Short Selling and Earnings Management: A Controlled Experiment[*]

Vivian W. Fang [a]
University of Minnesota

Allen Huang [b]
Hong Kong University of Science and Technology

Jonathan Karpoff [c]
University of Washington

This draft: March 23, 2014

Abstract

During 2005-2007, the SEC ordered a pilot program in which one-third of the Russell 3000 index were arbitrarily chosen as pilot stocks and exempted from short-sale price tests. Pilot firms' discretionary accruals reduce during this period, and revert to pre-experiment levels when the program ends. Among firms that initiate financial misconduct before the program begins, pilot firms are caught more quickly once the program starts. During the program, pilot firms' current returns better reflect future earnings, and their post-earnings announcement drift decreases. We conclude that decreases in short selling costs constrain firms' opportunistic reporting behavior and enhance stock price efficiency.

JEL classifications: G14; G18; G19; M41; M48

Keywords: Regulation SHO; Pilot Program; Short Selling; Earnings Management; Price efficiency

---

[*] We thank Adam Kolasinski, Paul Ma, Scott Richardson, Ed Swanson, Jake Thornock, Wendy Wilson, and seminar participants at FARS Midyear Meeting, HKUST Accounting Symposium, Conference on Financial Economics and Accounting, and UC Berkeley Multi-disciplinary Conference on Fraud and Misconduct for helpful comments. We are grateful to Russell Investments for providing the list of 2004 Russell 3000 index and to Alex Edmans for sharing the CEO wealth-performance sensitivity data used in this paper. Huang gratefully acknowledges financial support from a grant from the Research Grants Council of the HKSAR, China (Project No., HKUST691213).

[a] *Email:* fangw@umn.edu, *Tel:* +1 612-625-5050, Carlson School of Management, University of Minnesota, Minneapolis, MN 55455, USA.
[b] *Email:* allen.huang@ust.hk, The Hong Kong University of Science and Technology, Clear Water Bay, Kowloon, Hong Kong
[c] *Email:* karpoff@uw.edu, Foster School of Business, University of Washington, Seattle, WA 98195, USA.

Short Selling and Earnings Management: A Controlled Experiment

1. Introduction

Previous research shows that short sellers can identify earnings manipulation and fraud before they are publicly revealed.[1] But this is for earnings manipulations that have already taken place. Might short selling also constrain firms' incentives to manipulate or misrepresent earnings in the first place? That is, does short selling, or its prospect, help to improve the quality of firms' financial reporting in general?

In this paper we exploit a natural experiment that allows us to address this question. In July 2004, the Securities and Exchange Commission (SEC) adopted a new regulation governing short selling activities in the U.S. equity markets—Regulation SHO. Regulation SHO contained a Rule 202T pilot program in which every third stock ranked by trading volume within each exchange was drawn from the Russell 3000 index and designated as a pilot stock. From May 2, 2005 to August 6, 2007, pilot stocks were exempted from short-sale price tests, thus decreasing the cost of short selling these stocks. The price tests, which include the tick test for exchange-listed stocks and the bid test for Nasdaq National Market Stocks, were maintained for non-pilot stocks.[2]

The pilot program creates an ideal setting to examine the effect of short-sale constraints on corporate financial reporting decisions, for three reasons. First, the pilot program represents a truly exogenous shock to the cost of selling short in the affected firms. We can identify no evidence that the firms themselves lobbied for the pilot program, or that any individual firm

---

[1] See Dechow, Sloan and Sweeney (1996), Christophe, Ferri, and Angel (2004), Efendi, Kinney, and Swanson (2005), Desai, Krishnamurthy, and Venkataraman (2006), and Karpoff and Lou (2010).

[2] The pilot program was originally scheduled to commence on January 3, 2005 and end on December 31, 2005 (Securities Exchange Act Release No. 50104, July 28, 2004). However, the SEC postponed the commencement date to May 2, 2005 (Securities and Exchange Act Release No. 50747, November 29, 2004) and extended the end date to August 6, 2007 (Securities and Exchange Act Release No. 53684, April 20, 2006).

could know it would be in the pilot group until the program was announced. Second, the pilot program initiated a meaningful decrease in the cost of selling short among the pilot stocks.[3] The pilot program eliminates the need to estimate short selling costs – a notoriously difficult task (see Lamont, 2004) – since it defines a treatment group (the pilot stocks) in which short selling costs were decreased relative to the control group (the non-pilot stocks). Third, the pilot program had specific beginning and ending dates, facilitating a difference-in-differences (hereafter, DiD) analysis of the impact of short selling costs on firms' financial reporting. The known ending date allows us to investigate whether the effects of the pilot program reversed when it ended – an important check on the internal validity of the DiD tests (e.g., see Roberts and Whited, 2012).

We begin by verifying that pilot firms represent a random draw from the Russell 3000 population. In the fiscal year before the pilot program, the pilot and non-pilot firms are similar in size, growth, corporate spending, profitability, leverage, and dividend payout. Although the two groups of firms also exhibit similar levels of discretionary accruals before the program, pilot firms significantly reduce their discretionary accruals once the program starts.[4] After the program ends, pilot firms' discretionary accruals revert to pre-program levels. The non-pilot firms, meanwhile, show no significant change in their discretionary accruals during or after the pilot program.

We also find that the impact of the pilot program on earnings management is most pronounced among firms with high institutional ownership and high sensitivity of the CEO's

[3]For evidence of the effects of the change in the price tests on the cost of short selling, see the SEC's Office of Economic Analysis (2007). For evidence that the pilot program led to a meaningful increase in short selling among the pilot firms, see Diether, Lee, and Werner (2009). Survey evidence further indicates that the vast majority of corporate executives care about the impact of the elimination of price tests on the actual and potential amount of short selling in their firms (Opinion Research Corporation, 2008).

[4] Following the literature (e.g., Kothari, Leone, and Wasley, 2005), we measure discretionary accruals as the difference between actual accruals and a benchmark estimated for each industry-year. Details are provided in Section 3.3.

wealth to the company's stock price. Institutional ownership is associated with the availability of shares that makes short selling feasible (e.g., see Nagel, 2005). So these results indicate that a reduction in short selling costs is particularly effective at deterring earnings management when short selling is most feasible and when managers likely have greater incentives to manipulate earnings.

We consider several alternate interpretations for these patterns in pilot firms' discretionary accruals. One possibility is that pilot firms' discretionary accruals reflect changes in their investment, growth, or equity issues, as Grullon, Michenaud, and Weston (2013) document a significant reduction in pilot firms' investment and equity issuance compared to non-pilot firms during the pilot program. We consider several controls for firm growth and investment, both in the construction of our discretionary accruals measures and as controls in multivariate tests for changes in accruals. None of these controls has a material effect on our main findings. We also find that the pilot firms' investment levels do not follow a pattern during and after the pilot program that would explain their changes in discretionary accruals. Regarding the possible impact of equity issues, we find that pilot firms' discretionary accruals pattern is similar among firms that do not issue equity as for the overall sample. These results indicate that the effect of the pilot program on discretionary accruals is unlikely to be explained by the changes in the difference between two groups of firms' investment or equity issuance surrounding the program.

We also consider an alternate explanation that managers of the pilot firms decreased their use of discretionary accruals because of a general increase in investors' awareness of and attention to these firms. Using three measures of market attention, however, we do not find that pilot firms were subject to greater attention during the pilot program. In multivariate DiD tests, the market attention measures are not significantly related to changes in discretionary accruals; nor do they affect our main finding regarding the reversing pattern of pilot firms' discretionary accruals.

We then examine the effect of the pilot program on the discovery of financial misconduct that eventually leads to enforcement action by the SEC. Our main finding suggests that the increased threat of short selling deters aggressive earnings manipulations and decreases the incidence of financial misconduct. For firms that already were misrepresenting their earnings, however, the initiation of the pilot program should correspond to an increase in the speed with which pilot firms' misrepresentation will be discovered. This is because the reduction in short selling costs will encourage short sellers to gather private information and trade more aggressively, activities that are known to accelerate regulators' attention to financial misconduct.[5] Consistent with this expectation, we show that, among firms that do misrepresent their earnings prior to the pilot program, the conditional probability of detection is significantly higher for pilot firms after the pilot program begins.

Finally, we examine the implications of the pilot program for price efficiency and market quality through its effect on firms' reporting practices. We show that the pilot firms' coefficients of current returns on future earnings increase. Among firms announcing particularly negative earnings surprises, the well-documented post-earnings announcement drift disappears for pilot firms, while it remains significant for non-pilot firms. These two results indicate that the reduction in pilot firms' earnings management during the pilot program corresponds to an increase in the efficiency of their stock prices.

---

[5] See the references in footnote 1.

These findings make four contributions to the literature. First, they show that an increase in the prospect of short selling has real effects on firms' financial reporting. This demonstrates one avenue through which trading in secondary financial markets affects firms' decisions.[6] Second, our results highlight one important avenue through which short selling improves price discovery and makes prices more efficient. Previous research emphasizes how short selling facilitates the flow of private information into prices (e.g., Miller, 1977; Harrison and Kreps, 1978). Our findings suggest that short selling also improves price efficiency by decreasing managers' use of discretionary accruals. Third, our findings identify a new determinant of earnings management – short-sale constraints – in addition to the factors identified in previous research (for a review, see Dechow, Ge, and Schrand, 2010). And fourth, these results contribute to the policy debate over the benefits and costs of short selling. Previous research demonstrates that short sellers frequently are good at identifying the overpriced shares of firms that have manipulated earnings, and that short sellers' trading conveys external benefits to other investors by improving market quality and by accelerating the discovery of financial misconduct.[7] Our results indicate that the prospect of short selling decreases earnings management and increases price efficiency in general, even among firms that are not charged with financial reporting violations. This indicates that short selling, or its prospect, conveys external benefits to investors by improving the quality of financial reporting and the efficiency of stock prices.

[6] See Bond, Edmans, and Goldstein (2012) for a survey of research on the real effects of financial markets. For example, Fang, Noe, and Tice (2009) and Edmans, Fang, Zur (2013) examine the effect of stock liquidity on firm performance and governance; Kang and Kim (2013) examine the effect of liquidity on CEO turnover; Fang, Tian, and Tice (2013) examine the effect of liquidity on innovation, Grullon, Michenaud, and Weston (2013) examine the effect of short selling on equity issuance and investment; and Massa, Zhang, and Zhang (2013b) examine the effect of short selling on governance.

[7] See the references in footnote 1, and also the SEC's Office of Economic Analysis (2007), Alexander and Peterson (2008), and Diether, Lee, and Werner (2009).

This paper is organized as follows. Section 2 describes short-sale price tests in the U.S. equity markets, how they can affect firms' use of discretionary accruals, and related research. Section 3 describes the data. Sections 4 and 5 report tests of the effect of the Regulation SHO's pilot program on firms' use of discretionary accruals and on the discovery of firms' financial misconduct, and Section 6 reports on tests that examine whether the pilot program coincided with an increase in the efficiency of pilot firms' stock prices. Section 7 concludes.

2. Short-sale price tests, its effect on earnings management, and related research

2.1. Short-sale price tests in U.S. equity markets

Short-sale price tests were initially introduced to the U.S. equity markets in 1930s, ostensibly to avoid bear raids by short sellers in declining markets. The NYSE adopted an uptick rule in 1935, which was replaced in 1938 by a stricter SEC rule, Rule 10a-1, also known as the "tick test." The rule mandates that a short sale can only occur at a price above the most recently traded price (plus tick) or at the most recently traded price if that price exceeds the last different price (zero-plus tick).[8] In 1994, the NASD also adopted its own price test ("bid test") under Rule 3350. Rule 3350 requires a short sale to occur at a price one penny above the bid price if the bid is a downtick from the previous bid.[9]

To facilitate research on the effects of short-sale price tests on financial markets, the SEC initiated a pilot program under the Rule 202T of Regulation SHO in July 2004. Under the pilot

[8] Narrow exceptions apply, as specified in SEC's Rule 10a-1, section (e).
[9] Rule 3350 applies to Nasdaq National Market (Nasdaq NM or NNM) securities. Securities traded in the OTC markets, including Nasdaq Small Cap, OTCBB, and OTC Pink Sheets, are exempted. When Nasdaq became a national listed exchange in August 2006, NASD Rule 3350 was replaced by Nasdaq Rule 3350 for Nasdaq Global Market securities (formerly Nasdaq NM securities) traded on Nasdaq, and NASD Rule 5100 for Nasdaq NM securities traded over-the-counter. The Nasdaq switched from fractional pricing to decimal pricing over the interval of March 12, 2001 – April 9, 2001. Prior to decimalization, Rule 3350 required a short sale to occur at a price $1/8^{th}$ dollar (if before June 2, 1997) or $1/16^{th}$ dollar (if after June 2, 1997) above the bid.

program, every third stock in the Russell 3000 index ranked by trading volume was selected as a pilot stock. From May 2, 2005 to August 6, 2007, pilot stocks were exempted from short-sale price tests. Subsequent to the pilot program, on July 6, 2007, the SEC eliminated short-sale price tests for all exchange-listed stocks.

The decision to eliminate all short-sale price tests prompted a huge backlash from managers and politicians. The former state banking superintendent of New York argued that the SEC's repeal of the price tests added to market volatility, especially in down markets.[10] The *Wall Street Journal* argued that the SEC's Office of Economic Analysis (2007) was too biased to evaluate the short-sale price tests fairly.[11] Wachtell, Lipton, Rosen & Katz, a well-known law firm, argued that the uptick rule should be reinstated immediately, and three members of Congress introduced a bill (H.R. 6517) to require the SEC to reinstate the uptick rule. Presidential candidate Sen. John McCain blamed the SEC for the recent financial turmoil by "turning our markets into a casino," in part because of the increased prospect of short sales, and called for the SEC's chairman to be dismissed. In response to this pressure, the SEC partially reversed course and restored a modified uptick rule on February 24, 2010. Under the new rule, price tests are triggered when a security's price declines by 10% or more from the previous day's closing price. This policy reversal drew sharp criticism itself, this time from hedge funds and short sellers.[12]

2.2. The impact of the pilot program on earnings management

[10] Gretchen Morgenson, "Why the roller coaster seems wilder", *The New York Times*, August 26, 2007, Page 31.
[11] "There's a better way to prevent bear raids", *The Wall Street Journal*, November 18, 2008, Page A19.
[12] See "Hedge Funds Slam Short-Sale Rule," *The New York Times*, February 25, 2010.

