | -0.088 | | -0.056 |
| | | (-0.64) | | (-0.13) | | (-1.63) | | (1.05) |
| *LTASSET* | | 0.044 | | 0.043 | | 0.028 | | 0.012 |
| | | (1.34) | | (1.34) | | (0.60) | | (0.26) |
| *R&D* | | 0.182 | | 0.033 | | 0.771** | | 0.147 |
| | | (1.22) | | (0.23) | | (2.80) | | (0.58) |
| *RET* | | -0.016 | | -0.013 | | -0.059** | | -0.039* |
| | | (-1.44) | | (-1.31) | | (-3.38) | | (-2.44) |
| *SPREAD* | | -0.132** | | -0.130** | | -0.112** | | -0.104** |
| | | (-6.27) | | (-7.54) | | (-3.76) | | (-4.34) |
| *VOLATILITY* | | 0.012 | | 0.017* | | 0.017 | | 0.023* |
| | | (1.57) | | (2.27) | | (1.54) | | (2.13) |
| *S.D. CF* | | 0.213* | | 0.253** | | 0.225 | | 0.348 |
| | | (2.29) | | (2.68) | | (1.06) | | (1.63) |
| *S.D. SALES* | | -0.010 | | -0.010 | | -0.001 | | -0.004 |
| | | (-1.14) | | (-1.13) | | (-0.14) | | (-0.35) |
| *INTERCEPT* | 3.053** | 2.260** | 2.956** | 2.274** | 2.731** | 2.085** | 2.530** | 2.013** |
| | (10.72) | (7.74) | (10.02) | (7.55) | (18.46) | (10.93) | (16.94) | (10.62) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 5,600 | 5,600 | 6,053 | 6,053 | 2,999 | 2,999 | 3,272 | 3,272 |
| Adjusted $R^2$ | 0.11 | 0.15 | 0.12 | 0.15 | 0.16 | 0.19 | 0.17 | 0.18 |

## Table 7: The Effect of Investor Disagreement and Regulation SHO on CEO Compensation Duration

This table reports OLS results of differences in CEO compensation duration (*CPD*) between Regulation SHO pilot and control firms before, during, and after the program with the full sample and the balanced sample. All variables are defined in Section 3.3 and Appendix A. In Panel A, firms are partitioned by their average analyst dispersion. In Panel B, firms are partitioned by abnormal turnover. Both variables are estimated and averaged over 1999 to 2003. Abnormal turnover is the average monthly stock turnover minus the average turnover from the previous year. Analyst dispersion is the average monthly standard deviation in analysts' EPS forecasts, scaled by the consensus forecast. Industry fixed effects are based on SIC three-digit codes. T-statistics are reported in parentheses. * and ** denote significance at the 5% and 1% levels, respectively.

