The Journal of FINANCE
The Journal of THE AMERICAN FINANCE ASSOCIATION

THE JOURNAL OF FINANCE • VOL. LXXI, NO. 3 • JUNE 2016

Short Selling and Earnings Management: A Controlled Experiment

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ABSTRACT

During 2005 to 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 and likelihood of marginally beating earnings targets decrease during this period, and revert to pre-experiment levels when the program ends. After the program starts, pilot firms are more likely to be caught for fraud initiated before the program, and their stock returns better incorporate earnings information. These results indicate that short selling, or its prospect, curbs earnings management, helps detect fraud, and improves price efficiency.

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

In this paper we exploit a randomized 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 stocks in the Russell 3000 index were ranked by trading volume

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[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).

DOI: 10.1111/jofi.12369

within each exchange and every third one was designated as a pilot stock. From May 2, 2005 to August 6, 2007, pilot stocks were exempted from short-sale price tests, including the tick test for exchange-listed stocks and the bid test for NASDAQ National Market (NASDAQ-NM) stocks.[2]

The pilot program creates an ideal setting to examine the effect of short selling on corporate financial reporting decisions, for three reasons. First, the exemption from short-sale price tests decreased the cost of short selling in the pilot stocks relative to the nonpilot stocks (SEC (2007), Diether, Lee, and Werner (2009)). The pilot program thus eliminates the need to directly estimate short-selling costs, a notoriously difficult task (Lamont (2012)). Rather, we use the fact that the prospect of short selling increased for pilot firms relative to nonpilot firms during the program. Second, the pilot program represents a truly exogenous shock to the cost of selling short in the affected firms. We identify no evidence that the firms themselves lobbied for the pilot program, or that any individual firm could know it would be in the pilot group until the program was announced. Third, the pilot program had specific beginning and ending dates, facilitating difference-in-differences (hereafter, DiD) analysis of the impact of short-selling costs on firms' financial reporting. In particular, the anticipated 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., Roberts and Whited (2013)).

We begin by verifying that pilot firms represent a random draw from the Russell 3000 population. In the fiscal year before the pilot program, pilot and nonpilot firms are similar in size, growth, investment, 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 signed discretionary accruals once the program starts.[3] After the program ends, pilot firms' discretionary accruals revert to pre-program levels. The nonpilot firms, meanwhile, show no significant change in discretionary accruals around the pilot program. Our point estimates indicate that performance-matched discretionary accruals, as a percentage of assets, are one percentage point lower for pilot firms than for nonpilot firms during the pilot program compared to the pre-pilot period. This corresponds to 7.4% of the standard deviation of discretionary accruals in our sample.

We also examine the pilot program's effect on two alternative measures of earnings management. First, we find that the likelihood of beating the analyst

[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). Before the pilot program ran its entire course, the SEC eliminated short-sale price tests for all exchange-listed stocks on July 6, 2007 (Securities Exchange Act of 1934 Release No. 34-55970, July 3, 2007).

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

consensus forecast by up to one cent is 1.8 percentage points lower for the pilot firms than for the nonpilot firms during the pilot program compared to the pre-pilot period. This represents 11.1% of the unconditional likelihood of meeting or just beating the analyst consensus forecast in our sample. Similarly, the likelihood of meeting or just beating the firm's quarterly earnings per share (EPS) in the same quarter of the prior year is 0.8 percentage points lower for the pilot firms during the pilot program compared to the pre-pilot period, representing 14.2% of the unconditional likelihood. Second, we find that the likelihood of being classified as a misstating firm, based on the *F*-score of Dechow et al. (2011), is significantly lower for the pilot firms during the pilot period compared to the pre-pilot period. Combined with our results regarding discretionary accruals, these results indicate that pilot firms decrease earnings management during the pilot program.

We consider several alternative interpretations for the patterns we observe in discretionary accruals. One possibility is that pilot firms' discretionary accruals reflect changes in their growth, investment, or equity issuance, as Grullon, Michenaud, and Weston (2015) document a significant reduction in financially constrained pilot firms' investment and equity issuance during the pilot program. We consider several ways to control for firm growth and investment, both in the construction of our discretionary accruals measures and in the multivariate tests. None of these controls have a material effect on our main findings. We also find that the pilot firms' investment levels do not follow a pattern that would explain the changes in their discretionary accruals during and after the pilot program. Regarding the possible impact of equity issuance, we find that pilot firms' discretionary accruals pattern is similar between firms that do not seek to issue equity and the overall sample. These results indicate that the effect of the pilot program on discretionary accruals is unlikely to be explained by changes in pilot firms' growth, investment, or equity issuance around the program.

Another possible explanation is that managers of the pilot firms decreased earnings management because of a general increase in the attention investors paid 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 discretionary accruals, nor do they affect our main findings regarding discretionary accruals.

The most plausible interpretation of our results is that the pilot program reduced the cost of short selling sufficiently among the pilot firms to increase potential short sellers' monitoring activities, and that the increased monitoring induced a decrease in these firms' earnings management.[4] We conduct three additional tests to further probe this interpretation. First, we find that, among the pilot firms during the pilot program, short selling is positively related to signed discretionary accruals. Second, we find that short interest increases in

[4] Throughout this paper, we use "potential short sellers" or "short sellers" to refer to both investors who may take new short positions and investors with existing short positions.

months in which firms are later revealed to have engaged in financial misrepresentation during our sample period. And third, we find that, among firms that previously initiated financial fraud, pilot firms are more likely to get caught than control firms after the pilot period started. We also find that the unconditional likelihood of pilot firms being caught for financial fraud converges monotonically toward that of nonpilot firms as we sequentially include cases of fraud initiated after the pilot program begins. This result is consistent with the argument that pilot firms' conditional likelihood of being caught for any fraud they commit is higher during the program, and with our main finding that pilot firms endogenously adjust by decreasing earnings manipulation after the pilot program begins.

Finally, we examine the implications of the pilot program for price efficiency through its effect on firms' reporting practices. We show that the coefficients of pilot firms' current returns on future earnings increase during the pilot period. Among firms announcing particularly negative earnings surprises, the well-documented post-earnings announcement drift (PEAD) disappears for pilot firms during the period, while it remains significant for nonpilot firms. These 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 as their stock returns better incorporate earnings information.

The above findings make four contributions to the literature. First, they show that an increase in the prospect of short selling has a significant effect on firms' financial reporting. This result demonstrates one avenue through which trading in secondary financial markets affects firms' decisions.[5] Second, our findings identify a new determinant of earnings management, namely, short-sale constraints, adding to the factors identified in prior research (for a review, see Dechow, Ge, and Schrand (2010)). Third, our results indicate that the prospect of short selling improves price efficiency not only by facilitating the flow of private information into prices (e.g., Miller (1977), Harrison and Kreps (1978), Chang, Cheng, and Yu (2007), Boehmer and Wu (2013)), but also by decreasing managers' tendency to manage earnings. And fourth, our findings contribute to the policy debate on the benefits and costs of short selling. Previous research demonstrates that short sellers are good at identifying the overpriced shares of firms that have manipulated earnings, and short sellers' trading accelerates the discovery of financial misconduct.[6] Our results indicate that the prospect of short selling conveys additional external benefits to investors by improving financial reporting quality and stock price efficiency in general, even among firms not charged with financial reporting violations.

[5] See Bond, Edmans, and Goldstein (2012) for a survey of research on the real effects of financial markets. For example, Karpoff and Rice (1989) and Fang, Noe, and Tice (2009) examine the effect of stock liquidity on firm performance, Fang, Tian, and Tice (2014) examine the effect of liquidity on innovation, and Grullon, Michenaud, and Weston (2015) examine the effect of short-selling constraints on investment and equity issuance.

[6] See the references in footnote 1. To be sure, other studies have noted the potential dark side of short selling, as manipulative short selling could reduce price efficiency (e.g., Gerard and Nanda (1993), Henry and Koski (2010)).

This paper is organized as follows. Section I describes short-sale price tests in the U.S. equity markets, how they can affect firms' tendency to manage earnings, and related research. Section II describes the data. Section III reports tests of the effect of Regulation SHO's pilot program on firms' earnings management. Section IV examines whether short sellers actually increased their scrutiny of the pilot stocks during the pilot program by comparing the probability of fraud detection between pilot and nonpilot firms. Section V reports on tests that examine whether the pilot program coincided with an increase in the efficiency of pilot firms' stock prices with respect to earnings. Finally, Section VI concludes.