The strong public reactions to changes in the uptick rule indicate that the rule is economically important to investors, managers, and politicians. Consistent with practitioners' perception, most prior research indicates that short-sale price tests impose meaningful constraints on short selling.[13] In this paper we examine whether changes in the cost of short selling affect firms' financial reporting decisions. We draw from prior studies to construct hypotheses about a manager's choice to engage in earnings management. The Internet Appendix reports a simple model that generates the hypotheses that are described intuitively here.

We begin by noting that previous research indicates that executives have incentives to distort their firms' reported financial performance to bolster their compensation, gains through stock sales, job security, operational flexibility, or control.[14] This implies that managers can earn a personal benefit from managing earnings to inflate the stock price. Prior research also demonstrates that an increase in the prospect of short selling facilitates the flow of unfavorable information into stock prices, increases price efficiency, and dampens the price inflation that motivates managers to manipulate earnings in the first place (e.g., see Miller, 1977; Boehmer, Jones, and Zhang, 2013; Boehmer and Wu, 2013; Karpoff and Lou, 2010). These findings indicate that the manager's benefits of manipulating earnings decrease with the prospect of short selling because short sellers' activities partially offset the price inflation that motivates managers to manipulate earnings in the first place.

Regulation SHO's pilot program, which eliminated short-sale price tests for the pilot stocks, represents an exogenously imposed reduction in the cost of short selling and an increase

[13] See, for examples, McCormick and Reilly (1996), Angel (1997), Alexander and Peterson (1999, 2008), the SEC's Office of Economic Analysis (2007), and Diether, Lee, and Werner (2009). For a contradictory finding, however, see Ferri, Christophe, and Angel (2004).

[14] For evidence, see Bergstresser and Philippon (2006), Burns and Kedia (2006), Efendi, Srivastava, and Swanson (2007), Cornett, Marcus, and Tehranian (2008), Beneish and Vargus (2002), DeFond and Park (1997), Ahmed, Lobo, and Zhou (2006), DeFond and Jiambalvo (1994), and Sweeney (1994).

in the prospect of short selling among these stocks. We therefore expect the pilot program to decrease managers' expected benefits from earnings manipulation among the pilot firms relative to the non-pilot firms.

Although earnings management conveys benefits to managers, managers cannot manipulate earnings with impunity. This is because aggressive earnings management is associated with an increased likelihood of forced CEO turnover (see Hazarika, Karpoff, and Nahata, 2012; Karpoff, Lee, and Martin 2008). Previous findings also indicate that short sellers help to monitor managers' reporting behavior and uncover aggressive earnings management (see, e.g., Efendi, Kinney, and Swanson, 2005; Desai, Krishnamurthy, and Venkataraman, 2006; and Karpoff and Lou, 2010). These results indicate that, for any given level of earnings management, managers' potential cost increases with a reduction in the cost of short selling and an increase in the prospect of short sellers' scrutiny.

These effects on a manager's choice to manage earnings are illustrated in Figure 1. MB_0 and MC_0 represent the managers' marginal benefit and marginal cost of managing earnings before the initiation of the pilot program. In drawing these curves with their normal slopes, we assume that the benefits from artificial stock price inflation increase at a decreasing rate in the level of earnings management, while the costs from the prospect of being discovered increase at an increasing rate. The pre-program optimum amount of earnings management is EM_0. As discussed above, the effect of the pilot program is to decrease marginal benefits and increase marginal costs for any given level of earnings management. We represent these changes as shifts in the marginal benefits and marginal costs of earnings management to MB_1 and MC_1. The manager adjusts endogenously by choosing a new, lower level of earnings management, EM_1. This adjustment among pilot firms implies Hypothesis 1:

Hypothesis 1: Earnings management in the pilot firms will decrease relative to earnings management in the non-pilot firms during the pilot program.

Previous research also indicates that the cost of short selling is negatively related to a firm's level of institutional ownership because it is relatively easy to borrow shares from institutional owners to establish short positions (e.g., Nagel, 2005; Chen, Hong, and Stein, 2002; Hirshleifer, Teoh, and Yu, 2011). Short selling in firms with low levels of institutional ownership, in contrast, can be extremely costly simply because there are few shares to borrow. Consistent with this argument, institutional ownership is positively related to short selling in empirical tests (e.g., Asquith, Pathak, and Ritter, 2005; Karpoff and Lou, 2010). These findings imply that the effects of a change in the cost of short selling should be relatively large in firms with high institutional ownership: the manager's marginal benefit will decrease more, and her marginal cost will increase more, because the prospect of short selling will change more for these firms. Stated differently, the removal of the short-sale price tests will have little effect in firms with low institutional ownership because the cost of short selling is likely to remain high because there are relatively few shares for short sellers to borrow. This leads to our second hypothesis:

Hypothesis 2: The magnitude of the impact of the pilot program on pilot firms' discretionary accruals is positively related to the firm's level of institutional ownership.

A primary source of benefit for managers to manipulate earnings is the prospect of a higher value of the manager's equity-based compensation or share holdings in the firm (e.g., Bergstresser and Philippon, 2006; Burns and Kedia, 2006). Where this benefit is small to begin with, the incremental impact of the pilot program on managers' expected benefits will be relatively small. We therefore should expect the pilot program to have larger effects on earnings management in firms where managers' wealth is closely tied to stock prices. This implies our third hypothesis.

> *Hypothesis 3:* The magnitude of the impact of the pilot program on pilot firms' discretionary accruals is positively related to the sensitivity of the managers' compensation to the firm's stock price.

The tests below examine these three hypotheses. We also examine the impact of the pilot program on the rate at which short selling helps to uncover financial misrepresentation that leads to enforcement action by the SEC. Finally, we examine whether greater short selling, or its prospect, corresponds to an increase in the efficiency of the pilot firms' stock prices during the pilot program.

2.3. Related research

Our investigation is related to the small but growing literature that exploits changes in short sale regulations to examine the economic implications of short selling. Autore, Billingsley, and Kovacs (2011), Frino, Lecce, and Lepone (2011), and Boehmer, Jones, and Zhang (2013) examine the impact of a widespread ban on short selling in U.S. equity markets in 2008, and Beber and Pagano (2013) examine the impacts of short selling bans around the world. These studies conclude that the bans decreased various measures of market quality.

Using Regulation SHO's Rule 202T pilot program, Alexander and Peterson (2008) find that order execution and market quality improved for the pilot stocks during the pilot program. Diether, Lee, and Werner (2009) and the SEC's Office of Economic Analysis (2007) show that pilot stocks listed on both the NYSE and Nasdaq experienced a significant increase in short-sale trades and short sales-to-share volume ratio during the term of the pilot program. The former also shows that NYSE-listed pilot stocks experienced a higher level of order-splitting, suggesting that short sellers applied more active trading strategies. Other papers relate the pilot program to firm outcomes. Grullon, Michenaud, and Weston (2013), for example, examine the effect of the pilot program on pilot firms' stock prices, equity issuance, and investment. Kecskés, Mansi, and Zhang (2013) study bond yields, De Angelis, Grullon, and Michenaud (2013) study equity incentives, and He and Tian (2014) study corporate innovation.

We use the controlled experiment created by the pilot program to examine the effect of short selling costs on firms' earnings management decisions. This experiment is well suited for our research question, as it facilitates DiD comparisons of pilot vs. control firms' discretionary accruals before, during, and after the pilot program. The DiD tests allow us to control for time trends that may be common to both the pilot and control firms, and mitigates concerns about reverse causality or omitted variables (because the SEC assigned pilot stocks arbitrarily). This experimental design is thus superior to a blanket ban of short selling that applies to the entire cross-section of firms because the latter can be muddied by possible confounding events. For example, changes in accruals following the blanket ban on short selling during the recent financial crisis could be associated with economy-wide changes in investment opportunities rather than the changes in short selling regulations.

A contemporaneous paper by Massa, Zhang, and Zhang (MZZ, 2013a) also investigates the effects of short selling on firms' earnings management. Whereas we use the pilot program's removal of short-sale price tests in U.S. markets to identify our tests, MZZ focus on 33 international markets and use the amount of shares available to be lent for short sale to measure short-selling potential. To control for potential endogeneity, MZZ use ETF ownership to instrument for short-selling potential. Like us, MZZ also infer that short selling plays a disciplinary role in deterring firms' opportunistic reporting behavior.

3. Data

3.1. Sample

On July 28, 2004, the SEC issued its first pilot order (Securities Exchange Act Release No. 50104) and published a list of 986 stocks that would trade without being subject to any price tests during the term of the pilot program (available at http://www.sec.gov/rules/other/34-50104.htm). To create this list, the SEC first excluded stocks that were not previously subject to price tests (i.e., not listed on NYSE, AMEX, or Nasdaq NM) and stocks that went public or had spin-offs after April 30, 2004. The remaining 2004 Russell 3000 index members were then sorted by each stock's average daily dollar volume computed from June 2003 through May 2004 within each of the three listing markets. Finally, every third stock (beginning with the second one) within each listing market was designated as a pilot stock.

To construct our sample, we start with the 2004 Russell 3000 index and also exclude stocks that were not listed on the NYSE, AMEX, or Nasdaq NM, and stocks that went public or had spin-offs after April 30, 2004. Based on the SEC's pilot order, we identify an initial sample of 986 pilot stocks and 1,966 non-pilot stocks. An examination of the exchange distribution of these stocks shows that both the pilot and non-pilot groups are representative of the Russell 3000 index, confirming the statistics reported by the SEC. Specifically, of the 986 pilot stocks, 49.9% (492) are listed on the NYSE, 47.9% (472) on the Nasdaq NM, and 2.2% (22) on the AMEX. The exchange distribution of non-pilot stocks is very similar, with 50% (982) listed on the NYSE, 48% (944) on the Nasdaq NM, and 2% (40) on the AMEX.

In our tests we delete firms in the financial services (SIC 6000–6999) and utilities industries (SIC 4900–4949) because disclosure requirements and accounting rules are significantly different for these regulated industries. A further complication with financial stocks is the 2008 short-sale ban imposed on this sector. We require data from the *Compustat Industrial Annual Files* to construct earnings management proxies and control variables. In most tests, we require all firms to have data to calculate firm characteristics across the entire sample period, 2001–2003 and 2005–2010. The resulting balanced panel sample consists of 388 pilot firms and 709 control firms. If we relax this requirement, our unbalanced panel sample contains 741-782 pilot firms and 1,504-1,610 control firms in the year immediately before the announcement of the pilot program (i.e., 2003), depending on the data availability to calculate a given firm characteristic. We emphasize the results from the balanced panel sample, but also report results for the unbalanced sample. Throughout, the results are similar using either sample.

3.2. Key test variables

We create an indicator variable *PILOT* to denote firms with pilot stocks (pilot firms). Specifically, *PILOT* equals one if a firm's stock is designated as a pilot stock under Regulation SHO's pilot program and zero otherwise. Pilot firms constitute the treatment sample and non-pilot firms serve as the control sample. We also construct three variables to indicate time periods: *PRE* equals one if a firm-year's fiscal end falls between January 1, 2001 and December 31, 2003 and zero otherwise; *DURING* equals one if a firm-year's fiscal end falls between January 1, 2005 and December 31, 2007 and zero otherwise; and *POST* equals one if a firm-year's fiscal end falls between January 1, 2008 and December 31, 2010 and zero otherwise. In our primary DiD tests, we omit year 2004 because the identity of the pilot and non-pilot stocks was made public in July 2004, so it is not clear whether year 2004 should be classified as pre- or during-pilot period. In Table IA1 of the Internet Appendix, we report tests that indicate the results reported here are not substantially affected if we include the entire year of 2004 in the *PRE* or Q1-Q3 of 2004 in the *PRE* and Q4 in the *DURING* period.

3.3. Measures of earnings management

We proxy for earnings management using performance-matched discretionary accrual measure of Kothari, Leone, and Wasley (2005). To construct this measure, we first estimate the following cross-sectional model within each fiscal year and Fama-French 48 industry,

$$\frac{TA_{i,t+1}}{ASSET_{i,t}} = \beta_0 + \beta_1 \frac{1}{ASSET_{i,t}} + \beta_2 \frac{\Delta REV_{i,t+1}}{ASSET_{i,t}} + \beta_3 \frac{PPE_{i,t+1}}{ASSET_{i,t}} + \varepsilon_{i,t+1} \qquad (1.1)$$

where i indexes firms and t indexes fiscal years. Total accruals $TA_{i,t+1}$ are defined as earnings before extraordinary items and discontinued operations minus operating cash flows for fiscal year $t+1$, $ASSET_{i,t}$ the total assets at the end of year t, $\Delta REV_{i,t+1}$ the change in sales revenue from year t to $t+1$, and $PPE_{i,t+1}$ the gross value of property, plant and equipment at the end of year $t+1$. We require at least 10 observations to perform each cross-sectional estimation.

Next, we use the following model and the estimated coefficients from Eq. (1.1) to compute the fitted normal accruals $NA_{i,t+1}$,

$$NA_{i,t+1} = \widehat{\beta_0} + \widehat{\beta_1}\frac{1}{ASSET_{i,t}} + \widehat{\beta_2}\frac{(\Delta REV_{i,t+1} - \Delta AR_{i,t+1})}{ASSET_{i,t}} + \widehat{\beta_3}\frac{PPE_{i,t+1}}{ASSET_{i,t}} \quad (1.2)$$

Following prior studies, the change in accounts receivable is subtracted from the change in sales revenue as credit sales might also provide potential opportunity for accounting distortion. After obtaining the fitted normal accruals $NA_{i,t+1}$ from Eq. (1.2), we then calculate firm-year specific discretionary accruals as $DA_{i,t+1} = (TA_{i,t+1} / ASSET_{i,t}) - NA_{i,t+1}$.