| Panel A | Full Sample | | | | Balanced Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | Low *DISPERSION* | | High *DISPERSION* | | Low *DISPERSION* | | High *DISPERSION* | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *PILOT×DURING* | 0.055 | 0.071 | 0.163* | 0.150* | 0.011 | 0.043 | 0.269* | 0.222 |
| | (1.55) | (1.91) | (2.50) | (2.38) | (0.28) | (1.05) | (2.15) | (1.89) |
| *PILOT×POST* | 0.060 | 0.072 | 0.051 | 0.058 | 0.039 | 0.068 | 0.127 | 0.077 |
| | (1.45) | (1.75) | (0.69) | (0.80) | (0.56) | (0.96) | (0.99) | (0.59) |
| *PILOT* | -0.009 | -0.033 | -0.096 | -0.102 | 0.005 | -0.038 | -0.178 | -0.154 |
| | (-0.22) | (-0.83) | (-1.47) | (-1.51) | (0.08) | (-0.80) | (-1.35) | (-1.21) |
| *SIZE* | | 0.080** | | 0.076** | | 0.085** | | 0.068* |
| | | (5.43) | | (4.92) | | (3.81) | | (2.23) |
| *MB* | | 0.044** | | 0.037** | | 0.060** | | 0.020 |
| | | (5.07) | | (3.65) | | (4.89) | | (0.98) |
| *LEV* | | 0.032 | | -0.134 | | -0.131 | | 0.075 |
| | | (0.37) | | (-1.72) | | (-1.45) | | (0.59) |
| *LTASSET* | | 0.107 | | -0.012 | | 0.092 | | -0.026 |
| | | (1.87) | | (-0.21) | | (0.86) | | (-0.30) |
| *R&D* | | 0.266 | | -0.004 | | 0.174 | | 1.639** |
| | | (1.23) | | (-0.02) | | (0.42) | | (5.56) |
| *RET* | | -0.011 | | -0.021 | | -0.078 | | -0.037 |
| | | (-0.38) | | (-1.46) | | (-1.83) | | (-1.28) |
| *SPREAD* | | -0.080** | | -0.120** | | -0.081 | | -0.117** |
| | | (-3.12) | | (-7.02) | | (-1.88) | | (-3.56) |
| *VOLATILITY* | | 0.000 | | 0.025 | | -0.008 | | 0.058* |
| | | (-0.02) | | (1.18) | | (-0.31) | | (2.12) |
| *S.D. CF* | | 0.571* | | 0.091 | | 1.050 | | 0.199 |
| | | (2.32) | | (1.23) | | (1.70) | | (0.49) |
| *S.D. SALES* | | -0.004 | | -0.027 | | 0.003 | | -0.110 |
| | | (-0.50) | | (-1.23) | | (0.68) | | (-1.62) |
| *INTERCEPT* | 3.106** | 2.290** | 3.060** | 2.352** | 2.982** | 2.209** | 2.726** | 2.049** |
| | (27.09) | (18.68) | (78.53) | (10.94) | (24.58) | (7.47) | (12.61) | (5.13) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 3,380 | 3,380 | 2,097 | 2,097 | 1,948 | 1,948 | 1,034 | 1,034 |
| Adjusted $R^2$ | 0.14 | 0.17 | 0.12 | 0.16 | 0.19 | 0.22 | 0.19 | 0.22 |

Table 7 (cont'd)

| Panel B | Full Sample | | | | Balanced Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | Low *TURNOVER* | | High *TURNOVER* | | Low *TURNOVER* | | High *TURNOVER* | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *PILOT×DURING* | -0.018 | -0.008 | 0.142* | 0.139* | -0.095 | -0.052 | 0.208* | 0.201* |
| | (-0.29) | (-0.13) | (2.12) | (2.13) | (-1.29) | (-0.73) | (2.20) | (2.22) |
| *PILOT×POST* | -0.016 | 0.005 | 0.088 | 0.088 | -0.014 | 0.013 | 0.136 | 0.131 |
| | (-0.21) | (0.07) | (1.27) | (1.27) | (-0.17) | (0.16) | (1.47) | (1.45) |
| *PILOT* | 0.076 | 0.068 | -0.102 | -0.111 | 0.113 | 0.119 | -0.175 | -0.184* |
| | (1.28) | (1.24) | (-1.53) | (-1.72) | (1.59) | (1.81) | (-1.81) | (-1.96) |
| *SIZE* | | 0.086** | | 0.075** | | 0.064** | | 0.082** |
| | | (6.14) | | (5.16) | | (2.96) | | (4.27) |
| *MB* | | 0.071** | | 0.033** | | 0.105** | | 0.044** |
| | | (5.10) | | (3.37) | | (5.06) | | (2.70) |
| *LEV* | | -0.197** | | 0.026 | | -0.216 | | -0.030 |
| | | (-2.79) | | (0.40) | | (-1.82) | | (-0.32) |
| *LTASSET* | | 0.040 | | 0.064 | | -0.115 | | 0.096 |
| | | (0.56) | | (0.91) | | (-1.32) | | (1.15) |
| *R&D* | | 0.175 | | 0.199 | | 0.304 | | 0.628* |
| | | (0.63) | | (0.92) | | (0.44) | | (1.98) |
| *RET* | | -0.004 | | -0.020 | | -0.070* | | -0.052 |
| | | (-0.21) | | (-0.86) | | (-1.99) | | (-1.35) |
| *SPREAD* | | -0.083* | | -0.158** | | -0.067 | | -0.133* |
| | | (-2.14) | | (-3.45) | | (-1.07) | | (-2.46) |
| *VOLATILITY* | | 0.001 | | 0.015 | | -0.017 | | 0.030 |
| | | (0.07) | | (1.42) | | (-0.70) | | (1.48) |
| *S.D. CF* | | 0.408 | | 0.109 | | 1.125* | | -0.002 |
| | | (1.83) | | (1.04) | | (2.14) | | (-0.00) |
| *S.D. SALES* | | 0.078 | | -0.015 | | 0.099 | | -0.004 |
| | | (1.10) | | (-1.06) | | (1.25) | | (-0.62) |
| *INTERCEPT* | 3.056** | 2.281** | 3.072** | 2.334** | 3.042** | 2.477** | 2.744** | 1.970** |
| | (54.42) | (13.78) | (54.40) | (13.27) | (45.67) | (11.14) | (11.74) | (6.87) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 1,988 | 1,988 | 3,606 | 3,606 | 1,081 | 1,081 | 1,905 | 1,905 |
| Adjusted $R^2$ | 0.14 | 0.20 | 0.13 | 0.16 | 0.20 | 0.24 | 0.19 | 0.21 |