I. Short-Sale Price Tests, Their Effect on Earnings Management, and Related Research

A. Short-Sale Price Tests in U.S. Equity Markets

Short-sale price tests were initially introduced in the U.S. equity markets in the 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 latter 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).[7] In 1994, the National Association of Securities Dealers (NASD) adopted its own price test (the "bid test") under Rule 3350. Rule 3350 requires that a short sale occur at a price one penny above the bid price if the bid is a downtick from the previous bid.[8]

To facilitate research on the effects of short-sale price tests on financial markets, the SEC initiated a pilot program under Rule 202T of Regulation SHO in July 2004. Under the pilot program, every third stock in the Russell 3000 index ranked by trading volume within each exchange was selected as a pilot stock. From May 2, 2005, to August 6, 2007, pilot stocks were exempted from short-sale price tests. The program effectively ended one month early on July 6, 2007, when the SEC eliminated short-sale price tests for all exchange-listed stocks including the nonpilot stocks.

The decision to eliminate all short-sale price tests prompted a huge backlash from managers and politicians. In 2008, NYSE Euronext commissioned Opinion Research Corporation (2008) to conduct a study to seek corporate

[7] Narrow exceptions apply, as specified in SEC's Rule 10a-1, section (e).

[8] 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 March 12, 2001 to April 9, 2001 period. Prior to decimalization, Rule 3350 required a short sale to occur at a price 1/8th of a dollar (if before June 2, 1997) or 1/16th of a dollar (if after June 2, 1997) above the bid.

issuers' views on short selling. Fully 85% of the surveyed corporate managers favored reinstituting the short-sale price tests "as soon as practical," indicating that managers are aware of and sensitive to the impact of eliminating price tests on the potential amount of short selling in their firms. 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.[9] The *Wall Street Journal* argued that SEC (2007) was too biased to evaluate the short-sale price tests fairly.[10] 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) requiring 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.[11]

B. The Impact of the Pilot Program on Earnings Management

The strong public reactions to changes in the uptick rule indicate that the rule is 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, an assumption we examine further in the next section.[12] In this section, we draw from prior studies to construct our main hypothesis on how changes in the cost of short selling due to the removal of short-sale price tests, and the corresponding changes in the prospect of short selling, affect a manager's tendency to engage in earnings management.

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.[13] These findings imply that managers can earn a personal benefit from managing earnings

[9] Gretchen Morgenson, "Why the roller coaster seems wilder," *The New York Times*, August 26, 2007, page 31.

[10] See "There's a better way to prevent bear raids," *The Wall Street Journal*, November 18, 2008, page A19.

[11] See "Hedge funds slam short-sale rule," available at http://dealbook.nytimes.com/2010/02/25/hedge-funds-slam-short-sale-rule/?_r=0.

[12] See, for example, McCormick and Reilly (1996), Angel (1997), Alexander and Peterson (1999, 2008), SEC (2007), and Diether, Lee, and Werner (2009). For a contradictory finding, see Ferri, Christophe, and Angel (2004).

[13] For evidence regarding compensation motives, see Bergstresser and Philippon (2006), Burns and Kedia (2006), and Efendi, Srivastava, and Swanson (2007); for stock sale motives, see Beneish and Vargus (2002); and for job security and control-related motives, see DeFond and Park (1997), Ahmed, Lobo, and Zhou (2006), DeFond and Jiambalvo (1994), and Sweeney (1994).

to inflate the stock price. Prior research also demonstrates that 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., Miller (1977), Harrison and Kreps (1978), Chang, Cheng, and Yu (2007), Karpoff and Lou (2010), Boehmer and Wu (2013)). These findings imply that managers' benefits of manipulating earnings decrease with the prospect of short selling because these benefits are at least partially offset by short sellers' activities.

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

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 hence an increase in the prospect of short selling in these stocks. The effect was to decrease pilot firm managers' expected benefits and increase their expected costs of earnings management. These effects on a manager's earnings management decisions are illustrated in Figure 1. Let MB_0 and MC_0 represent the manager's marginal benefit and marginal cost of managing earnings before 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. Once the program starts, the marginal benefit and marginal cost of earnings management shift to MB_1 and MC_1, and the manager endogenously adjusts by choosing a new, lower level of earnings management, EM_1. This adjustment among pilot firms leads to our first hypothesis:

HYPOTHESIS 1: Earnings management in the pilot firms decreases relative to earnings management in the nonpilot firms during the pilot program.

C. The Impact of the Pilot Program on Fraud Discovery

In developing Hypothesis 1, we assume that the pilot program had a substantial enough effect on short sellers' activities to induce a measurable change in the pilot firms' financial reporting decisions. Previous research finds that, in general, short selling tracks firms' discretionary accruals and helps uncover

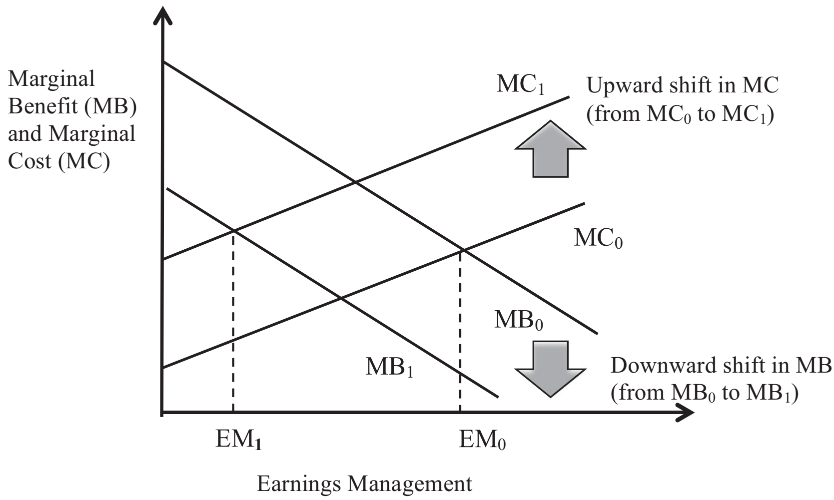

Figure 1. Managers' marginal benefits and marginal costs of earnings management. This figure illustrates Hypothesis 1, which posits that earnings management in the pilot firms decreases relative to earnings management in the nonpilot firms during the pilot program. In the figure, a decrease in the cost of short selling decreases managers' expected benefits from earnings management and increases their expected costs, leading to a decrease in the optimal amount of earnings management. Managers' benefits decrease because the increased prospect of short selling decreases the potential inflation in stock prices that motivates managers to manage earnings in the first place. 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. These changes result 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.

financial misrepresentation.[14] In Section I of the Internet Appendix, we report results that confirm these two findings in our sample, that is, pilot firms' short selling is positively related to their discretionary accruals during the pilot period, and short interest increases in months in which firms are later revealed to have engaged in financial misrepresentation.[15] These results are consistent with the view that the cost reduction induced by the pilot program did provide sufficient incentives for short sellers to increase their scrutiny of the pilot firms' reporting behavior. In this section, we construct a hypothesis and test for whether the pilot program also increased pilot firms' risk of detection for

[14] Desai, Krishnamurthy, and Venkataraman (2006), Cao et al. (2007), Karpoff and Lou (2010), and Hirshleifer, Teoh, and Yu (2011) report that short selling tracks discretionary accruals. Desai, Krishnamurthy, and Venkataraman (2006) find that short selling leads the announcement of earnings restatements, and Karpoff and Lou (2010) find that short selling accelerates the rate at which misrepresentation is detected.