Finally, we adjust the estimated discretionary accruals for performance. We match each sample firm with another firm from the same fiscal year-industry that has the closest return-on-assets ratio as the given sample firm. The performance-matched discretionary accruals, denoted as *PM_DA*, are then calculated as the firm-specific discretionary accruals minus the discretionary accruals of the matched firm. *PM_DA* is signed and constructed to be positively related to income-increasing earnings management.[15]

3.4. Firm characteristics

Following Grullon, Michenaud, and Weston (2013), we compare the pilot and non-pilot firms' characteristics in the fiscal year immediately before the announcement of the pilot program, 2003. Table 1, Panel A reports on the balanced panel sample, in which we require a firm to have financial data available to calculate firm characteristics and accrual measures in all

[15] We create three additional performance-matched discretionary accrual measures by removing the intercept term from Eq. (1.1) and/or replacing $\frac{\Delta REV_{i,t+1}}{ASSET_{i,t}}$ with $\frac{(\Delta REV_{i,t+1} - \Delta AR_{i,t+1})}{ASSET_{i,t}}$ in Eq. (1.1). The results using these alternative measures are reported in Table IA2 of the Internet Appendix and are consistent with those reported in the paper.

years of the sample period. The mean book value of assets in both groups is $3.7 billion. The two groups also exhibit similar mean and median values of the market-to-book ratio, one-year growth in assets, capital expenditures-to-assets ratio, R&D expenditures-to-assets ratio, annual return-on-assets ratio, cash flow-to-assets ratio, leverage, and the levels of cash and dividends (both as a percentage of total assets). In none of these comparisons is the difference statistically significant, which supports our contention that Regulation SHO's pilot program is a well-controlled experiment that is suitable for examining the effects of short-sale constraints.

Panel B of Table 1 reports similar comparisons for the larger unbalanced panel sample. Firms in the unbalanced panel sample are slightly smaller than the firms included in the balanced panel sample, with assets averaging $2.9 billion versus $3.7 billion. As in Panel A, the pilot and non-pilot firms in the unbalanced panel sample are similar to each other in the other financial characteristics we examine. The sole exception is that the median capital expenditure by pilot firms is slightly higher than that for the control firms.

4. The effect of Regulation SHO's pilot program on discretionary accruals

4.1. Tests of Hypothesis 1

4.1.1 Univariate difference-in-differences tests

Table 2 reports the results of the univariate DiD tests examining *Hypothesis 1*. Panel A reports on the balanced panel sample defined in Section 3.1. The mean value of our main measure of earnings management *PM_DA* during the three-year period before the pilot program (2001–2003), is -0.004 for both the pilot and control firms. The t-statistic for the difference in means (i.e., the cross-sectional estimator -0.001) is -0.03, and the Wilcoxon z-statistic for the difference in medians is 0.77. During the three-year period of the pilot program (2005-2007), the mean value of *PM_DA* decreases to -0.014 for the pilot firms while it remains at -0.004 for the control firms. The mean difference becomes -0.011 (t-statistic=-2.09) and the median difference is -0.009 (Wilcoxon z-statistic=-2.23), both significant at the 5% level. For the three-year period after the pilot program (2008-2010), *PM_DA* increases for the pilot firms to a mean of zero, while it changes slightly for the control firms to -0.003. The mean difference in the post-pilot period is 0.004 (t-statistic=0.69), and the median difference is 0.001 (Wilcoxon z-statistic=0.66), both statistically insignificant. The bottom-left cell of Table 2 Panel A reports the time-series estimators, which track the change in *PM_DA* within each group of firms across three different periods. The second column shows that the average *PM_DA* drops by -0.011 (significant at the 5% level) for the pilot firms from pre- to during- the pilot program, but bounces back by 0.013 (significant at the 1% level) after the program ends. Consistent with this reverting pattern, the time-series estimator comparing pilot firms' average *PM_DA* from pre- to post-the pilot program is 0.003 and insignificant. In contrast, the estimators in the fourth column are never significant, suggesting that non-pilot firms' *PM_DAs* do not change much over time.

The bottom-right cell of Table 2 Panel A reports on the DiD estimators. The mean DiD estimator for *PM_DA* from before to during the pilot program is -0.011 with a t-statistic of -1.67. This difference is statistically significant only at the 10% level. However, the results from other tests reported below, including multivariate DiD tests and the results from the unbalanced panel, are significant at lower levels. Also, the DiD estimator that tracks *PM_DA* from during to after the pilot program is 0.013 with a t-statistic of 2.06. Further, the DiD estimator that compares *PM_DA* pre-program to post-program is statistically insignificant with a t-statistic of 0.32. The last two DiD estimators demonstrate that the effect of the pilot program on discretionary accruals reverses when the program ends – an important check on the internal validity of the DiD test.

We also plot these univariate results in Figure 2 for a better illustration of the pattern in discretionary accruals. As the figure shows, the control firms' discretionary accruals do not change much over the entire sample period. The pilot firms' discretionary accruals are similar to those of the control firms before the pilot program, decrease significantly during the program, and then revert to levels that are similar to those of the control firms after the program.

Panel B of Table 2 reports on the changes in *PM_DA* using data from the unbalanced panel sample in which we do not require firms to have financial data available for all years of the sample period. The results are similar to those from the balanced panel sample even though we are only able to calculate the cross-sectional estimators given the unbalance sample.

4.1.2. Multivariate difference-in-differences tests

In this section we extend the DiD test using multivariate regressions. We retain firm-year observations for both pilot and non-pilot firms for the nine-year window (2001–2003 and 2005–2010) surrounding Regulation SHO's pilot program and estimate the following model:

$$PM\_DA_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \beta_3 PILOT_i + \beta_4 DURING_t + \beta_5 POST_t + \varepsilon_{i,t} \quad (2)$$

The variables are as defined in Section 3.2 and 3.3. The benchmark period consists of the three years before the pilot program (2001–2003). Again, year 2004 is omitted from these tests because the identity of the pilot and non-pilot stocks was announced midway through 2004. The regression results estimating Eq. (2) are reported in Column (1) of Table 3. The coefficients of interest are the two DiD estimators, β_1 and β_2. The coefficient on $PILOT_i \times DURING_t$, β_1, is negative and significant at the 5% level. The magnitude of β_1 is consistent with the univariate DiD results reported in Table 2 and indicates that *PM_DA* (i.e., discretionary accruals as a percentage of total assets) is one percentage point lower for the treatment group than for the control group during the three-year period of the pilot program compared to the three-year period before the program. The coefficient on $PILOT_i \times POST_t$, β_2, is insignificant, which once again demonstrates the reverting pattern as the difference between the pilot and non-pilot firms' discretionary accruals after the pilot program is not statistically different from that before the program. The coefficient on $PILOT_i$, β_3, is also insignificant, consistent with pilot firms and non-pilot firms exhibiting similar levels of discretionary accruals before the pilot program. Consistent with prior research, the regression R^2s are low, indicating that most of the cross-sectional differences in discretionary accruals are due to unmodeled factors.

In Columns (2), we augment Eq. (2) by including four controls that have been shown to affect a firm's level of discretionary accruals (e.g., Kothari, Leone, and Wasley, 2005; Zang, 2012): the natural logarithm of total assets (*SIZE*), market-to-book ratio (*MB*), return-on-assets (*ROA*), and leverage (*LEV*). In Column (3), we further include eight year fixed effects from 2002-2003 and from 2005-2010, but omit *DURING* and *POST* as well as the fixed effect for 2001 (the base year) to avoid multicollinearity. The results are similar when we include these additional controls.

4.2. Tests of Hypotheses 2 and 3

In this section we examine Hypotheses 2 and 3. Hypothesis 2 holds that the effects of the pilot program on discretionary accruals will be most pronounced in firms with high institutional ownership. To examine this hypothesis, we partition the balanced panel sample of pilot and non-pilot firms into two subsamples based on whether a firm's institutional ownership in the fourth quarter of 2003 is above the sample median. Institutional ownership is retrieved from Thomson's CDA/Spectrum database (form 13F) and aggregated on the firm level. We define institutional ownership (*IO*) as the shares held by all institutions divided by the total shares outstanding at the end of the quarter from CRSP monthly files (adjusted for stock splits and other distributions), following Asquith, Pathak, and Ritter (2005). Pilot firms constitute 33.9% of the subsample with high *IO* and 37.0% of the subsample with low *IO*.

The left panel of Table 4 reports the results from re-estimating Eq. (2) using these two subsamples. Consistent with Hypothesis 2, the impact of Regulation SHO's pilot program on discretionary accruals is much more pronounced among firms with high levels of *IO*. In fact, the evidence in Columns (3)-(4) indicates that there is no significant effect among firms with below-median levels of *IO*. Among firms with high levels of *IO*, the magnitude of the effect is nearly twice that reported in Table 3 for the overall sample. The differences in the magnitudes of the coefficients are statistically significant at the 5% level.

We note that the coefficient on *PILOT* is positive and significant in the subsample of firms with high *IO*, and negative and significant among firms with low *IO*. This suggests that the pilot firms and non-pilot firms exhibit different levels of discretionary accruals within the two subsamples before the pilot program. Given that pilot firms have larger discretionary accruals to begin with in the subsample of firms with high *IO*, one concern is that the larger drop we observe for the pilot firms during the pilot period merely captures a reversal of previous periods' earnings management, as reflected in the higher discretionary accruals. However, the magnitude of the coefficients on *PILOT×DURING* (-0.017 in Column (1) and -0.018 in Column (2)) is consistently much larger than that of the coefficients on *PILOT* (0.008 in both Columns (1) and

(2)). This indicates that there is a separate effect on the pilot firms that is over and above any possible reversal of discretionary accruals.

Previous evidence indicates that the cost of short selling is negatively related to institutional ownership because institutional owners increase the supply of shares that can be borrowed to implement a short sale. Our finding indicates that the impact of Regulation SHO's pilot program on short selling costs is largest among firms for which price tests are the binding constraints on short selling.[16]

Hypothesis 3 holds that the effects of the pilot program on discretionary accruals will be most pronounced in firms whose managers' wealth is highly sensitive to the stock price. To test *Hypothesis 3*, we partition the balanced panel sample of pilot and non-pilot firms into two subsamples based on whether the firm CEO's scaled wealth-performance sensitivity (*WPS*) in 2003 is above the sample median. The *WPS* measure, which aims to capture the sensitivity of the CEO's wealth to the stock price, is computed as the dollar change in the CEO's wealth for a 100 percentage point change in the stock price, scaled by annual pay (see Edmans, Gabaix, and Landier, 2009). The *WPS* measure is calculated using the ExecuComp database. Since ExecuComp covers S&P 1500 firms, this reduces our sample size by 29%. In this reduced sample, pilot firms constitute 39.8% of firms for which *WPS* is above the median, and 35.5% of the firms for which *WPS* is at or below the median.

The results re-estimating Eq. (2) using the two subsamples partitioned by *WPS* are reported in the right panel of Table 4. The coefficient estimate on *PILOT×DURING* is

[16] An alternative interpretation is that the pilot program encouraged institutional investors to pressure managers to abstain from engaging in earnings manipulation. It is not clear exactly how the pilot program would have such an effect unless it was through the threat of greater short selling. Even under this interpretation, the changes were prompted by the exogenous reduction in short-sale constraints for pilot firms through Regulation SHO's pilot program.

significant and negative only in the subsample of firms with above median *WPS*, with or without control variables included. That is, *PM_DA* is predictively lower for pilot firms during the pilot program, but only in the subsample of firms with above median *WPS*. This result is consistent with the view that short selling works to discipline earnings management that is motivated by managers' efforts to boost their equity compensation and/or the value of their equity holdings.

Overall, the results in Table 4 indicate that the disciplinary effect of short sellers on earnings management is most pronounced among firms that have high levels of institutional ownership and among firms where managers' wealth is closely tied to the stock price.

4.3. Other potential explanations

So far, our results indicate that an increase in the prospect of short selling due to the removal of short-sale price tests reduces firms' use of discretionary accruals. In this section, we evaluate several alternative explanations that might account for this finding.

4.3.1. Growth and investment

Prior research shows that a firm's discretionary accruals are highly correlated with its growth (e.g., Fairfield, Whisenant, and Yohn, 2003; Zhang, 2007; Wu, Zhang, and Zhang, 2010). Grullon, Michenaud, and Weston (2013) document that pilot firms significantly reduced their investment during the pilot program. So it is possible that our finding on discretionary accruals is driven by changes in the difference between the pilot and non-pilot firms' investment surrounding the pilot program.

To address this possibility, we consider three types of controls. First, the *PM_DA* measure of Kothari, Leone, and Wasley (2005), by matching firms on performance, is specifically designed to remove variation in accruals due to the changes in performance such as those caused by a reduction in investment.[17] Second, the market-to-book ratio, which is included as a control in Table 3, at least partly controls for firms' growth opportunities. Third, we re-estimate Eq. (2) controlling for R&D expenditures (*R&D*) and capital expenditures (*CAPEX*), both scaled by lagged total assets. (The results are similar if we measure capital expenditures as the annual increase in gross property, plant, and equipment from the balance sheet.)

The results including *R&D* and *CAPEX* as additional controls are reported in Table 5. In Columns (1), we include *R&D* and *CAPEX* in the regressions separately. In Columns (2), we sum the two variables to derive a firm's total investment *INVESTMENT*. As shown, the coefficients on the two DiD estimators, *PILOT×DURING* and *PILOT×POST*, are barely affected by the inclusion of these control variables. In Columns (3)-(4), we further include squared terms of investment variables in the multivariate DiD tests, to account for the possibility that the effect of investment on accruals may be non-linear. The results remain similar.

As an additional probe for any investment effect on accruals, we modified the Jones model to include a potential investment effect on the estimation of accruals. Specifically, we add the market-to-book ratio to both equations (1.1) and (1.2) when calculating the performance-matched discretionary accruals so total accruals are modeled as a function of the market-to-book ratio in addition to change in revenues and PPE (both scaled by total assets). The multivariate DiD tests with this modified measure (labled as *PM_DA_MBadj*) as the dependent variable yield results that are similar to those using *PM_DA*, and are tabulated in Column (5) of Table 5.