## Table 8: The Effect of Investor Disagreement Change and Regulation SHO on CEO Compensation Duration

This table reports OLS regression results of differences in CEO compensation duration (CPD) between Regulation SHO pilot and control firms around the Regulation SHO program with the full sample and the balanced sample. All variables are defined in Section 3.3 and Appendix A. Firms are partitioned by the change in average disagreement from the *PRE* period to the *DURING* period. *DISPERSION* is average monthly analyst forecast dispersion, scaled by the consensus forecast, and *TURNOVER* is average monthly abnormal turnover. Industry fixed effects are based on SIC three-digit codes. T-statistics are reported in parentheses. * and ** denote significance at the 5% and 1% levels, respectively.

| Panel A | Full Sample | | | | Balanced Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | Low Δ*DISPERSION* | | High Δ*DISPERSION* | | Low Δ*DISPERSION* | | High Δ*DISPERSION* | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *PILOT×DURING* | 0.153** | 0.137** | 0.056 | 0.071 | 0.130* | 0.114 | 0.069 | 0.085 |
| | (2.96) | (2.69) | (0.96) | (1.23) | (2.07) | (1.86) | (0.96) | (1.19) |
| *PILOT* | -0.050 | -0.038 | -0.054 | -0.073 | -0.015 | -0.013 | -0.090 | -0.097 |
| | (-1.20) | (-0.91) | (-1.19) | (-1.62) | (-0.29) | (-0.26) | (-1.56) | (-1.68) |
| *SIZE* | | 0.073** | | 0.051** | | 0.097** | | 0.052** |
| | | (5.75) | | (3.77) | | (5.39) | | (2.76) |
| *MB* | | 0.028** | | 0.039** | | 0.027 | | 0.034* |
| | | (2.85) | | (4.27) | | (1.86) | | (2.54) |
| *LEV* | | 0.011 | | -0.038 | | 0.047 | | -0.058 |
| | | (0.17) | | (-0.63) | | (0.46) | | (-0.68) |
| *LTASSET* | | -0.050 | | 0.212** | | -0.032 | | 0.224* |
| | | (-0.88) | | (3.16) | | (-0.44) | | (2.16) |
| *R&D* | | 0.640* | | -0.139 | | 1.185** | | 0.292 |
| | | (2.22) | | (-0.55) | | (2.98) | | (0.60) |
| *RET* | | -0.036 | | 0.003 | | -0.046 | | -0.007 |
| | | (-1.45) | | (0.10) | | (-1.44) | | (-0.18) |
| *SPREAD* | | -0.131** | | -0.125** | | -0.122** | | -0.143** |
| | | (-4.11) | | (-3.69) | | (-3.06) | | (-3.21) |
| *VOLATILITY* | | 0.058** | | 0.022 | | 0.078** | | 0.044 |
| | | (3.89) | | (1.46) | | (4.10) | | (1.67) |
| *S.D. CF* | | 0.146 | | 0.540 | | -0.205 | | 0.799 |
| | | (1.33) | | (1.66) | | (-0.65) | | (1.57) |
| *S.D. SALES* | | -0.083** | | -0.003 | | -0.174** | | 0.021 |
| | | (-2.67) | | (-0.10) | | (-3.01) | | (0.33) |
| *INTERCEPT* | 2.880** | 2.066** | 2.721** | 1.969** | 2.564** | 1.696** | 2.620** | 1.778** |
| | (7.22) | (4.91) | (27.32) | (9.82) | (14.33) | (6.60) | (19.21) | (6.14) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 2,192 | 2,192 | 1,697 | 1,697 | 1,406 | 1,406 | 1,134 | 1,134 |
| Adjusted $R^2$ | 0.17 | 0.20 | 0.14 | 0.17 | 0.19 | 0.23 | 0.15 | 0.16 |