[15] The Internet Appendix is available in the online version of the article on the *Journal of Finance* website.

earnings manipulation that rises to the level of financial misrepresentation or fraud.[16]

We begin by noting that there is generally a time lag between when a firm begins misrepresenting its earnings and when the misrepresentation is detected. Karpoff and Lou (2010) report that this lag varies across firms and has a median of 26 months in their sample. We therefore characterize a firm's conditional probability of being caught as

$$\Pr\left(Caught\left(t+n\right)\left|Fraud\left(t\right)\right.\right)=\delta\ \Sigma_{s=0}^{n}m_{s}SSP\left(t+s\right). \tag{1}$$

In equation (1), $\Pr(Caught(t+n)\,|\,Fraud(t))$ is the firm's probability of being caught at time $t+n$ conditional on misrepresenting at time t, where $n \geq 0$. On the right-hand side, $SSP(t+s)$ is short selling potential at time $t+s$; we expect this potential to be higher for pilot firms when $t+s$ falls within the pilot period. We use m_s to denote the individual weight each period's short-selling potential contributes to the conditional probability of detection. This weight depends on the wide range of non short-selling factors that affect a firm's probability of being caught. We hypothesize that an increase in short-selling potential helps uncover aggressive reporting, that is, $\delta > 0$. This leads to our second hypothesis:

HYPOTHESIS 2: Conditional on misreporting, pilot firms are more likely than nonpilot firms to get caught after the pilot program begins.

A challenge in testing Hypothesis 2 is that we do not directly observe the conditional probability of detection, but rather the unconditional probability that a firm both commits fraud and is detected, which can be expressed as

$$\Pr\left(Caught\left(t+n\right),\ Fraud\left(t\right)\right)=\Pr\left(Fraud\left(t\right)\right)\times\Pr\left(Caught\left(t+n\right)\left|Fraud\left(t\right)\right.\right). \tag{2}$$

To test Hypothesis 2, we exploit the time lag between the commission and detection of fraud. Since the pilot firms are randomly selected, it is reasonable to assume that, before the pilot program was announced in July 2004, the actual rate of fraud commission was equal between the pilot and nonpilot firms, that is, $\Pr(Fraud(t))_{pilot} = \Pr(Fraud(t))_{nonpilot}$ for $t <$ July 2004.[17] This allows us to use the unconditional probability of detection for fraud initiated before the pilot program was announced in July 2004 but detected after the program began in May 2005 to infer the conditional probability of getting caught. Hypothesis 2 then implies that

$$\begin{aligned}&\Pr\left(Caught\left(post\text{-}May\ 2005\right),\ Fraud\left(pre\text{-}July\ 2004\right)\right)_{pilot}>\\&\Pr\left(Caught\left(post\text{-}May\ 2005\right),\ Fraud\left(pre\text{-}July\ 2004\right)\right)_{nonpilot}.\end{aligned}$$

[16] Karpoff et al. (2016) point out that many instances of financial misrepresentation do not include charges of fraud. We nonetheless use the term "fraud" to refer to any illegal misrepresentation that attracts SEC enforcement action.

[17] We restrict t to the period before the announcement of the pilot program (in July 2004) to ensure that the expected rate of fraud commission is equal across the two groups of firms. Whereas short sellers arguably begin to change their behavior after the pilot program is implemented in May 2005, managers of pilot firms could change their reporting behavior in response to the prospect of short selling as early as when they learn the identity of the pilot stocks in July 2004.

Once the pilot program was announced, Hypothesis 1 implies that managers of the pilot firms endogenously began to adjust to the higher conditional probability of detection by decreasing earnings management, that is,

$$\Pr(Fraud(t))_{pilot} < \Pr(Fraud(t))_{nonpilot} \text{ for } t > \text{ July 2004.}$$

The pilot program therefore has two offsetting effects on the unconditional probability of detection for fraud committed after July 2004: pilot firms commit fewer frauds, but conditional on committing fraud, they are more likely to be caught. This implies that the difference between pilot and nonpilot firms in the unconditional likelihood of fraud detection should decrease as we consider fraud initiated after July 2004. Section IV reports results that support these implications of Hypothesis 2.

D. 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 short-selling bans decrease 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 SEC (2007) show that pilot stocks listed on both NYSE and NASDAQ experienced a significant increase in short-sale trades and in the ratio of short sales to share volume 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 apply more active trading strategies. Other papers relate the pilot program to firm outcomes. Grullon, Michenaud, and Weston (2015), 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 (2015) equity incentives, He and Tian (2014) corporate innovation, and Li and Zhang (2015) firms' voluntary disclosure practices.

In our main analyses, we use the 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. nonpilot firms' earnings management 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 nonpilot firms, and mitigate 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 muddled 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 changes in short-selling regulations.

A contemporaneous paper by Massa, Zhang, and Zhang (MZZ, 2015) also investigates the effects of short selling on firms' earnings management. Whereas we use the exogenous variation in firms' short-selling costs created by the pilot program to identify our tests, MZZ focus on 33 international markets and use the amount of shares available for lending to measure short-selling potential. Like us, MZZ also infer that short selling plays a disciplinary role in deterring firms' opportunistic reporting behavior.

II. Data

A. 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 started with 2004 Russell 3000 index members and 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 stocks were then sorted by their average daily dollar volume computed over the June 2003 to May 2004 period within each of the three listing markets. Every third stock (beginning with the second one) within each listing market was designated as a pilot stock.

Based on the description in the SEC's pilot orders and its report on the pilot program (SEC (2007)), we identify an initial sample of 986 pilot stocks and 1,966 nonpilot stocks.[18] An examination of the exchange distribution of these stocks shows that both the pilot and the nonpilot groups are representative of the Russell 3000 index, confirming the statistics reported by SEC (2007). Specifically, of the 986 pilot stocks, 49.9% (492) are listed on NYSE, 47.9% (472) on NASDAQ-NM, and 2.2% (22) on Amex. The exchange distribution of nonpilot stocks is very similar, with 50% (982) listed on NYSE, 48% (944) on NASDAQ-NM, and 2% (40) on Amex.

In our tests, we delete firms in the financial services (SIC 6000–6999) and utilities (SIC 4900–4949) industries because disclosure requirements,

[18] We use Thomson Reuters's Securities Data Company (SDC) Platinum database and the Compustat database to identify firms that went public or had spinoffs after April 30, 2004, and the CRSP monthly files to identify stocks that are not exchange-listed, and exclude all such stocks from the nonpilot sample. The SEC did not publish the final list of nonpilot stocks in its 2007 analysis, but any discrepancies between the SEC's sample and our sample of nonpilot stocks are likely to be immaterial. Further, firms that are not exchange-listed or that had significant changes in ownership structure around the pilot program are likely to be excluded from our tests because our main tests require that the sample firms have financial data each year from 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive).

accounting rules, and processes by which accruals are generated are significantly different for these regulated industries. A further complication with financial stocks is the 2008 short-sale ban imposed on this sector. We obtain data from the Compustat Industrial Annual Files to construct earnings management proxies and control variables. In most tests, we require that firms have data to calculate firm characteristics over the entire sample period, that is, 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive). The resulting balanced panel sample consists of 388 pilot firms and 709 nonpilot firms. If we relax this requirement, our unbalanced panel sample contains 741 to 782 pilot firms and 1,504 to 1,610 nonpilot firms in the year immediately before the announcement of the pilot program (i.e., 2003), depending on 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.

B. 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 nonpilot firms serve as the control sample. The sample period in our main analysis consists of nine calendar years, 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive). We construct three variables to indicate three subperiods: *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. We set the subperiods to three calendar years each so it is easier to align and compare firm financials across periods in the DiD tests, especially since our outcome variable of interest, earnings quality, exhibits seasonality.

Our during-pilot period, 2005 to 2007, is slightly longer than the course of the pilot program, which was scheduled to run from May 2, 2005 to August 6, 2007 but effectively ran from May 2, 2005 to July 6, 2007. This definition of *DURING* reflects our assumption that firms' annual reporting outcome is affected even if the increased prospect of short selling does not extend for the full year. Table IA.III of the Internet Appendix reports tests that yield similar results if we instead define the three subperiods as May 2001 to June 2003, May 2005 to June 2007, and May 2008 to June 2010, thus restricting the *DURING* period more closely to the actual start and end dates of the program. Also, in our primary DiD tests, we omit 2004 because the identity of the pilot and nonpilot stocks was made public in July 2004, and it is not clear whether 2004 should be classified as part of the pre- or during-pilot period. In Table IA.IV of the Internet Appendix, we report tests that indicate our main findings are not substantially affected if we include the entire year of 2004 in the *PRE* period or Q1 to Q3 of 2004 in the *PRE* period and Q4 in the *DURING* period (as most of Q4's financial statements would be released in calendar year 2005).