---

[17] Indeed, Kothari, Leone, and Wasley (2005, pg. 165) explain that, "[O]ur motivation for controlling for performance stems from the simple model of earnings, cash flows, and accruals in Dechow et al. (1998). This model shows that working capital accruals increase in forecasted sales growth and earnings because of a firm's investment in working capital to support the growth in sales. Therefore, if performance exhibits momentum or mean reversion (i.e., performance deviates from a random walk), then expected accruals would be nonzero. Firms with high growth opportunities often exhibit persistent growth patterns (i.e., earnings momentum)."

In an additional attempt to identify any influence of investment on our results, we examine changes in investment variables surrounding the pilot program for the two groups of firms. If discretionary accruals indeed reflect only growth, investment should follow a pattern around the pilot program that is similar to that in *PM_DA*. In Table IA3 of the Internet Appendix, we re-estimate Eq. (2) replacing the dependent variable *PM_DA* with *CAPEX* in Column (1), and *INVESTMENT* in Column (2). The controls are taken from Edmans, Fang, and Lewellen (2014). Our results are consistent with Grullon, Michenaud, and Weston (2013)'s finding that capital expenditures decreased for the pilot firms relative to the non-pilot firms during the pilot program. However, pilot and control firms' capital expenditures do not appear to converge when the pilot program ends, as *PILOT×POST* is significantly negative. We also find no evidence that pilot firms' overall *INVESTMENT* decreased during the pilot program. Overall, the results in this section do not support the contention that the pattern in discretionary accruals that we document is driven by changes in firms' investment levels.

4.3.2. Equity issuance

Friedlan (1994) and Teoh, Welch, and Wong (1998a, 1998b) show that firms with high incentives to issue equity are more likely to engage in accruals management, and Grullon, Michenaud, and Weston (2013) document that pilot firms significantly reduced their equity issuance during the pilot program. These results suggest that our findings regarding pilot firms' discretionary accruals could be attributable to the subset of these firms that issued equity during our sample period.

To examine the possible influence of equity issues on discretionary accruals, we partition sample firms based on whether a firm issued equity during a given year, as recorded in the

Thomson Reuters Securities Data Company (SDC) Platinum database. Firms that did not issue equity during a given year are designated "*Non-Equity Issuers*". The results are reported in Table 6. *Non-Equity Issuers* account for the majority of firm-years in the sample, and the results for this subsample are similar to those in Column (2) of Table 3 for the overall sample. Among firms that issued equity at least once during the year (labeled as "*Equity Issuers*"), the coefficient on *PILOT×DURING* is statistically insignificant. These results indicate that the effect of the pilot program on discretionary accruals is widespread and is not limited to firms that issue equity.

4.3.3. Market attention

Another potential explanation for our findings is that the pilot program brought widespread attention to the pilot firms. These firms could have reduced their use of discretionary accruals in response to this overall market attention rather than any attention from short sellers in particular.

To examine this market attention hypothesis, we construct three measures of investors' awareness of and attention to individual companies around the pilot program. Our first measure follows Da, Engelberg, and Gao (2011) and is based on the frequency with which a stock is searched on Google. This *Search Volume Index* (*SVI*) arguably reflects retail investors' awareness of and interest in a particular firm. Our second measure of market attention is the number of earnings forecasts issued by sell-side financial analysts. Sell-side financial analysts work for brokerage firms and their research is typically funded by trading commissions paid by institutions. We conjecture that, if a pilot firm experiences an increase in attention from institutional investors, the institutions' demand for information will prompt analysts to exert more effort in collecting information and issuing earnings forecasts, thereby generating more forecasts (e.g., see Jacob, Lys, and Neale, 1999). As a third measure of market attention we use total trading volume in the stock. Trading volume can also increase as a result of a decrease in the cost of short selling, but for this measure we will focus on the period between the identification of the pilot firms and the implementation of the program. Presumably, any increase in trading volume before the program was implemented is more likely to reflect an increase in investors' awareness of the firm, whereas an increase in trading volume after the pilot program is implemented could reflect lower trading costs.

If the announcement of the pilot program led to an increase in market attention, the effect should occur following the date of the SEC's first pilot order when the pilot firms were first identified on July 28, 2004. We begin our test by restricting the focus to year 2004 and calculating the DiD estimator for each of the three attention measures from the pre-announcement period (January 1, 2004 to July 27, 2004) to the post-announcement period (July 28, 2004 to December 31, 2004). As shown in Panel A of Table IA4 of the Internet Appendix, none of the DiD estimators is statistically significant, indicating that the announcement of the pilot program did not substantially raise market attention for the pilot firms compared to the non-pilot firms.

We then repeat the univariate DiD tests over our main sample period (2001-2003, 2005-2010). Table IA4, Panel B reports the results. As shown, most of the DiD estimators for changes in market attention remain statistically insignificant. There is only one statistically significant result, regarding the change in the number of analyst earnings forecasts from the pilot period to after the pilot period. This result arises because the number of analyst forecasts increases for both groups of firms in the post-program period, but the increase among non-pilot firms is particularly large. This result suggests that, if the pilot program focused greater attention on firms, the focus was greater for non-pilot firms than for pilot firms.

Finally, we re-estimate Eq. (2) including two of our attention measures, the number of analyst forecasts and total trading volume as additional controls. We cannot include *SVI* as an additional control because *SVI* only dates back to year 2004 while our sample period starts from 2001. The results, reported in Table IA5, remain similar to those reported in the paper.

Market attention is an elusive concept that is difficult to measure. But overall, the various tests we conduct to examine the market attention hypothesis indicate that we cannot attribute the patterns of changes in firms' discretionary accruals to an increase in the market's overall attention paid to the pilot firms during the pilot program.[18]

4.3.4. Industry controls and secular changes

Our measures of discretionary accruals are industry adjusted. Thus, our finding that pilot firms' discretionary accruals dropped during the pilot program could reflect a decrease in pilot firms' accruals or an increase in the accruals of the non-pilot firms in their industries. Beginning in July 2007 during the post-program period, the SEC repealed price tests on short sales for all firms (and restored them in a modified version in early 2010). So it is possible that this widespread reduction in short selling costs led to a general decrease in earnings management across non-pilot firms, suggesting that the reverting pattern we observe reflects a decrease in non-pilot firms' accruals rather than an increase in pilot firms' accruals.

[18] We also note that the reverting pattern we observe for the discretionary accruals at the end of the pilot program is not consistent with an investor awareness or market attention hypothesis. Other studies find that, when investor awareness increases for a particular firm, such attention persists for a prolonged period and does not quickly revert (e.g., see Chen, Noronha, and Singal, 2004).

To examine this possibility, we calculate a time-series estimator that compares non-pilot firms' total accruals from the three years during the pilot program to the three years after the program. As shown in Panel A of Table IA 6 of the Internet Appendix, the time-series estimator is negative and significant at the 1% level, indicating that non-pilot firms decreased their accruals after the pilot program. It is important to note that total accruals are more likely to capture investment and growth than performance-matched discretionary accruals. To control for this investment bias, we conduct a multivariate analysis for the subsample of control firms for the six years during and after the pilot program, regressing total accruals on the time dummy *POST*, firm controls used in our baseline specifications, as well as investment variables. The results are reported in Table IA6 Panel B in the Internet Appendix. As shown, *POST* remains negatively significant. This suggests that the convergence in pilot and non-pilot firms' discretionary accruals after the pilot program reflects, at least in part, a decrease in discretionary accruals among the non-pilot firms.

We note two other changes that likely affected discretionary accruals at both pilot and non-pilot firms. The first is a component of Regulation SHO that restricted the practice of naked short selling by imposing locate and close-out standards starting in January 2005, toward the beginning of the pilot period. This restriction may have raised the cost of selling short for both the pilot and non-pilot firms. The second is the financial crisis that occurred largely after the pilot period ended. For example, firms might have written down asset values during the crisis. The DiD experimental design is uniquely suited to control for such common time trends affecting both groups of firms, which allows us to draw the inference that earnings management at the pilot and non-pilot firms diverged when the cost of short selling in these two groups diverged, and converged only when the cost of short selling in these two groups converged - exactly as predicted by *Hypothesis 1*.

4.4. Does earnings management actually attract short selling?

The results in Section 4.1 and 4.2 are consistent with *Hypothesis 1* that a reduction in the cost of selling short reduces a firm's use of discretionary accruals. This hypothesis builds on the assumption that short sellers are more likely to scrutinize firms with higher levels of earnings management. That is, the *prospect* of short selling increases in the amount of earnings management. Previous evidence indicates that short selling indeed tracks firms' use of discretionary accruals (e.g., Desai, Krishnamurthy, and Venkataraman, 2006; Cao et al., 2006; Karpoff and Lou, 2010; Hirshleifer, Teoh, and Yu, 2011). In this section we provide evidence that this general pattern also holds among the pilot firms during the pilot period.[19]

To do so, we first obtain monthly short interest from the Compustat Supplemental Short Interest Files and data on executed short sales from the SEC's website.[20] We then regress monthly short interest in a given pilot firm on discretionary accruals as well as a list of controls similar to those in Karpoff and Lou (2010), including *SIZE*, *MB*, *MOMENTUM*, and *IO*. Monthly short interest is scaled by the shares outstanding at the end of the month and denoted as *ShortInterest_Shrout*. During the pilot period, pursuant to the SEC's request, each SRO marks the short sale trades and publicly discloses them at the transaction level. We sum up these high frequency short sale trades to obtain the monthly short sales for a given pilot stock. We then

[19] While we expect to see a positive cross-sectional relation between short selling and discretionary accruals among the pilot firms during the pilot period, how the change in short selling in the pilot firms compares to that in the non-pilot firms is unclear. On one hand, the cost of short selling is lower for the pilot firms during the pilot period. On the other hand, *Hypothesis 1* implies that pilot firms will adjust by decreasing their use of discretionary accruals. The net effect on the amount of short selling in the pilot firms, relative to non-pilot firms, is therefore ambiguous.

[20] Note that, the *prospect* of short selling is an ex ante concept. Although short interest and short sales represent our best effort to capture this concept, both variables could contain measurement error.

scale monthly short sales by the total trading volume of the month or the shares outstanding at the end of the month, and denote them as *Shortsales_Vol* and *Shortsales_Shrout*, respectively.

The regression results are reported in Table IA7 of the Internet Appendix. In Panel A, we include the four controls directly. *PM_DA* has significantly positive coefficient estimates in all columns, indicating a positive relation between short interest (or short sales) and discretionary accruals. In Panel B, to account for the possibility that the effect of *SIZE*, *MB*, and *MOMENTUM* on short selling might be non-linear (as shown in Karpoff and Lou, 2010), we include the dummies to indicate middle or the lowest tercile of these controls. The results are barely affected. These results demonstrate that the general finding that short selling is positively related to discretionary accruals also holds among pilot firms during the pilot period.

5. The effect of the pilot program on the discovery of financial misrepresentation

The results in Tables 2 – 6 indicate that the pilot program had a substantial effect on pilot firms' use of discretionary accruals. In this section we investigate an additional avenue by which the pilot program may have had real economic consequences – the discovery of financial misrepresentation. Dyck, Morse, and Zingales (2010) show that short sellers help to uncover financial misconduct, and Karpoff and Lou (2010) find that short selling accelerates the discovery and public revelation of financial misrepresentation. These results imply that the pilot program should facilitate the role of short sellers in uncovering particularly aggressive earnings manipulations among the pilot firms.

It is important to note that a decrease in the cost of short selling has offsetting effects on the likelihood that a pilot firm will be caught misrepresenting its financial statements. On one hand, short sellers may help to uncover any aggressive earnings manipulations that do occur.

But on the other hand, our tests of Hypothesis 1 indicate that the pilot firms are less likely to manage earnings in general during the pilot period. The net effect on the rate at which the pilot firms are found to misrepresent their earnings is therefore ambiguous, ex ante. To isolate the effect of the pilot program on the discovery of earnings manipulation, we examine whether the pilot program affected the probability of discovery for financial misrepresentation that began before the pilot program was announced. For these cases, the choice to manipulate earnings was made before any firms could have adjusted their earnings management decisions because of the pilot program. That is, by narrowing our sample to cases in which the misconduct pre-dates the pilot program, we control for the pilot program's effect on firms' incentives to engage in earnings management.

To test the proposition that pilot firms' misconduct will be discovered relatively quickly, we extend the time-to-discovery tests used by Karpoff and Lou (2010) to examine the effect of short selling on the speed with which financial misrepresentation is uncovered. To identify when firms misrepresent their financial statements, we use the data on the incidence of SEC enforcement actions for financial misrepresentations that are described in Karpoff et al. (2014). These data contain all enforcement actions initiated by the SEC from 1978 through 2011, and include information on the SEC's determination of when the financial misrepresentation began, ended, and was first revealed to the public.

Specifically, we estimate a Cox proportional odds model:

$$DISCOVERED_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i + \beta_3 DURING_t + \beta_4 MONTHS_{i,t} + \beta_5 MONTHS_{i,t}^2 + \beta_6 SIZE_{i,t} + \beta_7 BM_{i,t} + \beta_8 MOMENTUM_{i,t} + \beta_9 SEVERITY_{i,t} + \varepsilon_{i,t} \quad (3)$$

The data consist of firm-month observations for all firms in the Karpoff et al. (2014) database that also: (i) are in the balanced panel sample of pilot and non-pilot firms, (ii) initiated their financial misrepresentation before the pilot stocks were announced in July 2004, and (iii) were publicly revealed to have engaged in financial misrepresentation after the announcement of the pilot stocks in July 2004. We use July 2004 rather than January 2005 as the cutoff date for this test because Eq. (3) is estimated using firm-month (instead of firm-year) observations, allowing us to define the beginning date of the pilot program more finely. We include one time dummy, $DURING_t$, to differentiate during- and post-pilot periods.