Table 8 (cont'd)

| Panel B | Full Sample | | | | Balanced Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | Low Δ*TURNOVER* | | High Δ*TURNOVER* | | Low Δ*TURNOVER* | | High Δ*TURNOVER* | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *PILOT×DURING* | 0.167* | 0.167* | 0.033 | 0.031 | 0.237* | 0.226* | -0.029 | -0.035 |
| | (2.01) | (2.08) | (0.57) | (0.56) | (2.30) | (2.29) | (-0.44) | (-0.56) |
| *PILOT* | -0.129 | -0.140 | 0.035 | 0.026 | -0.173 | -0.198* | 0.072 | 0.089 |
| | (-1.58) | (-1.79) | (0.63) | (0.49) | (-1.68) | (-2.06) | (1.08) | (1.41) |
| *SIZE* | | 0.060** | | 0.089** | | 0.076** | | 0.102** |
| | | (3.69) | | (5.09) | | (3.48) | | (3.78) |
| *MB* | | 0.026* | | 0.038** | | 0.038* | | 0.022 |
| | | (2.04) | | (3.31) | | (2.10) | | (1.18) |
| *LEV* | | -0.059 | | 0.045 | | -0.132 | | 0.118 |
| | | (-0.76) | | (0.52) | | (-1.08) | | (0.83) |
| *LTASSET* | | 0.141 | | 0.050 | | 0.055 | | 0.018 |
| | | (1.24) | | (0.68) | | (0.46) | | (0.20) |
| *R&D* | | 0.237 | | 0.433 | | 0.381 | | 1.150 |
| | | (0.87) | | (1.72) | | (0.74) | | (1.92) |
| *RET* | | -0.024 | | -0.005 | | -0.059 | | 0.014 |
| | | (-0.62) | | (-0.14) | | (-0.95) | | (0.29) |
| *SPREAD* | | -0.173** | | -0.120** | | -0.150** | | -0.108* |
| | | (-3.50) | | (-3.28) | | (-2.86) | | (-2.26) |
| *VOLATILITY* | | 0.030 | | 0.050** | | 0.066* | | 0.044* |
| | | (1.57) | | (2.96) | | (2.36) | | (2.00) |
| *S.D. CF* | | 0.125 | | 0.116 | | -0.448 | | 0.544 |
| | | (0.38) | | (1.44) | | (-0.89) | | (1.23) |
| *S.D. SALES* | | -0.058 | | 0.013 | | -0.015 | | 0.090 |
| | | (-1.85) | | (0.54) | | (-0.18) | | (1.09) |
| *INTERCEPT* | 3.121** | 2.595** | 2.881** | 1.827** | 3.142** | 2.391** | 2.246** | 1.332** |
| | (51.40) | (11.94) | (54.00) | (7.99) | (34.15) | (7.82) | (67.90) | (5.09) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 2,018 | 2,018 | 1,987 | 1,987 | 1,275 | 1,275 | 1,285 | 1,285 |
| Adjusted $R^2$ | 0.14 | 0.16 | 0.19 | 0.23 | 0.13 | 0.16 | 0.20 | 0.24 |