C. Measures of Earnings Management

Our primary proxy for earnings management is the performance-matched discretionary accruals 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:

$$\frac{TA_{i,t}}{ASSET_{i,t-1}} = \beta_0 + \beta_1 \frac{1}{ASSET_{i,t-1}} + \beta_2 \frac{\Delta REV_{i,t}}{ASSET_{i,t-1}} + \beta_3 \frac{PPE_{i,t}}{ASSET_{i,t-1}} + \varepsilon_{i,t}, \quad (3)$$

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

Next, we use the following model and the estimated coefficients from equation (3) to compute the fitted normal accruals $NA_{i,t}$:

$$NA_{i,t} = \widehat{\beta_0} + \widehat{\beta_1} \frac{1}{ASSET_{i,t-1}} + \widehat{\beta_2} \frac{(\Delta REV_{i,t} - \Delta AR_{i,t})}{ASSET_{i,t-1}} + \widehat{\beta_3} \frac{PPE_{i,t}}{ASSET_{i,t-1}}. \quad (4)$$

Following Dechow, Sloan, and Sweeney (1995), the change in accounts receivable is subtracted from the change in sales revenue as credit sales might also provide a potential opportunity for accounting distortion. After obtaining the fitted normal accruals $NA_{i,t}$ from equation (4), we calculate firm-year-specific discretionary accruals as $DA_{i,t} = (TA_{i,t} \ / \ ASSET_{i,t-1}) - NA_{i,t}$.

Finally, we adjust the estimated discretionary accruals for performance. We match each sample firm with the firm from the same fiscal year-industry that has the closest return on assets as the given firm. The performance-matched discretionary accruals, denoted as *Discretionary accruals*, are then calculated as the firm-specific discretionary accruals minus the discretionary accruals of the matched firm. Note that *Discretionary accruals* is signed and constructed to be positively related to income-increasing earnings management.[19]

D. Firm Characteristics

Similar to Grullon, Michenaud, and Weston (2015), we compare pilot and nonpilot firms' characteristics in the fiscal year immediately before the announcement of the pilot program, 2003. Table I, Panel A, reports descriptive statistics for the balanced panel sample, in which we require that firms have

[19] We create three additional performance-matched discretionary accrual measures by removing the intercept term from equations (3) and (4) and/or replacing $\frac{\Delta REV_{i,t}}{ASSET_{i,t-1}}$ with $\frac{(\Delta REV_{i,t} - \Delta AR_{i,t})}{ASSET_{i,t-1}}$ in

Table I

Firm Characteristics of the Treatment and Control Groups in Fiscal Year 2003

Panel A and Panel B report summary statistics of the firm characteristics for the balanced and the unbalanced panel sample of the treatment and control groups, respectively. All characteristics are measured in 2003, the year immediately before Regulation SHO's pilot program was announced. The balanced sample comes from the 2004 Russell 3000 index and contains firms that have data available to calculate firm characteristics and discretionary accruals over the entire sample period (i.e., annual data from 2001 to 2003 and 2005 to 2010). The unbalanced sample is similar to the balanced sample but only requires data to be available to calculate a firm characteristic of interest in a given year. 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. Variable definitions are provided in the Appendix. 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.

Panel A: Treatment and Control Groups in the Balanced Panel Sample

	Treatment Group (*PILOT* = *1*)				Control Group (*PILOT* = *0*)			
	N	Mean	Median	*SD*	*N*	Mean	Median	*SD*
ASSET	388	3,748.61	817.69	8,512.30	709	3,746.25	817.42	8,647.29
MB	388	2.75	1.95	3.79	709	2.60	1.98	3.13
ASSETGR	388	13.42	7.88	31.28	709	13.22	7.66	30.18
CAPEX	388	5.55	3.76	5.54	709	5.50	3.65	5.88
R&D	388	4.19	0.00	8.40	709	4.04	0.32	7.83
ROA	388	14.37	14.51	12.70	709	14.15	14.29	14.30
CFO	388	11.36	11.33	10.68	709	10.56	10.46	13.26
LEV	388	29.36	26.46	27.50	709	29.80	27.56	28.25
CASH	388	21.07	12.22	25.76	709	21.81	10.66	27.87
DIVIDENDS	388	0.83	0.00	1.49	709	0.73	0.00	1.33

	Tests for differences between *PILOT* = *1* and *PILOT* = *0*	
	t-statistic	Wilcoxon *z*-statistic
ASSET	0.00	0.51
MB	0.68	0.03
ASSETGR	0.10	−0.30
CAPEX	0.14	0.91
R&D	0.27	−0.94
ROA	0.25	0.22
CFO	1.10	1.10
LEV	−0.25	−0.24
CASH	−0.44	−0.32
DIVIDENDS	1.14	1.07

(*Continued*)

data available to calculate financial characteristics and accrual measures in all years of the sample period. The mean book value of assets in both groups

equation (3). The results using these alternative measures are reported in Table IA.V of the Internet Appendix and are consistent with those reported in the paper.

Table I—*Continued*

Panel B: Treatment and Control Groups in the Unbalanced Panel Sample

	Treatment Group (*PILOT* = *1*)				Control Group (*PILOT* = *0*)			
	N	Mean	Median	*SD*	*N*	Mean	Median	*SD*
ASSET	782	2,918.37	726.92	7,471.08	1,610	2,941.31	669.22	7,883.20
MB	759	2.66	1.89	3.84	1,534	2.55	1.85	4.15
ASSETGR	781	17.81	8.82	40.64	1,605	17.61	8.45	43.10
CAPEX	741	5.59	3.61	6.21	1,504	5.28	3.31	5.98
R&D	781	5.78	0.00	11.96	1,605	6.10	0.22	12.22
ROA	778	10.67	12.79	18.92	1,604	9.68	11.87	20.88
CFO	742	8.19	9.88	17.20	1,505	7.20	9.24	19.20
LEV	781	30.82	28.06	29.18	1,602	31.54	27.27	31.21
CASH	781	26.45	13.03	35.86	1,605	27.17	12.17	37.37
DIVIDENDS	778	0.73	0.00	1.55	1,600	0.77	0.00	1.65

	Tests for differences between *PILOT* = *1* and *PILOT* = *0*	
	t-statistic	Wilcoxon z-statistic
ASSET	−0.07	1.26
MB	0.60	1.05
ASSETGR	0.11	0.38
CAPEX	1.13	1.74*
R&D	−0.61	−1.06
ROA	1.15	1.23
CFO	1.23	1.39
LEV	−0.55	0.11
CASH	−0.48	−0.66
DIVIDENDS	−0.69	−1.13

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, R&D expenditures to assets, annual return on assets, cash flow to assets, 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 argument 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 I reports similar comparisons for the larger unbalanced panel sample. Firms in this sample are slightly smaller than those in the balanced panel sample, with assets averaging $2.9 billion versus $3.7 billion. As in Panel A, the pilot and nonpilot firms in the unbalanced panel sample are similar to each other along the financial characteristics we examine. The sole exception is that the median capital expenditure of pilot firms is slightly higher than that of nonpilot firms.

III. The Effect of Regulation SHO's Pilot Program on Earnings Management

A. Discretionary Accruals

Table II reports the results of univariate DiD tests examining Hypothesis 1 using our primary measure of earnings management based on discretionary accruals. Panel A reports results for the balanced panel sample defined in Section II.A. The mean *Discretionary accruals* during the three-year period before the pilot program (2001 to 2003) is −0.004 for both pilot and nonpilot 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, both insignificant. During the three-year period of the pilot program (2005 to 2007), the mean *Discretionary accruals* decreases to −0.014 for pilot firms while it remains at −0.004 for nonpilot firms. The mean difference is −0.011 (*t*-statistic = −2.09) and the median difference is −0.008 (Wilcoxon *z*-statistic = −2.23), both significant at the 5% level. For the three-year period after the pilot program (2008 to 2010), *Discretionary accruals* increases for the pilot firms to a mean of zero while it changes slightly for the nonpilot firms to −0.003. The mean difference is 0.004 (*t*-statistic = 0.69) and the median difference is 0.001 (Wilcoxon *z*-statistic = 0.66), both insignificant. The bottom-left cell of Table II, Panel A, reports the time-series estimators, which track the change in *Discretionary accruals* within each group of firms across the three periods. The second column shows that the average *Discretionary accruals* for pilot firms drops by −0.011 (significant at the 5% level) from the pre- to during-pilot period, but increases 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 *Discretionary accruals* from the pre- to post-pilot period is 0.003 and insignificant. In contrast, the estimators in the fourth column are never significant, suggesting that nonpilot firms' *Discretionary accruals* do not change much over time.