The dependent variable, $DISCOVERED_{i,t}$, equals one in the month in which firm *i* is revealed to have engaged in misrepresentation, and zero otherwise. For example, if a firm's violation period is from January 1999 through December 2005 and the misconduct is discovered in December 2005, this firm would have 84 observations (84 months from January 1999 through December 2005). $DISCOVERED_{i,t}$ would equal zero for the first 83 months and 1 for the last month. In Eq. (3), $MONTHS_{i,t}$ equals the number of months from the beginning of the misrepresentation through month *t*. $MONTHS_{i,t}^2$ is included as a control for possible nonlinearity in the relation between the length of the violation period and the probability of discovery. $SIZE_{i,t}$ (log of assets), $BM_{i,t}$ (book-to-market ratio) and $MOMENTUM_{i,t}$ (buy and hold return in the previous 12 months) are measured at the beginning of month *t*. $SEVERITY_{i,t}$, the control for the severity of the violation, is measured as the stock price reaction upon the initial revelation of the misconduct.

Table 7 reports the results. The probability of detection increases with the number of months since the beginning of the violation, as the coefficient on $MONTHS_{i,t}$ is positive. The probability of detection is positively related to firm size and negatively related to the firm's most recent 12-month stock return (*MOMENTUM*) and the *SEVERITY* of the misconduct. This latter result could reflect reverse causality, as the surprise from the revelation of misconduct may increase with the time that elapses until the misconduct is uncovered.

Most important for our purposes, however, are the results concerning pilot firms. The coefficient on $PILOT_i$ is negative and significant, indicating that pilot firms that are engaged in financial misrepresentation are in general less likely to be detected than non-pilot firms. During the pilot program, however, pilot firms with violations that began before the pilot period are more likely to be detected, as the coefficient on $PILOT_i \times DURING_t$ is positive and statistically significant at 5% level. This result indicates that, among firms that initiated a financial reporting violation before the pilot program was announced, the decrease in short selling costs during the pilot program corresponds to an increased probability of detection among the pilot firms.

These results help to complete the overall picture of the effect of short selling on firms' reporting practices. A decrease in the cost of selling short corresponds to a decrease in firms' willingness to manage their earnings. Among firms that began to manipulate earnings aggressively enough to attract SEC sanction before the pilot program was announced, however, the decrease in the cost of short selling increases their likelihood and speed of discovery. In tests that are detailed in the Internet Appendix, we find that these two effects offset each other such that the net effect of the pilot program on the unconditional likelihood that a pilot firm will be caught manipulating its earnings is statistically insignificant.

6. The effect of short selling on price efficiency during the pilot program

The results in Section 4 show that pilot firms decreased their use of discretionary accruals during the pilot period. In this section we examine whether the pilot firms' stock prices correspondingly became more informative of future earnings. Such a connection is implied by previous research that shows that price efficiency improves with earnings quality (e.g., Dechow, Ge, and Schrand, 2010), as well as previous findings that price efficiency improves with short selling (Boehmer and Wu, 2013). We examine whether an increase in price efficiency is apparent in the extent to which future earnings are incorporated in current stock prices and in whether the market reacts efficiently to negative earnings news.

6.1. Coefficient of current returns on future earnings

To examine if the pilot firms' stock prices became more informative about future earnings during the pilot program, we follow Lundholm and Myers (2002) and model the returns-earnings relation using the following equation,

$$R_{i,t} = \beta_0 + \beta_1 X_{i,t-1} + \beta_2 X_{i,t} + \beta_3 X3_{i,t} + \beta_4 R3_{i,t} + \varepsilon_{i,t} \quad . \tag{4}$$

$R_{i,t}$ is firm i's annual buy-and-hold return for year t, measured over the 12-month period ending three months after the end of fiscal year t. $X_{i,t-1}$ and $X_{i,t}$ are the annual earnings for fiscal year $t\text{-}1$ and t, calculated as income before extraordinary items available for common stock in year $t\text{-}1$ and t scaled by the market value of equity three months after the end of fiscal year $t-1$. $X3_{i,t}$ is the aggregated earnings for the three years following fiscal year t. It is calculated as the sum of income before extraordinary items available for common stock in fiscal years $t+1$, $t+2$, and $t+3$, divided by the market value of equity three months after the end of year $t-1$. $R3_{i,t}$ is the buy-and-hold return for the three-year period following year t, starting three months after the end of fiscal year t.

Lundholm and Myers (2002) argue that Eq. (4) can be viewed as a model of the current year's stock return as a function of unexpected current earnings and the cumulative change in expectations about future earnings. Unexpected current earnings are reflected in the levels of past and current earnings, $X_{i,t-1}$ and $X_{i,t}$. Lundholm and Myers (2002) show that, if annual earnings follow a random walk, these two variables will have coefficients of similar magnitude but opposite sign. A white noise process would imply a coefficient on $X_{i,t-1}$ of zero. The cumulative change in expectations about future earnings is measured by the aggregated realized future earnings $X3_{i,t}$. Future stock returns, $R3_{i,t}$, are included to partially control for the unexpected shock to $X3_{i,t}$. We refer to β_3, the coefficient of $X3_{i,t}$, as the coefficient of current returns on future earnings. It captures the degree to which current price reflects future earnings, or in other words, the efficiency of the current price with respect to future earnings. stock price

To assess the effect of pilot program on the coefficient of current returns on future earnings, we augment Eq. (4) by including interactions of pilot-related variables with $X3_{i,t}$,

$$R_{i,t} = \beta_0 + \beta_1 X_{i,t-1} + \beta_2 X_{i,t} + \beta_3 X3_{i,t} + \beta_4 R3_{i,t} + \beta_5 X3_{i,t} \times PILOT_i \times DURING_t + \beta_6 X3_{i,t} \times PILOT_i + \beta_7 X3_{i,t} \times DURING_t + \varepsilon_{i,t} \qquad (5)$$

We estimate Eq. (4) and (5) using a sample of pilot and non-pilot firms that have data to construct all variables for the six-year (rather than nine-year) period surrounding the pilot program (i.e., 2001-2003 and 2005-2007). Including the three-year post-pilot period (2008-2010) would require annual returns and earnings beyond 2012, for which we do not have data.

The results from estimating Eq. (4) are reported in Column (1) of Table 8, and are consistent with those reported in Lundholm and Myers (2002). $X_{i,t-1}$ and $X_{i,t}$ have coefficients of similar magnitude but opposite sign, suggesting that earnings are treated by the market as following a random walk. The significantly positive coefficient on aggregated future earnings $X3_{i,t}$ demonstrates that current return does incorporate information from future earnings. As discussed earlier, although $X3_{i,t}$ is used as a proxy for the change in expectations of future earnings, it also contains unexpected shocks to future earnings (a measurement error). Future return $R3_{i,t}$ is included to remove the effect of this measurement error and exhibits a predicatively negative coefficient.

The results estimating Eq. (5) are presented in Column (2) of Table 8. The coefficients on the first four variables ($X_{i,t-1}$, $X_{i,t}$, $X3_{i,t}$, and $R3_{i,t}$) are similar in sign and magnitude to those in Column (1). More importantly, the coefficient of current returns on future earnings is higher for pilot firms during the three-year period of the pilot program, as evidenced by a positive coefficient on $X3_{i,t} \times PILOT_i \times DURING_t$. That is, pilot stocks' stock prices better reflect their future earnings during the pilot program, consistent with greater price efficiency. In terms of economic significance, the coefficient of current returns on future earnings for pilot firms during the pilot program (0.270+0.158-0.014+0.037=0.451) is nearly 47% higher than that for control firms during the pilot program (0.270+0.037=0.307). The difference between pilot and non-pilot firms is absent before the pilot program, as the coefficient on $X3_{i,t} \times PILOT_i$ is statistically insignificant. In Column (3), we estimate a full model by also including the interaction terms of pilot-related variables with $X_{i,t-1}$, $X_{i,t}$, and $R3_{i,t}$, and the results remain similar.

6.2. Post earnings announcement drift (PEAD)

The PEAD test builds on the notion that, when investors fail to fully capitalize the information in earnings surprises at earnings announcements, returns will drift in the same direction as the earnings surprise (Ball and Brown, 1968; Bernard and Thomas, 1989, 1990). The magnitude of the PEAD can thus be used as a measure of price inefficiency. By its nature, short selling facilitates the incorporation of negative information into stock prices (e.g., see Miller, 1977). We therefore expect a decrease in the cost of short selling to accelerate price discovery particularly after negative earnings news. This implies that the pilot firms' PEADs following negative earnings surprises should be smaller in magnitude than those of non-pilot firms during the pilot program.

To test this hypothesis, we follow Boehmer and Wu (2103)'s methodology and examine firms' returns following earnings surprises during the pilot program. First, we calculate earnings surprise as the firms' actual earnings per share (EPS) minus the latest analyst consensus EPS forecast before the earnings announcement dates (both from I/B/E/S), scaled by the stock price two days before the earnings announcement date. Within each quarter, we sort our sample firms based on their latest earnings surprises into ten deciles, with decile one (D1) consisting of stocks with the most negative earnings surprises and decile ten (D10) consisting of stocks with the most positive earnings surprises. Finally, we define PEAD as the cumulative abnormal return (*CAR*) following the earnings surprise, calculated as the stock's raw return minus the corresponding value-weighted market return over the (+2, +11) window relative to the earnings announcement date.

Table 9 reports the average *PEAD* in each decile for the pilot and non-pilot firms. For the non-pilot firms, PEAD is negative and statistically significant in the bottom deciles, and positive and statistically significant in the top deciles. This result is consistent with prior findings (e.g., Bernard and Thomas, 1989, 1990). Among the pilot firms, however, the PEAD is small in magnitude and statistically insignificant in the lowest earnings surprise decile, D1. The difference in PEAD between the pilot and non-pilot firms is significant at the 5% level for this decile. Furthermore, Decile 1 is the only decile for which the pilot and non-pilot firms' PEADs are significantly different from each other. This result supports the hypothesis that the pilot firms' stock prices more efficiently incorporated negative information about future earnings during the pilot period relative to the non-pilot firms' stock prices.

Tables 8 and 9 document a higher coefficient of current returns on future earnings and the absence of significant post earnings announcement drift (PEAD) following negative earnings surprises for pilot firms during the pilot program. These results are consistent with the view that the pilot program's exogenous reduction in short-sales constraints increased the informativeness of the pilot firms' earnings. We infer that a decrease in the cost of short selling facilitates short selling that is based on earnings-related private information, or that the increased prospect of short selling disciplines opportunistic reporting behavior and improves earnings quality, or both.

7. Conclusion

In this paper we exploit a natural experiment to shed light on an important effect of short selling on firms' financial reporting practices. The SEC's Regulation SHO included a pilot program in which every third stock ranked by trading volume within each exchange was drawn from the Russell 3000 index and designated as a pilot stock. From May 2, 2005 to August 6, 2007, pilot stocks were exempted from short-sale price tests, thus decreasing the cost of short selling these stocks. The costs of short selling in non-pilot stocks remained unchanged. We find that pilot and non-pilot firms have similar levels of discretionary accruals before the announcement of the pilot program. Once the program begins, pilot firms' discretionary accruals decrease substantially, only to revert to pre-program levels after the pilot program ends. The impact of the pilot program on discretionary accruals is most pronounced among firms for which the pilot program was most likely to lower the marginal cost of short selling (i.e., where institutional ownership is high), and for which managers' incentives to manage earnings are relatively high (i.e., a high sensitivity of the CEO's wealth to the company's stock price). These patterns are not explained by changes in these firms' investment around the program, and are not limited to the subsample of firms that issue equity. These patterns also are not explained by a general increase in investors' attention paid to the pilot firms.

Although the pilot program decreased firms' tendencies to manage earnings, it is associated with an increased likelihood of discovery among firms that began to manipulate earnings aggressively enough to attract SEC sanction before the pilot program was announced. The decrease in earnings management and increase in the conditional probability of getting caught appear to offset each other such that the unconditional probability of being caught for financial misrepresentation is not significantly higher for pilot firms compared to non-pilot firms.

Finally, we document that, during the pilot program, pilot firms' coefficients of current returns on future earnings increase, and the magnitude of post earnings announcement drift (PEAD) decreases among pilot firms with the most negative earnings surprises. These results indicate that pilot firms' reduction in earnings management during the pilot program corresponds to an increase in the efficiency of their stock prices.

Short selling remains a controversial activity in financial markets. But our results uncover an important external benefit from short selling activity. A decrease in the cost of short selling curbs managers' willingness to manipulate earnings through discretionary accruals and makes stock prices more informative of future earnings. This demonstrates one path by which trading in secondary financial markets affects business decisions (see Bond, Edmans, and Goldstein, 2012).

References

Ahmed, A., Zhou, J., Lobo, G., 2006. Job security and income smoothing: an empirical test of the Fudenberg and Tirole (1995) model. Unpublished Manuscript.

Alexander, G. J., Peterson, M. A., 1999. Short selling on the New York stock exchange and the effects of the uptick rule. Journal of Financial Intermediation 8, 90–116.

Alexander, G. J., Peterson, M. A., 2008. The effect of price tests on trader behavior and market quality: An analysis of Reg SHO. Journal of Financial Markets 11, 84–111.

Angel, J. J., 1997. Short selling on the NYSE. Unpublished Manuscript.

Autore, D.M., Billingsley, R. S., Kovacs T., 2011. The 2008 short sale ban: Liquidity, dispersion of opinion, and the cross-section of returns of US financial stocks. Journal of Banking & Finance 35, 2252–2266.

Asquith, P., Pathak, P. A., Ritter, J. R., 2005. Short interest, institutional ownership, and stock returns. Journal of Financial Economics 78, 243–276.

Ball, R., and P. Brown. 1968. An empirical evaluation of accounting income numbers. Journal of Accounting Research 6, 159-178.

Beber, A., Pagano, M., 2013. Short-selling bans around the world: Evidence from the 2007–09 crisis. The Journal of Finance 68, 343–381.

Beneish, M. D., Vargus, M. E., 2002. Insider trading, earnings quality, and accrual mispricing. The Accounting Review 77, 755–791.