## Table 9: The Effect of Short-Term-Oriented Institutional Investors and Regulation SHO on CEO Compensation Duration

This table reports OLS regression results of differences in CEO compensation duration (CPD) between Regulation SHO pilot and control firms before, during, and after the program with the full sample and the balanced sample. All variables are defined in Section 3.3 and Appendix A. Firms are partitioned by the ratio of short-term-oriented institutional ownership divided by long-term-oriented institutional ownership in 2003 (*STIO/LTIO*). Short-term-oriented institutional ownership consists of transient investors, and long-term-oriented institutional ownership consists of dedicated and quasi-indexers, as defined by Bushee (1998). Industry fixed effects are based on SIC three-digit codes. T-statistics are reported in parentheses. * and ** denote significance at the 5% and 1% levels, respectively.

| | Full Sample | | | | Balanced Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | Low *STIO/LTIO* | | High *STIO/LTIO* | | Low *STIO/LTIO* | | High *STIO/LTIO* | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *PILOT×DURING* | -0.028 | -0.026 | 0.171* | 0.167* | -0.013 | 0.011 | 0.213* | 0.191* |
| | (-0.42) | (-0.40) | (2.36) | (2.39) | (-0.15) | (0.13) | (2.09) | (1.97) |
| *PILOT×POST* | -0.023 | -0.018 | 0.115 | 0.123 | 0.010 | 0.030 | 0.139 | 0.120 |
| | (-0.30) | (-0.25) | (1.50) | (1.63) | (0.11) | (0.37) | (1.32) | (1.18) |
| *PILOT* | 0.056 | 0.024 | -0.070 | -0.073 | 0.102 | 0.056 | -0.187 | -0.148 |
| | (0.97) | (0.42) | (-0.93) | (-1.01) | (1.34) | (0.76) | (-1.62) | (-1.40) |
| *SIZE* | | 0.108** | | 0.069** | | 0.096** | | 0.075** |
| | | (5.81) | | (4.77) | | (4.81) | | (3.41) |
| *MB* | | 0.054** | | 0.041** | | 0.058* | | 0.035* |
| | | (3.35) | | (4.75) | | (2.01) | | (2.18) |
| *LEV* | | -0.048 | | -0.083 | | -0.179 | | -0.035 |
| | | (-0.52) | | (-1.26) | | (-1.85) | | (-0.32) |
| *LTASSET* | | 0.073 | | 0.038 | | -0.006 | | 0.098 |
| | | (0.87) | | (0.61) | | (-0.06) | | (0.99) |
| *R&D* | | 1.005* | | -0.059 | | 1.359 | | 0.473 |
| | | (2.25) | | (-0.32) | | (1.37) | | (1.25) |
| *RET* | | -0.021 | | -0.023 | | -0.034 | | -0.066 |
| | | (-0.68) | | (-1.08) | | (-0.75) | | (-1.70) |
| *SPREAD* | | -0.128 | | -0.127** | | -0.054 | | -0.136** |
| | | (1.81) | | (-3.90) | | (-0.78) | | (-2.64) |
| *VOLATILITY* | | 0.044* | | 0.004 | | 0.043* | | 0.019 |
| | | (2.47) | | (0.45) | | (1.99) | | (1.00) |
| *S.D. CF* | | 0.471* | | 0.147 | | 0.795 | | 0.246 |
| | | (1.99) | | (1.34) | | (1.16) | | (0.48) |
| *S.D. SALES* | | -0.112 | | -0.002 | | -0.095 | | 0.002 |
| | | (-1.80) | | (-0.19) | | (-1.09) | | (0.66) |
| *INTERCEPT* | 2.968** | 1.728** | 3.147** | 2.620** | 2.662** | 1.614** | 2.792** | 2.091** |
| | (48.29) | (6.59) | (62.87) | (18.92) | (63.34) | (5.49) | (11.49) | (7.02) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 2,554 | 2,554 | 3,022 | 3,022 | 1,531 | 1,531 | 1,410 | 1,410 |
| Adjusted $R^2$ | 0.15 | 0.20 | 0.12 | 0.16 | 0.21 | 0.25 | 0.14 | 0.17 |

Table 10: The Effect of Regulation SHO on Corporate Behavior

This table reports 2SLS and reduced-form regression results of the effect of Regulation SHO on CEO and corporate behavior before, during, and after the program. Panel A uses the full sample, and Panel B uses the balanced sample. All variable definitions are in Section 3 and Appendix A. For all specifications, we include the full set of control variables as in Table 4. Industry fixed effects are based on SIC three-digit codes. T-statistics are reported in parentheses. * and ** denote significance at the 5% and 1% levels, respectively.