The bottom-right cell of Table II, Panel A, reports results on the DiD estimators. The mean DiD estimator for *Discretionary accruals* from before to during the pilot program is −0.011 with a *t*-statistic of −1.67. This difference is statistically significant 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. The DiD estimator that tracks *Discretionary accruals* from during to after the pilot program is 0.013 with a *t*-statistic of 2.06. In addition, the DiD estimator that compares *Discretionary accruals* 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 plot these univariate results in Figure 2 to better illustrate the pattern in discretionary accruals. As the figure shows, nonpilot firms' discretionary accruals do not change much over the sample period. The pilot firms' discretionary accruals, similar to those of the nonpilot firms before the pilot program,

Table II

Discretionary Accruals before, during, and after the Pilot Program

The top half of Panel A reports summary statistics on the level of annual discretionary accruals for the balanced panel sample of the treatment and control groups for the three-year periods before, during, and after Regulation SHO's pilot program, and differences in the mean or median. The bottom half of Panel A reports univariate results of difference-in-differences (DiD) tests. The sample comes from the 2004 Russell 3000 index and contains firms that have data available to calculate firm characteristics and discretionary accruals over the entire sample period (i.e., 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive)). 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. Panel B reports summary statistics on the level of discretionary accruals for the unbalanced panel sample of the treatment and control groups. This sample also comes from the 2004 Russell 3000 index and contains firms that have data available to calculate discretionary accruals in a given year. Variable definitions are provided in the Appendix. ***, **, 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	
	N	Mean	Median	*N*	Mean	Median	Difference in Mean	Difference in Median
Discretionary accruals								
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
ΔDiscretionary accruals						
DURING-PRE	388	−0.011**	709	0.000	−0.011	−1.67*
POST-DURING	388	0.013***	709	0.001	0.013	2.06**
POST-PRE	388	0.003	709	0.001	0.002	0.32

Panel B: Unbalanced Panel Sample

	Treatment Group (*PILOT* = *1*)			Control Group (*PILOT* = *0*)			Cross-Sectional Estimator	
	N	Mean	Median	*N*	Mean	Median	Difference in Mean	Difference in Median
Discretionary accruals								
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

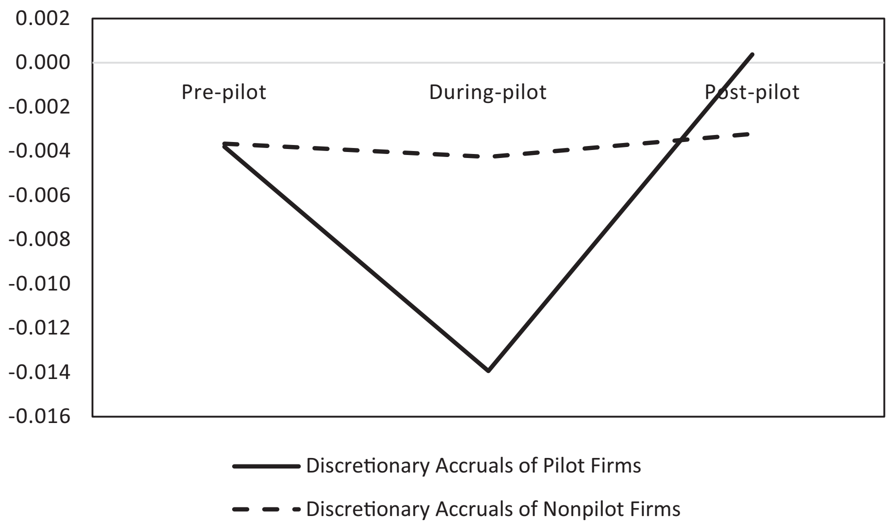

Figure 2. Discretionary accruals for pilot vs. nonpilot firms. This figure displays the results reported in Panel A of Table II. It depicts the mean *Discretionary accruals* for the balanced panel sample of the treatment group and control group for the periods before, during, and after Regulation SHO's pilot program, that is, 2001 to 2003, 2005 to 2007, and 2008 to 2010. The sample comes from the 2004 Russell 3000 index and contains firms that have data available to calculate financial characteristics and discretionary accruals over the entire sample period (i.e., 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive)).

decrease significantly during the program and then revert to levels that are similar to those of the nonpilot firms after the program.

Panel B of Table II reports on the changes in *Discretionary accruals* using data from the unbalanced panel sample, in which we do not require that firms 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 unbalanced sample.

Next, we extend the DiD test using multivariate regressions. To do so, we retain firm-year observations for both pilot and nonpilot firms for the nine-year window (2001 to 2003 (inclusive) and 2005 to 2010 (inclusive)) around Regulation SHO's pilot program and estimate the following model:

$$\begin{aligned} \textit{Discretionary accruals}_{i,t} &= \beta_0 + \beta_1 \textit{PILOT}_i \times \textit{DURING}_t + \beta_2 \textit{PILOT}_i \\ &\quad \times \textit{POST}_t + \beta_3 \textit{PILOT}_i + \beta_4 \textit{DURING}_t + \beta_5 \textit{POST}_t + \varepsilon_{i,t}. \end{aligned} \quad (5)$$

The variables are as defined in Sections II.B and II.C. The benchmark period consists of the three years before the pilot program (2001 to 2003). As previously discussed, 2004 is omitted from these tests because the identity of the pilot stocks was announced midway through 2004. The regression results estimating equation (5) are reported in column (1) of Table III. The coefficients of interest are the two DiD estimators, β_1 and β_2. The coefficient on *PILOT*×*DURING*, β_1, is negative and significant at the 5% level. The

Table III

The Effect of Pilot Program on Discretionary Accruals

This table reports OLS regression results on differences in pilot and nonpilot firms' discretionary accruals for the periods before, during, and after Regulation SHO's pilot program, using a balanced panel sample. The sample comes from the 2004 Russell 3000 index and contains firms that have data available to calculate firm characteristics and discretionary accruals over the entire sample period (i.e., 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive)). 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 using annual data: $Discretionary\ accruals_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \beta_3 PILOT_i + \beta_4 DURING_t + \beta_5 POST_t + \varepsilon_{i,t}$ in column (1). We augment the model by including *SIZE*, *MB*, *ROA*, and *LEV* in column (2) and by further including year fixed effects for 2002 to 2003 and 2005 to 2010 in column (3). We omit *PILOT* and *POST* in column (3) to avoid multicollinearity. Variable definitions are provided in the Appendix. Standard errors clustered by year and firm are displayed in parentheses. 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.

	$Discretionary\ accruals_t$		
	(1)	(2)	(3)
$PILOT \times DURING_t$	−0.010**	−0.010**	−0.010**
	(0.004)	(0.004)	(0.004)
$PILOT \times POST_t$	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)
$DURING_t$	−0.001	−0.001	
	(0.002)	(0.002)	
$POST_t$	0.000	−0.001	
	(0.005)	(0.005)	
$SIZE_t$		0.002*	0.002*
		(0.001)	(0.001)
MB_t		−0.001	−0.001
		(0.001)	(0.001)
ROA_t		−0.041**	−0.041***
		(0.016)	(0.016)
LEV_t		−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
No. of obs.	9,873	9,873	9,873
Adjusted R^2	0.10%	0.40%	0.40%

magnitude of β_1 is consistent with the univariate DiD results reported in Table II and indicates that *Discretionary accruals* (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 pre-pilot period. This corresponds to 7.4% of the standard deviation of *Discretionary accruals* in the pooled sample, 0.135.

The coefficient on $PILOT \times POST$, β_2, is insignificant. This result once again demonstrates a reverting pattern because the difference between the pilot and non-pilot firms' discretionary accruals after the pilot program is not statistically different from that before the program. Also, the coefficient on $PILOT$, β_3, is insignificant, indicating that pilot and nonpilot firms exhibit similar levels of discretionary accruals before the pilot program. Consistent with prior research, the regression R^2 is low, indicating that most of the cross-sectional differences in discretionary accruals are due to unmodeled factors.

In column (2), we augment equation (5) by including four controls previously 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 (*MB*), return on assets (*ROA*), and leverage (*LEV*). In column (3), we further include year fixed effects from 2002 to 2003 and from 2005 to 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.