Bergstresser, D., Philippon, T., 2006. CEO incentives and earnings management. Journal of Financial Economics 80, 511–529.

Bernard, V. L., and Thomas, J. K.,1989. Post-earnings-announcement drift: Delayed price response or risk premium? Journal of Accounting Research 27, 1-36.

Bernard, V. L., and Thomas, J. K.,1990. Evidence that stock prices do not fully reflect the implications of current earnings for future earnings. Journal of Accounting and Economics 13, 305-340.

Boehmer, E., Jones, C. M., and Zhang, X. 2013. Shackling short sellers: The 2008 shorting ban. Review of Financial Studies 26, 1363-1400.

Boehmer, E., Wu, J., 2013. Short selling and the price discovery process. Review of Financial Studies 26, 287–322.

Bond, P., Edmans, A., Goldstein, I., 2012. The real effects of financial markets. Annual Review of Financial Economics 4, 339–360.

Burns, N., Kedia, S., 2006. The impact of performance-based compensation on misreporting. Journal of Financial Economics 79, 35–67.

Cao, B., Dhaliwal, D., Kolasinski, A. C., Reed, A. V., 2006. Bears and numbers: investigating how short sellers exploit and affect earnings-based pricing anomalies. Unpublished Manuscript.

Chen, J., Hong, H., Stein, J., 2002. Breadth of ownership and stock returns. Journal of Financial Economics 66, 171–205.

Chen, H., Noronha, G., Singal, V., 2004. The price response to S&P 500 index additions and deletions: Evidence of asymmetry and a new explanation. Journal of Finance 59, 1901–1930.

Christophe, S. E., Ferri, M. G., Angel, J. J., 2004. Short-selling prior to earnings announcements. Journal of Finance 59, 1845–1876.

Cornett, M. M., Marcus, A. J., Tehranian, H., 2008. Corporate governance and pay-for-performance: the impact of earnings management. Journal of Financial Economics 87, 357–373.

Da, Z., Engelberg, J., Gao, P., 2011. In Search of Attention. Journal of Finance 66, 1461–1499.

De Angelis, D., Grullon, G., Michenaud, S., 2013. Downside risk and the design of executive incentives: Evidence from the removal of short-selling constraints. Unpublished Manuscript.

Dechow, P. M., Ge, W., Schrand, C., 2010. Understanding earnings quality: a review of the proxies, their determinants and their consequences. Journal of Accounting and Economics 50, 344–401.

Dechow, P. M., Ge, W., Larson, C. R., Sloan, R. G., 2011. Predicting material accounting misstatements. Contemporary Accounting Research 28, 17–82.

Dechow, P., Kothari, S. P., and Watts, R., 1998. The relation between earnings and cash flows. Journal of Accounting and Economics 25, 133-168.

Dechow, P. M., Sloan, R. G., Sweeney, A. P., 1996. Causes and consequences of earnings manipulation: an analysis of firms subject to enforcement actions by the SEC. Contemporary Accounting Research 13, 1–36.

DeFond, M. L., Jiambalvo, J., 1994. Debt covenant violation and manipulation of accruals. Journal of Accounting and Economics 17, 145–176.

DeFond, M. L., Park, C. W., 1997. Smoothing income in anticipation of future earnings. Journal of Accounting and Economics 23, 115–139.

Desai, H., Krishnamurthy, S., Venkataraman, K., 2006. Do short sellers target firms with poor earnings quality? Evidence from earnings restatements. Review of Accounting Studies 11, 71-90.

Diether, K. B., Lee, K.H., Werner, I. M., 2009. It's SHO time! Short-sale price tests and market quality. Journal of Finance 64, 37–73.

Dyck, A., Morse, A., Zingales, L., 2010. Who blows the whistle on corporate fraud? Journal of Finance 65, 2213-2253.

Edmans, A., Fang, V. W., Zur, E., 2013. The effect of liquidity on governance. Review of Financial Studies 26, 1443–1482.

Edmans, A., Gabaix, X., Landier, A., 2009. A multiplicative model of optimal CEO incentives in market equilibrium. Review of Financial Studies 22, 4881–4917.

Edmans, A., Fang, V.W., Lewellen, K., 2014. Equity vesting and managerial myopia. Unpublished Manuscript.

Efendi, J., Kinney, M., Swanson, E., 2005. Can short sellers anticipate accounting restatements? Unpublished Manuscript.

Efendi, J., Srivastava, A., Swanson, E. P., 2007. Why do corporate managers misstate financial statements? The role of option compensation and other factors. Journal of Financial Economics 85, 667–708.

Fairfield, P. M., Whisenant, J. S., Yohn, T. L., 2003. Accrued earnings and growth: implications for future profitability and market mispricing. The Accounting Review 78, 353–371.

Fang, V. W., Tian, X., Tice, S., 2013. Does stock liquidity enhance or impede firm innovation? Journal of Finance, forthcoming.

Fang, V. W., Noe, T. H., Tice, S., 2009. Stock market liquidity and firm value. Journal of Financial Economics 94, 150–169.

Ferri, M. G., Christophe, S. E., and Angel, J. J., 2004. A short look at bear raids: Testing the bid test, Unpublished Manuscript.

Friedlan, J.M., 1994. Accounting choices of issuers of initial public offerings. Contemporary Accounting Research 11, 1–31.

Frino, A., Lecce, S., Lepone, A., 2011. Short-sales constraints and market quality: Evidence from the 2008 short-sales bans. International Review of Financial Analysis 20, 225–236.

Grullon, G., Michenaud, S., Weston, J., 2013. The real effects of short-selling constraints. Unpublished Manuscript.

Harrison, J. M., Kreps, D. M., 1978. Speculative investor behavior in a stock market with heterogeneous expectations. Quarterly Journal of Economics 92, 323–336.

Hazarika, S., Karpoff, J. M., Nahata, R., 2012. Internal corporate governance, CEO turnover, and earnings management. Journal of Financial Economics 104, 44–69.

He, J., and Tian, X., 2014. Short sellers and innovation: Evidence from a quasi-natural experiment. Unpublished Manuscript.

Hirshleifer, D., Teoh, S. H., Yu, J. J., 2011. Short arbitrage, return asymmetry, and the accrual anomaly. Review of Financial Studies 24, 2429–2461.

Jacob, J., Lys, T., and Neale, M. A., 1999. Expertise in forecasting performance of security analysts. Journal of Accounting and Economics 28, 51-82.

Jones, J. J., 1991. Earnings management during import relief investigations. Journal of Accounting Research 29, 193 – 228.

Kang, J.M., Kim, Y.H., 2013. Stock market liquidity and short-termism-driven CEO turnover. Unpublished Manuscript.

Karpoff, J. M., Koester, A., Lee, D. S., Martin, G., 2014. Databases challenges in financial misconduct research. Unpublished Manuscript.

Karpoff, J.M., Lee, D.S., Martin, G.S., 2008. The cost to firms of cooking the books. Journal of Financial & Quantitative Analysis 43, 581–611.

Karpoff, J. M., Lou, X., 2010. Short sellers and financial misconduct. Journal of Finance 65, 1879–1913.

Kecskés, A., Mansi, S.A., Zhang, A., 2013. Are short sellers informed? Evidence from the bond market. The Accounting Review 88, 611–639.

Kothari, S. P., Leone, A. J., Wasley, C. E., 2005. Performance matched discretionary accrual measures. Journal of Accounting and Economics 39, 163–197.

Lamont, O., 2004. Go down fighting: short sellers vs. firms. Unpublished Manuscript.

Lundholm, R., Myers, L. A., 2002. Bringing the future forward: the effect of disclosure on the returns-earnings relation. Journal of Accounting Research 40, 809–839.

Massa, M., Zhang, B., Zhang, H., 2013a. The invisible hand of short-selling: does short-selling discipline earnings manipulation? Unpublished Manuscript.

Massa, M., Zhang, B., Zhang, H., 2013b. Governance through threat: does short selling improve internal governance? Unpublished Manuscript.

McCormick, T., Reilly, L., 1996. The economic impact of the Nasdaq short sale rule. Nasdaq Economic Study.

Miller, E. M., 1977. Risk, uncertainty, and divergence of opinion. Journal of Finance 32, 1151–1168.

Nagel, S., 2005. Short sales, institutional investors and the cross-section of stock returns. Journal of Financial Economics 78, 277–309.

Opinion Research Corporation, 2008. Short selling study: the views of corporate issuers, prepared on behalf of NYSE Euronext.

Roberts, M., Whited, T., 2012. Endogeneity in empirical corporate finance. *Handbook of the Economics of Finance.*

SEC's OEA, 2007. Economic analysis of the short sale price restrictions under the regulation SHO pilot. Office of Economic Analysis, U.S. Securities and Exchange Commission.

Sweeney, A. P., 1994. Debt-covenant violations and managers' accounting responses. Journal of Accounting and Economics 17, 281–308.

Teoh, S. H., Welch, I., Wong, T. J., 1998a. Earnings management and the long-run market performance of initial public offerings. Journal of Finance 53, 1935–1974.

Teoh, S. H., Welch, I., Wong, T. J., 1998b. Earnings management and the underperformance of seasoned equity offerings. Journal of Financial Economics 50, 63–99.

Wu, J., Zhang, L., and Zhang, X. F., 2010. The q-theory approach to understanding the accrual anomaly. Journal of Accounting Research 48, 177–223.

Zang, A. Y., 2012. Evidence on the trade-off between real activities manipulation and accrual-based earnings management. The Accounting Review 87, 675–703.

Zhang, X. F., 2007. Accruals, investment, and the accrual anomaly. The Accounting Review 82, 1333–1363.

Appendix A- Variable Definitions

Variable Name	Definition
Dependent variables	
PM_DA	Performance-matched discretionary accruals, calculated as a firm's discretionary accruals minus the corresponding discretionary accruals of a matched firm from the same fiscal year and the same Fama-French 48 industry with the closest return-on-assets ratio. A firm's discretionary accruals are defined as the difference between its total accruals and the fitted normal accruals derived from a modified Jones model (Jones, 1991). The modified Jones model is specified as $\frac{TA_{i,t+1}}{ASSET_{i,t}} = \beta_0 + \beta_1 \frac{1}{ASSET_{i,t}} + \beta_2 \frac{\Delta REV_{i,t+1}}{ASSET_{i,t}} + \beta_3 \frac{PPE_{i,t+1}}{ASSET_{i,t}} + \varepsilon_{i,t+1}$. Total accruals $TA_{i,t+1}$ are defined as earnings before extraordinary items and discontinued operations (IBC) minus operating cash flows (OANCF-XIDOC), $ASSET_{i,t}$ is total assets at the beginning of the fiscal year $t+1$ (AT), $\Delta REV_{i,t+1}$ is the change in sales revenue (SALE) from the preceding fiscal year, and $PPE_{i,t+1}$ is the gross property, plant and equipment (PPEGT). The fitted normal accruals are computed as $NA_{i,t+1} = \widehat{\beta_0} + \widehat{\beta_1} \frac{1}{ASSET_{i,t}} + \widehat{\beta_2} \frac{(\Delta REV_{i,t+1} - \Delta AR_{i,t+1})}{ASSET_{i,t}} + \widehat{\beta_3} \frac{PPE_{i,t+1}}{ASSET_{i,t}}$ with the change in accounts receivable (RECT) subtracted from the change in sales revenue. Firm-year specific discretionary accruals are then calculated as $DA_{i,t+1} = (TA_{i,t+1} / ASSET_{i,t}) - NA_{i,t+1}$;
PM_DA_MBadj	Similar to *PM_DA*, except that market-to-book ratio *MB* is included as an additional regressor in both steps of the estimation procedure;
Experiment-related variables	
PILOT	A dummy variable that equals one if a firm's stock is designated as pilot stock in the Regulation SHO's pilot program and zero otherwise;
PRE	A dummy variable that equals one if a firm's fiscal year end falls between January 1, 2001 and December 31, 2003 and zero otherwise;
DURING	A dummy variable that equals one if a firm's fiscal year end falls between January 1, 2005 and December 31, 2007 and zero otherwise;
POST	A dummy variable that equals one if a firm's fiscal year end falls between January 1, 2008 and December 31, 2010 and zero otherwise;
Firm Characteristics	
ASSET	Book value of total assets (AT), *SIZE* is the natural logarithm of *ASSET*;
MB	Market-to-book ratio, calculated as market value of equity (PRCC_F×CSHO) divided by book value of equity (CEQ). *BM* is the inverse of *MB*;
ASSETGR	Total assets at the end of a given year divided by total assets at the beginning of the year minus one;
CAPEX	Capital expenditures (CAPX) during a given year scaled by the total assets at the beginning of the year;
R&D	Research and Development expenditures (XRD) during a given year scaled by the total assets at the beginning of the year, set to zero if missing;
INVESTMENT	The sum of *R&D* and *CAPEX*;
CFO	Operating cash flow (ONACF) during a given year scaled by the total assets at the beginning of the year;
LEV	Long term debt (DLTT) plus debt in current liabilities (DLC) scaled by the sum of (long term debt, debt in current liabilities, and total shareholders' equity (SEQ)) at the end of a given year;
CASH	Cash and short term investment (CHE) at the end of a given year scaled by the total assets at the beginning of the year;
DIVIDENDS	Common share dividends (DVC) plus preferred shares dividends (DVP) during a given year scaled by the total assets at the beginning of the year;

Additional variables used in the financial misconduct analysis	
DISCOVERED	A dummy variable that equals one in the month in which a firm is revealed to have engaged in misrepresentation, and zero otherwise.
MONTHS	The number of months from the beginning of the misrepresentation period to a given month; $MONTHS^2$ is the squared term of *MONTHS*;
MOMENTUM	The buy and hold return, measured in the 12 months prior to month *t*;
SEVERITY	The abnormal return of the trigger date, calculated as the raw return subtracting the corresponding return on the CRSP value-weighted index, upon the initial revelation of the misconduct;
Additional variables used in the price efficiency analysis	
X	X_t (X_{t-1}) is the earnings for fiscal year *t* (*t*-*1*), calculated as income before extraordinary items (IB) in year *t* (*t*-*1*) scaled by market value (PRC×SHROUT) three months after the end of year $t-1$. $X3_t$ is the aggregated earnings for the next three years following fiscal year *t*, calculated as the sum of income before extraordinary items in fiscal years $t+1$, $t+2$, and $t+3$ also scaled by market value three months after the end of year $t-1$;
R	R_t is the buy-and-hold return for fiscal year *t*, measured over the 12-month period ending three months after the end of year *t*. $R3_t$ is the buy-and-hold return for the three-year period following fiscal year *t*, starting three months after the end of year *t*.