Panel A: Full Sample

| Dependent Variable | *OVERINVEST* | | *RD* | | *ARP* | | *EM* | | *HOR* | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Log(CPD) (fitted)* | -0.907** | | 0.309 | | -0.030 | | -0.498** | | -0.129* | |
| | (-2.66) | | (1.86) | | (-1.66) | | (-2.65) | | (-2.48) | |
| *PILOT×DURING* | | -0.056** | | 0.020 | | -0.003** | | -0.058** | | -0.021* |
| | | (-3.09) | | (1.37) | | (-2.88) | | (-3.10) | | (-2.09) |
| *PILOT×POST* | | -0.014 | | 0.003 | | -0.003* | | -0.029 | | -0.011 |
| | | (-0.54) | | (0.19) | | (-2.08) | | (-1.43) | | (-0.90) |
| *PILOT* | | -0.013 | | 0.008 | | 0.002 | | 0.045** | | 0.014 |
| | | (-0.62) | | (0.68) | | (1.88) | | (3.12) | | (1.42) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 5,510 | 5,510 | 5,612 | 5,612 | 2,458 | 2,458 | 5,526 | 5,526 | 2,389 | 2,389 |

Table 10 (cont'd)

| Panel B: Balanced Sample | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Dependent Variable | *OVERINVEST* | | *RD* | | *ARP* | | *EM* | | *HOR* | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Log(CPD) (fitted)* | -0.817* | | 0.032 | | -0.020 | | -0.390 | | -0.018 | |
| | (-2.35) | | (0.21) | | (-0.83) | | (-1.81) | | (-0.70) | |
| *PILOT×DURING* | | -0.095** | | 0.024 | | -0.003* | | -0.052* | | -0.007 |
| | | (-3.16) | | (1.35) | | (-2.11) | | (-2.06) | | (-0.96) |
| *PILOT×POST* | | -0.016 | | -0.005 | | 0.000 | | -0.033 | | -0.009 |
| | | (-0.42) | | (-0.26) | | (0.16) | | (-1.20) | | (-1.03) |
| *PILOT* | | 0.018 | | 0.014 | | -0.001 | | 0.040* | | 0.004 |
| | | (0.48) | | (1.05) | | (-0.48) | | (2.05) | | (0.49) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Controls | YES | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 2,846 | 2,846 | 2,948 | 2,948 | 1,188 | 1,188 | 2,908 | 2,908 | 1,209 | 1,209 |

## Table A1: Volatility and Market Quality around Regulation SHO

This table reports volatility and market quality around Regulation SHO. We report the average of monthly return volatility, effective spread, and fraction of zero-return days for the Regulation SHO pilot and control firms before and during the program, respectively. Return volatility (*VOLATILITY*) is defined as the annualized standard deviation of daily stock returns. Effective spread (*EFF. SPREAD*) is the effective tick measure by Goyenko, Holden, and Trzcinka (2009). Fraction of zero-return days (*RET FRACTION*) is the fraction of trading days with zero return and non-zero volume.

| | Pilot | | | | Control | | | | Pilot-Control | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Before | During | Diff | T-Stat | Before | During | Diff | T-Stat | Diff-Diff | T-Stat |
| *VOLATILITY* | 0.074 | 0.059 | -0.015 | -15.49 | 0.072 | 0.061 | -0.011 | -16.11 | -0.003 | -2.80 |
| *EFF. SPREAD* | 0.152 | 0.095 | -0.056 | -9.85 | 0.160 | 0.120 | -0.039 | -7.15 | -0.017 | -1.98 |
| *RET FRACTION* | 0.059 | 0.059 | -0.001 | -0.51 | 0.059 | 0.064 | 0.005 | -3.92 | -0.006 | -2.79 |