B. Alternative Measures

In this section, we examine whether our results based on discretionary accruals are robust to using two alternative measures of earnings management. First, we examine whether the pilot program differentially affected firms' likelihood of meeting or marginally beating the analyst consensus forecast. Graham, Harvey, and Rajgopal (2005) report that a large majority of CFOs consider it important to beat the analyst consensus forecast and are willing to engage in earnings manipulation to do so. For this reason, many papers (e.g., Bhojraj et al. (2009)) infer earnings management from the tendency of firms to meet or beat the analyst consensus by up to one cent.

We follow prior research and run the following probit model on a panel of quarterly earnings announcements that take place during the same nine-year window (2001 to 2003 (inclusive) and 2005 to 2010 (inclusive)):

$$\begin{aligned} BEAT\_ALY_{i,q} = \beta_0 + \beta_1 PILOT_i \times DURING_q + \beta_2 PILOT_i \times POST_q \\ + \beta_3 PILOT_i + \beta_4 DURING_q + \beta_5 POST_q + \varepsilon_{i,q}. \end{aligned} \tag{6}$$

Subscript q indexes fiscal quarters. The dependent variable *BEAT_ALY* is coded one for quarters in which the firm's reported EPS meets or beats the most recent analyst consensus EPS forecast before the earnings announcement by up to one cent. Both reported EPS and analyst forecasts are retrieved from I/B/E/S. To calculate analyst consensus, we take each analyst's latest forecast issued within 90 days of the fiscal quarter-end and before the earnings announcement, and require a firm-quarter to have at least three analysts. We define *PILOT* as before but the two time indicators are now based on the earnings announcement date, that is, $DURING_q$ ($POST_q$) equals one if quarter

q's earnings announcement takes place during (post) the pilot period, and zero otherwise.

The regression results estimating equation (6) are reported in column (1) of Table IV. Consistent with our earlier results using discretionary accruals, the coefficient on our main variable of interest–the DiD estimator (*PILOT*×*DURING*) – is negative and significant at the 5% level. The marginal effect of β_1, calculated using the methodology suggested in Ai and Norton (2003), indicates that the likelihood of marginally beating the analyst consensus is 1.8 percentage points lower for the treatment group than for the control group during the three-year period of the pilot program compared to the three-year pre-pilot period. This corresponds to 11.1% of the unconditional likelihood of *BEAT_ALY* in the sample, 16.2%.[20] The coefficient on the second DiD estimator (*PILOT*×*POST*), β_2, continues to be insignificant, indicating that the difference between pilot and nonpilot firms' likelihood of marginally beating the analyst consensus in the post-pilot period is not statistically different from that pre-pilot. The coefficient on *PILOT*, β_3, also remains insignificant, consistent with pilot firms and nonpilot firms having a similar likelihood of marginally beating the analyst consensus pre-pilot.

In column (2), we include a list of controls previously shown to affect the likelihood of beating the analyst consensus. We largely follow Edmans, Fang, and Lewellen (2015) to construct these controls, which include the log of market capitalization (*MV*), market-to-book (*MB*), return on assets (*ROA*), the log of analyst coverage (*ALY_N*), the log of the average forecast horizon (*ALY_HORIZON*), and analyst forecast dispersion (*ALY_DISP*). We also include two proxies for real earnings management: the change in R&D expenditures from quarter $q-4$ to quarter q, $\Delta R\&D_q$, and the change in capital expenditures, $\Delta CAPEX_q$, both scaled by total assets at the beginning of quarter q. Among these controls, the market-to-book ratio is positively related to the likelihood of marginally beating the analyst consensus while forecast horizon, forecast dispersion, and an increase in R&D spending are negatively related to this likelihood. The inclusion of these controls, however, does not significantly alter the magnitude or significance of the coefficients on the pilot-related variables.

The time dummy variables *DURING* and *POST* both exhibit significantly negative coefficients in columns (1) and (2). This result is consistent with the view that Regulation FD in 2000 reduced firms' ability to guide analyst forecasts toward reported earnings. In Table IA.VI of the Internet Appendix, we repeat the analysis including quarterly fixed effects to control for secular changes in firms' tendency to meet or marginally beat the analyst consensus forecast. The results, particularly for the pilot-related variables, are similar to those reported in Table IV.

[20] Ai and Norton (2003) argue that the magnitude of the interaction effect in nonlinear regressions may not equal its marginal effect and propose a way to correct for the interaction term's magnitude and standard error. Le (1998) and Kolasinski and Siegel (2010), however, argue that the coefficient on the interaction term is relevant even in a nonlinear regression, especially when used to measure proportional rather than absolute marginal effects. In our setting, the interaction effect calculated using the conventional linear method is only slightly larger (2% vs. 1.8%).

Table IV

The Effect of Pilot Program on Firm's Likelihood of Beating Earnings Targets

This table reports probit regression results on differences in pilot and nonpilot firms' likelihood of meeting or marginally beating the quarterly analyst consensus forecast (or the reported earnings for the same quarter of the prior year) for the periods before, during, and after Regulation SHO's pilot program. The sample comes from the 2004 Russell 3000 index and contains firms that have data available for analyst forecast related variables (or reported EPS in the same quarter of the prior year), and controls during the sample period (i.e., 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive)). 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 using quarterly data: $BEAT\_ALY\ (BEAT\_EPS)_{i,q} = \beta_0 + \beta_1 PILOT_i \times DURING_q + \beta_2 PILOT_i \times POST_q + \beta_3 PILOT_i + \beta_4 DURING_q + \beta_5 POST_q + \varepsilon_{i,q}$ in column (1) (column (3)). We augment the model by including *MV*, *MB*, *ROA*, *ALY_N*, *ALY_HORIZON*, *ALY_DISP*, $\Delta R\&D$, and $\Delta CAPEX$ in column (2) and *MV*, *MB*, *ROA*, $\Delta R\&D$, and $\Delta CAPEX$ in column (4). Variable definitions are provided in the Appendix. Standard errors clustered by quarter-end and firm are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	$BEAT\_ALY_q$		$BEAT\_EPS_q$	
	(1)	(2)	(3)	(4)
$PILOT \times DURING_q$	-0.081^{**}	-0.079^{**}	-0.073^{*}	-0.074^{*}
	(0.040)	(0.040)	(0.043)	(0.044)
$PILOT \times POST_q$	0.020	0.017	0.001	−0.003
	(0.043)	(0.043)	(0.032)	(0.033)
PILOT	0.028	0.022	0.041^{*}	0.045^{*}
	(0.028)	(0.029)	(0.023)	(0.024)
$DURING_q$	-0.217^{***}	-0.219^{***}	0.081^{**}	0.089^{**}
	(0.048)	(0.050)	(0.038)	(0.039)
$POST_q$	-0.461^{***}	-0.437^{***}	-0.072^{**}	-0.062^{*}
	(0.055)	(0.056)	(0.033)	(0.032)
MV_q		0.002		-0.065^{***}
		(0.010)		(0.007)
MB_q		0.018^{***}		0.006^{**}
		(0.003)		(0.003)
ROA_q		−0.632		1.389^{***}
		(0.412)		(0.324)
ALY_N_q		−0.020		
		(0.024)		
$ALY\_HORIZON_q$		-0.031^{**}		
		(0.015)		
ALY_DISP_q		-0.202^{***}		
		(0.040)		
$\Delta R\&D_q$		-1.865^{**}		−0.245
		(0.865)		(0.423)
$\Delta CAPEX_q$		−0.140		0.340
		(0.988)		(0.379)
INTERCEPT	-0.759^{***}	0.680^{***}	-1.682^{***}	-1.233^{***}
	(0.042)	(0.057)	(0.024)	(0.057)
No. of obs.	28,626	28,341	59,846	59,589
Pseudo R^2	1.87%	2.21%	0.15%	0.64%

Graham, Harvey, and Rajgopal (2005) report that an alternative benchmark that firms might try to beat is the firm's EPS in the same quarter of the prior year. We therefore repeat our tests using *BEAT_EPS*, an indicator that equals one if the firm's quarterly EPS meets or beats the prior year same quarter's EPS by up to one cent. The results are tabulated in columns (3) and (4) of Table IV. The results are qualitatively similar to those using *BEAT_ALY*, although the coefficients on *PILOT*×*DURING* are significant at only the 10% level. The weaker statistical significance is consistent with Brown and Caylor's (2005) finding that analyst consensus, and not prior-year earnings, has become the most important earnings target for U.S. firms in recent years. Nonetheless, the economic significance remains sizable, as the likelihood of marginally beating the prior year's same-quarter EPS is 0.8 percentage points lower for the treatment group than for the control group during the three-year period of the pilot program compared to the three-year pre-pilot period. This represents a 14.2% difference based on the unconditional likelihood of *BEAT_EPS* in the sample, which is 5.3%.