Table 1: Firm characteristics of the treatment group and control group one year pre-SHO

Panel A: Treatment group and control group drawn from the 2004 Russell 3000 index, balanced panel sample

This panel reports summary statistics of the firm characteristics for the balanced panel sample of the treatment and control groups measured in 2003, the year immediately before the Regulation SHO's pilot program was announced. The sample is drawn from the 2004 Russell 3000 index and requires a firm to have data available to calculate firm characteristics and discretionary accruals across the entire sample period used in the empirical tests (i.e., 2001-2003, and 2005-2010). A firm is classified into the treatment group if its stock is designated as a pilot stock and into the control group otherwise. Variable definitions are listed in Appendix A. All variables are winsorized at the 1% and 99% levels. *ASSET* is in millions of dollars. *ASSETGR*, *CAPEX*, *R&D*, *ROA*, *CFO*, *LEV*, *CASH*, and *DIVIDENDS* are in percentage points. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	Treatment Group (*PILOT=1*)				Control Group (*PILOT=0*)				Tests for differences	
Variable	N	Mean	Median	Std. Dev	N	Mean	Median	Std. Dev	t-statistic	Wilcoxon z-statistic
ASSET	388	3,748.61	817.69	8,512.30	709	3,746.25	817.42	8,647.29	0.00	0.51
MB	388	2.75	1.95	3.79	709	2.60	1.98	3.13	0.68	0.03
ASSETGR	388	13.42	7.88	31.28	709	13.22	7.66	30.18	0.10	-0.30
CAPEX	388	5.55	3.76	5.54	709	5.50	3.65	5.88	0.14	0.91
R&D	388	4.19	0.00	8.40	709	4.04	0.32	7.83	0.27	-0.94
ROA	388	14.37	14.51	12.70	709	14.15	14.29	14.30	0.25	0.22
CFO	388	11.36	11.33	10.68	709	10.56	10.46	13.26	1.10	1.10
LEV	388	29.36	26.46	27.50	709	29.80	27.56	28.25	-0.25	-0.24
CASH	388	0.21	0.12	0.26	709	0.22	0.11	0.28	-0.44	-0.32
DIVIDENDS	388	0.83	0.00	1.49	709	0.73	0.00	1.33	1.14	1.07

Table 1 (continued)

Panel B: Treatment group and control group drawn from the 2004 Russell 3000 index, unbalanced panel sample

This panel reports summary statistics of the firm characteristics for the unbalanced panel sample of the treatment and control groups measured in 2003, the year immediately before the Regulation SHO's pilot program was announced. The sample is drawn from the 2004 Russell 3000 index and requires a firm to have data available to calculate a given firm characteristic in a given year. A firm is classified into the treatment group if its stock is designated as a pilot stock and into the control group otherwise. Variable definitions are listed in Appendix A. All variables are winsorized at the 1% and 99% levels. *ASSET* is in millions of dollars. *ASSETGR*, *CAPEX*, *R&D*, *ROA*, *CFO*, *LEV*, *CASH*, and *DIVIDENDS* are in percentage points. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	Treatment Group (*PILOT=1*)				Control Group (*PILOT=0*)				Tests for differences	
Variable	N	Mean	Median	Std. Dev	N	Mean	Median	Std. Dev	t-statistic	Wilcoxon z-statistic
ASSET	782	2,918.37	726.92	7,471.08	1,610	2,941.31	669.22	7,883.20	-0.07	1.26
MB	759	2.66	1.89	3.84	1,534	2.55	1.85	4.15	0.60	1.05
ASSETGR	781	17.81	8.82	40.64	1,605	17.61	8.45	43.10	0.11	0.38
CAPEX	741	5.59	3.61	6.21	1,504	5.28	3.31	5.98	1.13	1.74*
R&D	781	5.78	0.00	11.96	1,605	6.10	0.22	12.22	-0.61	-1.06
ROA	778	10.67	12.79	18.92	1,604	9.68	11.87	20.88	1.15	1.23
CFO	742	8.19	9.88	17.20	1,505	7.20	9.24	19.20	1.23	1.39
LEV	781	30.82	28.06	29.18	1,602	31.54	27.27	31.21	-0.55	0.11
CASH	781	0.27	0.13	0.36	1,605	0.27	0.12	0.38	-0.48	-0.66
DIVIDENDS	778	0.73	0.00	1.55	1,600	0.77	0.00	1.65	-0.69	-1.13

Table 2: Discretionary accruals before, during, and after the pilot program, univariate difference-in-differences tests

The top half of Panel A reports summary statistics of the level of discretionary accruals for the balanced panel sample of the treatment and control groups for the three-year periods before, during, and after the Regulation SHO's pilot program, and the differences in the mean and/or median. The bottom half of Panel A reports the univariate results of difference-in-differences (DiD) tests, with standard errors reported in parentheses below the DiD estimators. The sample is drawn from the 2004 Russell 3000 index and requires a firm to have data available to calculate firm characteristics and discretionary accruals across the entire sample period (i.e., 2001-2003, and 2005-2010). A firm is classified into the treatment group if its stock is designated as pilot stock during the program and into the control group otherwise. Panel B reports summary statistics of the level of discretionary accruals for the unbalanced panel sample of the treatment and control groups. The sample is also drawn from the 2004 Russell 3000 index and but only requires a firm to have data available to calculate discretionary accruals in a given year. Variables definitions are provided in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

Panel A: Balanced Panel Sample	Treatment Group (*PILOT=1*)			Control Group (*PILOT=0*)			Cross-sectional estimator:	
Variable of interest	N	Mean	Median	N	Mean	Median	Difference in Mean	Difference in Median
PM_DA								
PRE (2001-2003)	1,164	**-0.004**	-0.001	2,127	**-0.004**	-0.003	-0.001	0.002
DURING (2005-2007)	1,164	**-0.014**	-0.012	2,127	**-0.004**	-0.004	**-0.011****	**-0.008****
POST (2008-2010)	1,164	**0.000**	0.000	2,127	**-0.003**	-0.001	0.004	0.001
Univariate DiD test		N	Time-series estimator		N	Time-series estimator	DiD estimator	t-statistic of DiD estimator
ΔPM_DA								
DURING-PRE		388	**-0.011****		709	0.000	**-0.011** (0.006)	**-1.67***
POST-DURING		388	**0.013*****		709	0.001	**0.013** (0.006)	**2.06****
POST-PRE		388	0.003		709	0.001	**0.002** (0.006)	0.32

Panel B: Unbalanced Panel Sample	Treatment Group (*PILOT=1*)			Control Group (*PILOT=0*)			Cross-sectional estimator:	
Variable of interest	N	Mean	Median	N	Mean	Median	Difference in Mean	Difference in Median
PM_DA								
PRE (2001-2003)	2,067	**-0.002**	0.000	4,151	**0.000**	-0.002	-0.002	0.002
DURING (2005-2007)	1,865	**-0.012**	-0.009	3,740	**-0.003**	-0.003	**-0.009****	**-0.006****
POST (2008-2010)	1,605	**0.001**	0.001	3,087	**-0.005**	-0.003	0.006	0.004

Table 3: Multivariate difference-in-differences tests

This table reports the regression results that estimate differences in pilot and non-pilot firms' discretionary accruals for the periods before, during, and after Regulation SHO's pilot program, using a balanced panel sample. The sample is drawn from the 2004 Russell 3000 index and requires a firm to have data available to calculate firm characteristics and discretionary accruals across the entire sample period (i.e., 2001-2003, and 2005-2010). A firm is classified into the treatment group if its stock is designated as a pilot stock during the program and into the control group otherwise. We estimate the following model: $PM\_DA_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \beta_3 PILOT_i + \beta_4 DURING_t + \beta_5 POST_t + \varepsilon_{i,t}$ in Column (1). We then augment the model by including *SIZE*, *MB*, *ROA*, and *LEV* in Columns (2) and by further including eight year fixed effects from 2002-2003, and from 2005-2010 in Column (3). We omit *PILOT* and *POST* in Column (3) to avoid multicollinearity. Variable definitions are provided in Appendix A. Coefficient estimates are shown in bold and their robust standard errors clustered by year and firm are displayed in parentheses below. For brevity, the coefficient estimates on year fixed effects in Column (3) are not reported. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	(1)	(2)	(3)
Dependent Variables		*PM_DA*	
PILOT×DURING	**-0.010****	**-0.010****	**-0.010****
	(0.004)	(0.004)	(0.004)
PILOT×POST	**0.004**	**0.003**	**0.003**
	(0.004)	(0.004)	(0.005)
PILOT	**-0.000**	**0.000**	**0.000**
	(0.003)	(0.003)	(0.003)
DURING	**-0.001**	**-0.001**	
	(0.002)	(0.002)	
POST	**0.000**	**-0.001**	
	(0.005)	(0.005)	
SIZE		**0.002***	**0.002***
		(0.001)	(0.001)
MB		**-0.001**	**-0.001**
		(0.001)	(0.001)
ROA		**-0.041****	**-0.041*****
		(0.016)	(0.016)
LEV		**-0.014**	**-0.013**
		(0.009)	(0.008)
INTERCEPT	**-0.004****	**-0.006**	**-0.006**
	(0.002)	(0.007)	(0.008)
Year fixed effects			Included
# of obs.	9,873	9,873	9,873
Adjusted R^2	0.10%	0.40%	0.40%

Table 4: Multivariate difference-in-differences tests, partitioned on institutional ownership and CEO scaled wealth-performance sensitivity

The left panel of this table reports the regression results that estimate differences in pilot and non-pilot firms' discretionary accruals for the periods before, during, and after Regulation SHO's pilot program, separately for subsamples of firms with institutional ownership in 2003 above, or equal to or below, the sample median. The right panel of this table reports the regression results, separately for subsamples of firms with CEO scaled wealth-performance sensitivity (WPS) in 2003 above, or equal to or below, the sample median. Institutional ownership data are from the Thomson Institutional (13f) holding database. The WPS measure is from Edmans, Gabaix, and Landier (2009). We estimate the following model: $PM\_DA_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \beta_3 PILOT_i + \beta_4 DURING_t + \beta_5 POST_t + \varepsilon_{i,t}$ in Columns (1), (3), (5), and (7). We then augment the model by including *SIZE*, *MB*, *ROA*, and *LEV* in Columns (2), (4), (6), and (8). Variable definitions are provided in Appendix A. Coefficient estimates are shown in bold and their robust standard errors clustered by year and firm are displayed in parentheses below. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High Institutional Ownership		*Low Institutional Ownership*		*High WPS*		*Low WPS*	
Dependent Variables	*PM_DA*				*PM_DA*			
PILOT×DURING	**-0.017*****	**-0.018*****	**-0.002**	**-0.003**	**-0.019***	**-0.020****	**-0.012**	**-0.011**
	(0.005)	(0.005)	(0.004)	(0.004)	(0.010)	(0.010)	(0.008)	(0.008)
PILOT×POST	**-0.005**	**-0.006**	**0.011**	**0.011**	**0.007**	**0.005**	**0.010***	**0.011***
	(0.005)	(0.004)	(0.007)	(0.007)	(0.010)	(0.009)	(0.006)	(0.006)
PILOT	**0.008****	**0.008****	**-0.008****	**-0.007***	**0.011***	**0.012****	**-0.004**	**-0.005**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)
DURING	**0.001**	**-0.002**	**-0.002**	**-0.001**	**0.003**	**0.001**	**0.003**	**0.002**
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
POST	**-0.005**	**-0.008**	**0.006**	**0.006**	**0.003**	**0.001**	**-0.001**	**-0.004**
	(0.008)	(0.007)	(0.004)	(0.004)	(0.005)	(0.004)	(0.008)	(0.008)
SIZE		**0.007*****		**-0.001**		**0.003***		**0.004**
		(0.002)		(0.001)		(0.002)		(0.003)
MB		**0.000**		**-0.001**		**-0.002***		**-0.000**
		(0.001)		(0.001)		(0.001)		(0.001)
ROA		**-0.065**		**-0.018**		**-0.013**		**-0.066****
		(0.040)		(0.019)		(0.046)		(0.026)
LEV		**-0.025***		**-0.001**		**-0.010**		**-0.004**
		(0.013)		(0.005)		(0.009)		(0.013)
INTERCEPT	**-0.007*****	**-0.036****	**-0.001**	**0.012*****	**-0.013*****	**-0.025**	**-0.002**	**-0.019**
	(0.002)	(0.017)	(0.004)	(0.004)	(0.004)	(0.021)	(0.004)	(0.019)
# of obs.	4,936	4,936	4,937	4,937	3,523	3,523	3,524	3,524
Adjusted R^2	0.10%	0.09%	0.20%	0.50%	0.40%	0.70%	0.20%	0.60%