Table A2: The Effect of Regulation SHO on CEO Compensation Duration: Stocks vs. Options

This table reports OLS regression results of differences in CEO compensation duration (CPD) between Regulation SHO pilot and control firms before, during, and after the program with the full sample and the balance sample. *Log(CPD Stock)* and *Log(CPD Option)* are the value-weighted average vesting periods of stock grants and option grants for each CEO-year, respectively. All variables are defined in Section 3.3 and Appendix A. Industry fixed effects are based on SIC three-digit codes. T-statistics are reported in parentheses. * and ** denote significance at the 5% and 1% levels, respectively.

| | Full Sample | | | | Balanced Sample | | | |
|---|---|---|---|---|---|---|---|---|
| Dependent Variable | *Log(CPD Stock)* | | *Log(CPD Option)* | | *Log(CPD Stock)* | | *Log(CPD Option)* | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *PILOT×DURING* | 0.125* | 0.132* | -0.014 | -0.013 | 0.147 | 0.144 | -0.005 | -0.011 |
| | (2.08) | (2.18) | (-0.68) | (-0.62) | (1.58) | (1.62) | (-0.22) | (-0.43) |
| *PILOT×POST* | 0.083 | 0.095 | 0.011 | 0.012 | 0.079 | 0.097 | 0.008 | 0.007 |
| | (1.39) | (1.61) | (0.46) | (0.52) | (0.81) | (1.04) | (0.23) | (0.19) |
| *PILOT* | -0.094 | -0.106 | 0.006 | 0.005 | -0.092 | -0.096 | -0.054 | -0.046 |
| | (-1.54) | (-1.72) | (0.24) | (0.18) | (-0.87) | (-0.96) | (-1.69) | (-1.49) |
| *SIZE* | | 0.010 | | -0.003 | | 0.018 | | -0.004 |
| | | (0.67) | | (-0.39) | | (1.07) | | (-0.36) |
| *MB* | | 0.020* | | 0.007 | | 0.027 | | -0.007 |
| | | (2.17) | | (1.36) | | (1.52) | | (-0.76) |
| *LEV* | | -0.054 | | -0.020 | | -0.160* | | 0.027 |
| | | (-1.01) | | (-0.49) | | (-2.00) | | (0.38) |
| *LTASSET* | | 0.073 | | 0.026 | | 0.139 | | 0.062 |
| | | (1.64) | | (0.84) | | (1.46) | | (1.21) |
| *R&D* | | -0.423 | | 0.132 | | 0.086 | | 0.514* |
| | | (-1.53) | | (1.22) | | (0.16) | | (2.11) |
| *RET* | | -0.046** | | 0.010 | | -0.080* | | 0.015 |
| | | (-3.45) | | (1.27) | | (-2.03) | | (0.75) |
| *SPREAD* | | -0.023 | | -0.006 | | -0.082 | | 0.007 |
| | | (-0.44) | | (-0.49) | | (-1.63) | | (0.42) |
| *VOLATILITY* | | -0.014 | | -0.002 | | -0.023 | | 0.007 |
| | | (-1.40) | | (-0.29) | | (-1.49) | | (1.16) |
| *S.D. CF* | | 0.002 | | -0.010 | | 0.509 | | -0.282 |
| | | (0.05) | | (-0.17) | | (1.25) | | (-1.17) |
| *S.D. SALES* | | 0.007 | | -0.005 | | 0.019** | | 0.003 |
| | | (0.58) | | (-0.53) | | (3.56) | | (0.63) |
| *INTERCEPT* | 3.619** | 3.505** | 3.234** | 3.223** | 3.540** | 3.459** | 3.475** | 3.484** |
| | (9.58) | (28.64) | (84.79) | (41.72) | (50.33) | (17.82) | (121.76) | (24.12) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 2,558 | 2,558 | 4,374 | 4,374 | 1,348 | 1,348 | 2,455 | 2,455 |
| Adjusted $R^2$ | 0.13 | 0.14 | 0.12 | 0.12 | 0.17 | 0.19 | 0.16 | 0.16 |