Next, we examine whether the pilot program differentially affected firms' likelihood of being classified as a misstating firm based on the *F*-score. The *F*-score is the predicted probability of a misstatement using fitted values from a model developed by Dechow et al. (2011), who use the model to characterize firms subject to SEC enforcement actions for financial misconduct that include one or more Accounting and Auditing Enforcement Release (AAER). The model includes balance sheet items (which are intended to capture accruals quality and financial performance), and/or nonfinancial measures, off-balance-sheet activities, and market-based measures. The advantage of this measure is that it is correlated with an ex post indication of earnings management, namely, the incidence of SEC enforcement action for earnings manipulation.

We calculate three versions of the *F*-score, *F1–F3*, using the three different sets of coefficient estimates provided in Dechow et al. (2011). The first set of coefficients is obtained from their Model (1), which includes accruals quality and financial performance measures. The second set comes from their Model (2), which includes all variables in Model (1) plus nonfinancial measures and off-balance-sheet activities. The third set comes from their Model (3), which further includes market-based measures. We then define a binary variable, *HF1*, which stands for "High *F*-score" and equals one if the firm's *F1* is greater than or equal to the 99th percentile of the sample. We define *HF2* and *HF3* similarly based on Dechow et al.'s (2011) second and third models.

The results, replacing *Discretionary accruals* with *HF1-HF3* in equation (5), are reported in Table V. As can be seen, the coefficients on our main DiD estimator of interest, *PILOT*×*DURING*, are once again negative and significant at the 5% level in all columns. The marginal effect in column (1) indicates that the likelihood of being classified as a misstating firm is 0.4 percentage points lower for the treatment group than for the control group during the three-year period of the pilot program compared to the three-year pre-pilot period. As before, the coefficients on *PILOT* are insignificant, indicating that pilot and nonpilot firms have similar probabilities of being classified as misstating firms before the

Table V

The Effect of Pilot Program on Firm's Likelihood of Misstating Based on *F*-Scores

This table reports probit regression results on differences in pilot and nonpilot firms' likelihood of being classified as misstating based on *F*-scores of Dechow et al. (2011) for the periods before, during, and after Regulation SHO's pilot program. The sample comes from the 2004 Russell 3000 index and contains firms that have data available to calculate *F*-scores and controls over the entire sample period (i.e., 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive)). 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 using annual data: *HF1* (or *HF2, HF3*)$_{i,t}$ $= \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \beta_3 PILOT_i + \beta_4 DURING_t + \beta_5 POST_t + \varepsilon_{i,t}$ in columns (1), (3), and (5). We augment the model by including *SIZE*, *MB*, *ROA*, and *LEV* in columns (2), (4), and (6). Variable definitions are provided in the Appendix. Standard errors clustered by year and firm are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	$HF1_t$		$HF2_t$		$HF3_t$	
	(1)	(2)	(3)	(4)	(5)	(6)
$PILOT \times DURING_t$	−0.178**	−0.174**	−0.189**	−0.187**	−0.200**	−0.196**
	(0.080)	(0.081)	(0.079)	(0.080)	(0.080)	(0.081)
$PILOT \times POST_t$	−0.177**	−0.170*	−0.186**	−0.181**	−0.169**	−0.167*
	(0.087)	(0.088)	(0.087)	(0.088)	(0.086)	(0.087)
PILOT	−0.001	−0.011	−0.006	−0.016	−0.004	−0.012
	(0.035)	(0.036)	(0.036)	(0.036)	(0.038)	(0.039)
$DURING_t$	−0.976***	−1.024***	−0.942***	−0.989***	−0.974***	−1.015***
	(0.046)	(0.047)	(0.045)	(0.046)	(0.046)	(0.046)
$POST_t$	−0.941***	−0.989***	−0.928***	−0.977***	−0.887***	−0.939***
	(0.049)	(0.050)	(0.048)	(0.050)	(0.048)	(0.049)
$SIZE_t$		0.079***		0.084***		0.072***
		(0.017)		(0.017)		(0.017)
MB_t		0.021***		0.020***		0.014**
		(0.006)		(0.006)		(0.006)
ROA_t		0.093		−0.000		0.026
		(0.168)		(0.167)		(0.170)
LEV_t		0.127		0.108		0.294***
		(0.084)		(0.083)		(0.088)
INTERCEPT	−0.081***	−0.735***	−0.063***	−0.726***	−0.004	−0.628***
	(0.021)	(0.111)	(0.022)	(0.110)	(0.023)	(0.111)
No. of obs.	9,871	9,871	9,871	9,871	9,871	9,871
Pseudo R^2	11.5%	12.5%	11.0%	12.1%	10.8%	12.0%

pilot program began. However, the results using the higher *F*-score variables do differ in one respect from those using discretionary accruals or the meet or beat measures of earnings management: the coefficients on *PILOT*×*POST* are negative and statistically significant. This result suggests that there is a prolonged effect of the pilot program on pilot firms' earnings quality when it is measured by the high *F*-score variables. We make this inference with caution, however, because the balance sheet items used to calculate the *F*-score include accruals and other variables that can be linked to investment and growth. It is possible that our *F*-score results reflect the pilot program's effect on firm investment as well as its effect on earnings management. We address this concern for our primary tests in Section III.C below.

C. Alternative Explanations

So far, our results indicate that the increase in the prospect of short selling due to the removal of short-sale price tests is associated with a significant decrease in pilot firms' earnings management. Hypothesis 1 implies that these results reflect how the prospect of short selling curbs firms' opportunistic reporting behavior. In this section, we evaluate several alternatives to this explanation.

C.1. Growth, Investment, and Equity Issuance

Grullon, Michenaud, and Weston (2015) document that financially constrained pilot firms significantly reduce their investment and equity issuance during the pilot program. So it is possible that pilot firms' tendency to decrease earnings management during the pilot program reflects changes in the difference between pilot and nonpilot firms' investment and/or equity issuance around the pilot program. This concern is particularly pertinent when discretionary accruals are used to measure earnings management, because prior research shows that a firm's accruals correlate with its growth (e.g., Fairfield, Whisenant, and Yohn (2003), Zhang (2007), Wu, Zhang, and Zhang (2010)) and its incentives to issue equity (e.g., Friedlan (1994), Teoh, Welch, and Wong (1998a, 1998b)).

To investigate this concern, we adopt several controls for firms' investment. We begin by re-estimating equation (5) controlling for R&D expenditures (*R&D*) and capital expenditures (*CAPEX*), both scaled by lagged total assets. The results are reported in Table VI. In column (1), we include *R&D* and *CAPEX* separately. In column (2), we include the sum of the two, *INVESTMENT*. The coefficients on the two DiD estimators, *PILOT*×*DURING* and *PILOT*×*POST*, are barely affected by the inclusion of these controls. In columns (3) and (4), we further include squared terms of the investment variables to account for the possibility that the effect of investment on accruals may be nonlinear. The main results remain similar. In column (5), we modify the Jones model by adding the market-to-book ratio to equations (3) and (4) when calculating the performance-matched discretionary accruals. That is, total accruals are modeled as a function of the market-to-book ratio in addition to the changes in revenues (or revenues adjusted for accounts receivable in equation (4)) and PPE (both scaled by total assets). The results are again similar to those reported in Table III.