Table 5: Multivariate difference-in-differences tests controlling for investment

This table reports the regression results that estimate differences in pilot and non-pilot firms' discretionary accruals for the periods before, during, and after Regulation SHO's pilot program, using a balanced panel sample. The sample is drawn from the 2004 Russell 3000 index and requires a firm to have data available to calculate firm characteristics and discretionary accruals across the entire sample period (i.e., 2001-2003, and 2005-2010). A firm is classified into the treatment group if its stock is designated as a pilot stock during the program and into the control group otherwise. We estimate the following model: $PM\_DA_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \beta_3 PILOT_i + \beta_4 DURING_t + \beta_5 POST_t + \beta_6 SIZE_{i,t} + \beta_7 MB_{i,t} + \beta_8 ROA_{i,t} + \beta_9 LEV_{i,t} + \varepsilon_{i,t}$. We include *R&D* and *CAPEX* in Column (1), *INVESTMENT* in Column (2), and their squared terms in Columns (3)-(4). In Column (5), we replace the dependent variable *PM_DA* with *PM_DA_MBadj*. Variable definitions are provided in Appendix A. Coefficient estimates are shown in bold and their robust standard errors clustered by year and firm are displayed in parentheses below. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	(1)	(2)	(3)	(4)	(5)
Dependent Variables		*PM_DA*			*PM_DA_MBadj*
PILOT×DURING	**-0.010****	**-0.010****	**-0.010****	**-0.010****	**-0.018*****
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
PILOT×POST	**0.003**	**0.003**	**0.003**	**0.003**	**0.004**
	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)
PILOT	**-0.000**	**-0.000**	**-0.000**	**-0.000**	**0.004**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
DURING	**-0.001**	**-0.001**	**-0.001**	**-0.001**	**0.002**
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
POST	**-0.002**	**-0.003**	**-0.002**	**-0.003**	**0.001**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
SIZE	**0.001**	**0.001**	**0.001**	**0.001**	**0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
MB	**-0.000**	**-0.000**	**-0.000**	**-0.000**	**-0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
ROA	**-0.062*****	**-0.053*****	**-0.064*****	**-0.058*****	**-0.044****
	(0.014)	(0.015)	(0.014)	(0.015)	(0.019)
LEV	**-0.017****	**-0.016***	**-0.016***	**-0.015***	**-0.013**
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)
R&D	**-0.001*****		**-0.001***		
	(0.000)		(0.000)		
CAPEX	**-0.001*****		**-0.001****		
	(0.000)		(0.000)		
INVESTMENT		**-0.001*****		**-0.001****	
		(0.000)		(0.000)	
R&D^2			**-0.000**		
			(0.000)		
CAPEX^2			**0.000**		
			(0.000)		
INVESTMENT^2				**-0.000**	
				(0.000)	
INTERCEPT	**0.015***	**0.013***	**0.013***	**0.010**	**-0.001**
	(0.008)	(0.008)	(0.008)	(0.007)	(0.009)
# of obs.	9,849	9,871	9,849	9,871	9,206
Adjusted R^2	1.06%	1.02%	1.10%	1.10%	0.50%

Table 6: Multivariate difference-in-differences tests partitioned on seasoned equity offering

This table reports the regression results that estimate differences in pilot and non-pilot firms' discretionary accruals for the periods before, during, and after Regulation SHO's pilot program, separately for subsamples of firms with seasoned equity offering as recorded in the Thomson Reuters Securities Data Company (SDC) Platinum database or not. The sample is drawn from the 2004 Russell 3000 index and requires a firm to have data available to calculate firm characteristics and discretionary accruals across the entire sample period (i.e., 2001-2003, and 2005-2010). Firms that did not issue equity during a given year are labeled as "*Non-Equity Issuer*" and those that issued equity at least once during the year are labeled as "*Equity Issuer*". A firm is classified into the treatment group if its stock is designated as a pilot stock during the program and into the control group otherwise. We estimate the following model: $PM\_DA_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \beta_3 PILOT_i + \beta_4 DURING_t + \beta_5 POST_t + \varepsilon_{i,t}$ in Columns (1) and (3). We then augment the model by including *SIZE*, *MB*, *ROA*, and *LEV* in Columns (2) and (4). Variable definitions are provided in Appendix A. Coefficient estimates are shown in bold and their robust standard errors clustered by year and firm are displayed in parentheses below. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	(1)	(2)	(3)	(4)
	Equity Issuer		*Non-Equity Issuer*	
Dependent Variables	*PM_DA*		*PM_DA*	
PILOT×DURING	**-0.027**	**-0.020**	**-0.009*****	**-0.010*****
	(0.036)	(0.038)	(0.003)	(0.003)
PILOT×POST	**0.007**	**0.006**	**0.003**	**0.003**
	(0.052)	(0.052)	(0.006)	(0.006)
PILOT	**-0.016**	**-0.016**	**0.001**	**0.002**
	(0.012)	(0.012)	(0.002)	(0.002)
DURING	**-0.002**	**-0.009**	**0.000**	**-0.000**
	(0.036)	(0.038)	(0.001)	(0.002)
POST	**-0.005**	**-0.011**	**0.001**	**0.000**
	(0.014)	(0.016)	(0.006)	(0.006)
SIZE		**-0.001**		**0.002****
		(0.005)		(0.001)
MB		**-0.002**		**-0.001**
		(0.002)		(0.001)
ROA		**-0.040**		**-0.041***
		(0.048)		(0.021)
LEV		**0.016**		**-0.016**
		(0.018)		(0.010)
INTERCEPT	**0.011****	**0.023**	**-0.005*****	**-0.008**
	(0.004)	(0.030)	(0.001)	(0.007)
# of obs.	559	558	9,314	9,313
Adjusted R^2	0.70%	1.40%	0.10%	0.30%

Table 7: The effect of pilot program on the discovery of financial misrepresentation

This table reports results of tests for the effect of short selling on the speed with which financial misrepresentation is uncovered, using a sample of monthly observations of the firms that initiated financial misrepresentation prior to the announcement of the pilot program (i.e., July of 2004) but were caught and publicly revealed after the announcement of the pilot program. The data consist of the intersection of the balanced panel sample as described in Table 3 and the database on SEC enforcement actions for financial misrepresentations as described in Karpoff et al. (2014). We estimate the Cox proportional odds model: $DISCOVERED_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i + \beta_3 DURING_t + \beta_4 MONTHS_{i,t} + \beta_5 MONTHS_{i,t}^2 + \beta_6 SIZE_{i,t} + \beta_7 BM_{i,t} + \beta_8 MOMENTUM_{i,t} + \beta_9 SEVERITY_{i,t} + \varepsilon_{i,t}$. Variable definitions are provided in Appendix A. Coefficient estimates are shown in bold and their robust standard errors clustered by year and firm are displayed in parentheses below. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

Dependent Variable	*DISCOVERED*
PILOT×DURING	**2.074****
	(1.053)
PILOT	**-1.911***
	(1.030)
DURING	**2.281*****
	(0.293)
MONTHS	**0.025***
	(0.013)
MONTHS^2	**-0.000**
	(0.000)
SIZE	**0.103***
	(0.055)
BM	**-0.099**
	(0.337)
MOMENTUM	**-0.902*****
	(0.284)
SEVERITY	**-2.539****
	(1.045)
INTERCEPT	**-6.748*****
	(0.677)
# of obs.	5,348
Pseudo R^2	20.29%

Table 8: Tests of price efficiency: the coefficient of current returns on future earnings

This table examines differences in the annual current return-future earnings relation across the treatment firms and control firms for the six-year period surrounding Regulation SHO's pilot program (i.e., 2001-2003 and 2005-2007). The data consist of the balanced panel sample as described in Table 3 with earnings and returns information available. We estimate the following model $R_{i,t} = \beta_0 + \beta_1 X_{i,t-1} + \beta_2 X_{i,t} + \beta_3 X3_{i,t} + \beta_4 R3_{i,t} + \varepsilon_{i,t}$ in Column (1). We then augment the model by including the interactions of $PILOT_i \times DURING_t$, $PILOT_i$, $DURING_t$, with $X3_{i,t}$ in Column (2) and by further including the interactions of $PILOT_i \times DURING_t$, $PILOT_i$, $DURING_t$, with $X_{i,t-1}$, $X_{i,t}$, $X3_{i,t}$, and $R3_{i,t}$ in Column (3). Variable definitions are provided in Appendix A. Coefficient estimates are shown in bold and their robust standard errors clustered by year and firm are displayed in parentheses below. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	(1)	(2)	(3)
Dependent Variable		R_t	
X_{t-1}	**-0.723***** (0.279)	**-0.676***** (0.054)	**-0.522***** (0.075)
X_t	**0.534***** (0.092)	**0.576***** (0.044)	**0.618***** (0.064)
$X3_t$	**0.321***** (0.050)	**0.270***** (0.027)	**0.228***** (0.029)
$R3_t$	**-0.080**** (0.034)	**-0.125***** (0.007)	**-0.119***** (0.009)
$X3_t \times PILOT \times DURING$		**0.158***** (0.060)	**0.210**** (0.084)
$X3_t \times PILOT$		**-0.014** (0.036)	**0.020** (0.052)
$X3_t \times DURING$		**0.037** (0.030)	**0.134***** (0.039)
$X_{t-1} \times PILOT \times DURING$			**-0.387** (0.385)
$X_{t-1} \times PILOT$			**-0.621***** (0.223)
$X_{t-1} \times DURING$			**-0.235*** (0.141)
$X_t \times PILOT \times DURING$			**0.145** (0.394)
$X_t \times PILOT$			**0.092** (0.193)
$X_t \times DURING$			**-0.324**** (0.134)
$R3_t \times PILOT \times DURING$			**-0.037** (0.026)
$R3_t \times PILOT$			**0.011** (0.015)
$R3_t \times DURING$			**-0.041**** (0.018)
INTERCEPT	**0.209*** (0.111)	**0.347***** (0.010)	**0.347***** (0.010)
# of obs.	13,844	13,844	13,844
Adjusted R^2	7.20%	10.90%	11.37%

Table 9: Tests of price efficiency: post-earnings announcement drift (PEAD)

This table reports differences in the post-earnings announcement drift across the pilot firms and non-pilot firms for the three-year period during Regulation SHO's pilot program (i.e., 2005-2007). The data consist of the balanced panel sample as described in Table 3 with earnings, analyst forecasts, and returns information available. In each quarter, the sample firms are sorted into ten deciles (D1-D10) based on their latest earnings surprises. The earnings surprise is calculated as the firm's actual earnings per share (EPS) minus the latest analyst consensus EPS forecast before the earnings announcement date (both from I/B/E/S), scaled by the firm's stock price two days before the earnings announcement date. PEAD is the cumulative abnormal return (*CAR*) following the earnings surprise, calculated as the stock's raw return minus the corresponding value-weighted market return over the (+2, +11) window relative to the earnings announcement date. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	Post-earnings announcement drift (+2, +11)		
	Treatment Group	Control Group	*t*-Statistics Control – Treatment
Earnings surprise			
D1 (Most negative)	**-0.38%**	**-1.26%*****	**2.47****
D2	-0.41%**	-0.41%***	0.01
D3	-0.38%**	-0.57%***	0.88
D4	0.00%	-0.07%	0.35
D5	-0.22%	-0.24%**	0.12
D6	-0.13%	-0.16%	0.13
D7	0.10%	-0.10%	0.95
D8	-0.02%	-0.06%	0.16
D9	0.23%	0.36%**	0.45
D10 (Most positive)	0.97%***	0.83%***	0.43

Figure 1. Managers' marginal benefits and costs of earnings management

This diagram illustrates Hypothesis 1, which states that earnings management in the pilot firms will decrease relative to earnings management in the non-pilot firms during the pilot program. In the diagram, a decrease in the cost of short selling decreases managers' expected benefits from earnings management and increases managers' expected costs, leading to a decrease in the optimal amount of earnings management. The managers' benefits decrease because the increased prospect of short selling decreases the potential inflation in stock prices that motivate managers to manage earnings in the first place. The managers' costs increase because the increased prospect of short selling increases the probability that the managers will be discovered and face adverse consequences for any given level of earnings management This results in a downward shift in the marginal benefit and an upward shift in the marginal cost of earnings management. MB_0 and MC_0 represent the manager's marginal benefits and marginal costs before the decrease in short selling costs, while MB_1 and MC_1 represent the marginal benefits and marginal costs after the decrease in short selling costs.

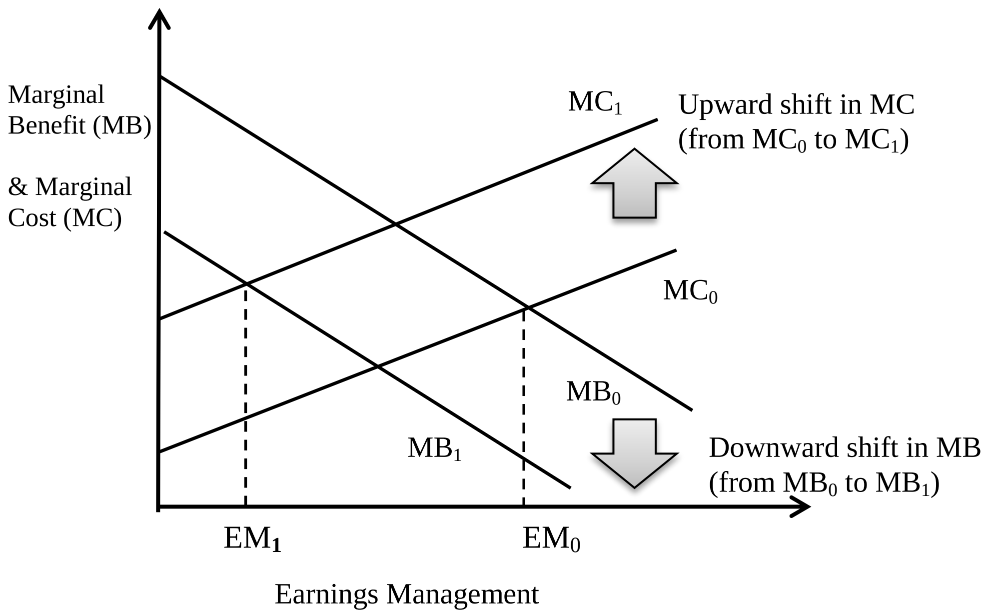

Figure 2. Discretionary accruals for pilot vs. non-pilot firms

This figure displays the results reported in Panel A of Table 2. It depicts the mean discretionary accruals for the balanced panel sample of the treatment group and control group for the periods before, during, and after Regulation SHO's pilot program, i.e., 2001-2003, 2005-2007, and 2008-2010. The sample is drawn from the 2004 Russell 3000 index and requires a firm to have data available to calculate firm characteristics and discretionary accruals across the entire sample period used in the empirical tests (i.e., 2001-2003, and 2005-2010).