## Table A3: The Effect of Asymmetric Information and Regulation SHO on CEO Compensation Duration

This table reports OLS regression results of differences in CEO compensation duration (CPD) between Regulation SHO pilot and control firms before, during, and after the program with the full sample and the balanced sample. All variables are defined as in Section 3.3 and Appendix A. Firms are partitioned by their average asymmetric information measure (γ) in the *PRE* period, following Pastor and Stambaugh (2003). Industry fixed effects are based on SIC three-digit codes. T-statistics are reported in parentheses. * and ** denote significance at the 5% and 1% levels, respectively.

| | Full Sample | | | | Balanced Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | Low Asym. Info. | | High Asym. Info. | | Low Asym. Info. | | High Asym. Info. | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *PILOT×DURING* | 0.042 | 0.071 | 0.122* | 0.105* | -0.012 | 0.026 | 0.187** | 0.165** |
| | (0.72) | (1.25) | (2.41) | (2.11) | (-0.16) | (0.35) | (2.93) | (2.60) |
| *PILOT×POST* | 0.018 | 0.050 | 0.085 | 0.076 | 0.033 | 0.066 | 0.120 | 0.103 |
| | (0.29) | (0.81) | (1.54) | (1.39) | (0.42) | (0.85) | (1.72) | (1.49) |
| *PILOT* | -0.002 | -0.041 | -0.042 | -0.040 | -0.007 | -0.085 | -0.075 | -0.059 |
| | (-0.04) | (-0.91) | (-1.06) | (-1.01) | (-0.11) | (-1.35) | (-1.48) | (-1.17) |
| *SIZE* | | 0.094** | | 0.073** | | 0.090** | | 0.063** |
| | | (7.36) | | (7.94) | | (4.48) | | (4.42) |
| *MB* | | 0.060** | | 0.034** | | 0.064** | | 0.036** |
| | | (5.97) | | (4.77) | | (4.14) | | (3.09) |
| *LEV* | | 0.022 | | -0.055 | | -0.098 | | -0.116 |
| | | (0.36) | | (-1.18) | | (-1.00) | | (-1.70) |
| *LTASSET* | | 0.057 | | 0.056 | | 0.084 | | -0.010 |
| | | (1.07) | | (1.28) | | (0.98) | | (-0.15) |
| *R&D* | | -0.119 | | 0.265 | | 0.626 | | 0.777* |
| | | (0.42) | | (1.47) | | (1.17) | | (2.34) |
| *RET* | | -0.032 | | -0.015 | | -0.058* | | -0.067** |
| | | (-1.64) | | (-1.01) | | (-2.43) | | (-2.59) |
| *SPREAD* | | -0.096** | | -0.162** | | -0.101* | | -0.108** |
| | | (-3.17) | | (-5.27) | | (-2.22) | | (-2.59) |
| *VOLATILITY* | | 0.004 | | 0.018 | | -0.008 | | 0.036* |
| | | (0.34) | | (1.87) | | (-0.47) | | (2.42) |
| *S.D. CF* | | 0.691* | | 0.144 | | 1.204* | | -0.131 |
| | | (2.40) | | (1.46) | | (2.25) | | (-0.55) |
| *S.D. SALES* | | -0.004 | | -0.010 | | 0.224* | | -0.002 |
| | | (-0.14) | | (-1.08) | | (1.99) | | (-0.25) |
| *INTERCEPT* | 2.496** | 1.699** | 3.039** | 2.306** | 2.477** | 1.650** | 3.066** | 2.495** |
| | (12.33) | (7.20) | (10.77) | (7.71) | (12.57) | (6.05) | (13.97) | (8.56) |
| Year Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| Industry Effect | YES | YES | YES | YES | YES | YES | YES | YES |
| No. of Obs. | 2,382 | 2,382 | 3,213 | 3,213 | 1,362 | 1,362 | 1,637 | 1,637 |
| Adjusted $R^2$ | 0.14 | 0.17 | 0.13 | 0.16 | 0.17 | 0.20 | 0.20 | 0.23 |