As an additional check for any investment effect on accruals, we examine changes in the investment variables around the pilot program for the two groups of firms. If discretionary accruals reflect only growth, investment should

Table VI

The Effect of Pilot Program on Discretionary Accruals Controlling for Investment

This table reports OLS regression results on differences in pilot and nonpilot firms' discretionary accruals for the periods before, during, and after Regulation SHO's pilot program, using a balanced panel sample. The sample comes from the 2004 Russell 3000 index and contains firms that have data available to calculate firm characteristics and discretionary accruals over the entire sample period (i.e., 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive)). 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 using annual data: $Discretionary\ accruals_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \beta_3 PILOT_i + \beta_4 DURING_t + \beta_5 POST_t + \beta_6 SIZE_{i,t} + \beta_7 MB_{i,t} + \beta_8 ROA_{i,t} + \beta_9 LEV_{i,t} + \varepsilon_{i,t}$. We include *R&D* and *CAPEX* in column (1), *INVESTMENT* in column (2), and further include their squared terms in columns (3) and (4). In column (5), we replace the dependent variable *Discretionary accruals* with *Discretionary accruals_MBadj*. Variable definitions are provided in the Appendix. Standard errors clustered by year and firm are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	Discretionary accruals$_t$				*Discretionary accruals_MBadj$_t$*
	(1)	(2)	(3)	(4)	(5)
PILOT×DURING$_t$	−0.010**	−0.010**	−0.010**	−0.010**	−0.018***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
PILOT×POST$_t$	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$_t$	−0.001	−0.001	−0.001	−0.001	0.002
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
POST$_t$	−0.002	−0.003	−0.002	−0.003	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
SIZE$_t$	0.001	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
MB$_t$	−0.000	−0.000	−0.000	−0.000	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
ROA$_t$	−0.062***	−0.053***	−0.064***	−0.058***	−0.044**
	(0.014)	(0.015)	(0.014)	(0.015)	(0.019)
LEV$_t$	−0.017**	−0.016*	−0.016*	−0.015*	−0.013
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)
R&D$_t$	−0.001***		−0.001*		
	(0.000)		(0.000)		
CAPEX$_t$	−0.001***		−0.001**		
	(0.000)		(0.000)		
INVESTMENT$_t$		−0.001***		−0.001**	
		(0.000)		(0.000)	
R&D$_t$ ^{2}			−0.000		
			(0.000)		
CAPEX$_t$ ^{2}			0.000		
			(0.000)		
INVESTMENT$_t$ ^{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)
No. of obs.	9,849	9,873	9,873	9,873	9,206
Adjusted R^2	1.06%	1.02%	1.10%	1.10%	0.50%

follow a pattern around the pilot program that is similar to the pattern in *Discretionary accruals*. In Table IA.VII of the Internet Appendix, we reestimate equation (5) replacing *Discretionary accruals* with *CAPEX* in column (1) and *INVESTMENT* in column (2). Our results in column (1) are consistent with Grullon, Michenaud, and Weston's (2015) finding that capital expenditures decreased for pilot firms relative to nonpilot firms during the pilot program. However, pilot and nonpilot firms' capital expenditures do not appear to converge when the pilot program ends, as the coefficient on *PILOT*×*POST* is significantly negative and of a larger magnitude than that on *PILOT*×*DURING*. We also find no evidence that pilot firms' overall *INVESTMENT* decreases during the pilot program, which is consistent with Grullon, Michenaud, and Weston's (2015) finding that investment decreases during the pilot program only among the financially constrained pilot firms.

We also examine whether our findings regarding pilot firms' discretionary accruals are concentrated among firms that seek to issue equity during our sample period. To do so, we partition the sample according to whether firms are likely to issue equity during the pilot period. As Kadan et al. (2009) point out, recent equity issuance is positively correlated with the ex ante likelihood that a firm will issue equity again. We thus classify firms that issued equity at least once in the two prior fiscal years, as recorded in Thomson Reuters' SDC Platinum database, as *Equity Issuers*, and firms that did not as *Nonequity Issuers*. As an alternative test, we also partition the sample based on whether, ex post, the firm issues equity in the given year. This alternative approach has the advantage of identifying *Equity Issuers* based on firms' actual equity issuance. A drawback, however, is that some potential issuers could be mistakenly classified as *Nonequity Issuers* if they refrain from issuing equity because of the pilot program.

The results are reported in Table VII. The first four columns report results when the sample is partitioned on the ex ante measure of firms' desire to issue equity, and the last four columns report the results using the ex post measure. Using either partition, the coefficients on *PILOT*×*DURING* is statistically significant only among the subsample of *Nonequity Issuers*. The coefficients on *PILOT*×*DURING* for the subsample of *Equity Issuers* range from −0.027 to 0.006 but are not statistically significant in either panel. These results indicate that the effect of the pilot program on discretionary accruals is widespread and is not limited to firms that are likely to issue equity. Overall, the results in this section do not support the view that the pattern in discretionary accruals that we document is driven by changes in firms' investment levels and/or equity issuance.[21]

[21] The discretionary accruals measure is industry-adjusted. As a result, it is possible that our finding that pilot firms' discretionary accruals reverted to pre-program levels after the pilot program could reflect changes in nonpilot firms' accruals rather than changes in pilot firms' accruals. We examine this concern in Section III of the Internet Appendix and find that the convergence in pilot and nonpilot firms' discretionary accruals after the pilot program does partly reflect a decrease in total accruals among nonpilot firms.

Table VII

The Effect of Pilot Program on Discretionary Accruals Partitioned on Seasoned Equity Offering

This table reports OLS regression results on differences in pilot and nonpilot firms' discretionary accruals for the periods before, during, and after Regulation SHO's pilot program, separately for the subsample of equity issuers and the subsample of nonequity issuers. The sample comes from the 2004 Russell 3000 index and contains firms that have data available to calculate firm characteristics and discretionary accruals over the entire sample period (i.e., 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive)). In columns (1)-(4), we define a firm as an *Equity Issuer* if the firm issued equity at least once during the two prior fiscal years, as recorded in Thomson Reuters's SDC Platinum database, and as a *Nonequity Issuer* otherwise. In columns (5)-(8), we define a firm as an *Equity Issuer* if the firm issues equity at least once during a given fiscal year, as recorded in the Thomson Reuters Securities Data Company (SDC) Platinum database, and *Nonequity Issuer* otherwise. 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 using annual data: $Discretionary\ accruals_{i,t} = \beta_0 + \beta_1 PILOT_i \times DURING_t + \beta_2 PILOT_i \times POST_t + \beta_3 PILOT_i + \beta_4 DURING_t + \beta_5 POST_t + \varepsilon_{i,t}$ in columns (1), (3), (5), and (7). We augment the model by including *SIZE*, *MB*, *ROA*, and *LEV* in columns (2), (4), (6), and (8). Variable definitions are provided in the Appendix. Standard errors clustered by year and firm are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	$Discretionary\ accruals_t$							
	Partition based on past equity issuance				Partition based on current equity issuance			
	Equity Issuers		Nonequity Issuers		Equity Issuers		Nonequity Issuers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$PILOT \times DURING_t$	0.004	0.006	−0.011**	−0.012**	−0.027	−0.020	−0.009***	−0.010***
	(0.008)	(0.008)	(0.005)	(0.005)	(0.036)	(0.038)	(0.003)	(0.003)
$PILOT \times POST_t$	−0.036	−0.035	0.007	0.006	0.007	0.006	0.003	0.003
	(0.028)	(0.028)	(0.005)	(0.005)	(0.052)	(0.052)	(0.006)	(0.006)
$PILOT$	0.003	0.003	−0.001	−0.001	−0.016	−0.016	0.001	0.002
	(0.006)	(0.006)	(0.004)	(0.004)	(0.012)	(0.012)	(0.002)	(0.002)
$DURING_t$	−0.001	−0.002	−0.001	−0.002	−0.002	−0.009	0.000	−0.000
	(0.010)	(0.010)	(0.002)	(0.003)	(0.036)	(0.038)	(0.001)	(0.002)
$POST_t$	0.013**	0.011	−0.001	−0.003	−0.005	−0.011	0.001	0.000
	(0.006)	(0.009)	(0.006)	(0.006)	(0.014)	(0.016)	(0.006)	(0.006)
$SIZE_t$		−0.002		0.002**		−0.001		0.002**
		(0.002)		(0.001)		(0.005)		(0.001)
MB_t		−0.002		−0.000		−0.002		−0.001
		(0.001)		(0.001)		(0.002)		(0.001)
ROA_t		−0.021		−0.049***		−0.040		−0.041*
		(0.040)		(0.019)		(0.048)		(0.021)
LEV_t		0.005		−0.016**		0.016		−0.016
		(0.018)		(0.008)		(0.018)		(0.010)
$INTERCEPT$	−0.010	0.009**	−0.002	−0.005	0.011**	0.023	−0.005***	−0.008
	(0.009)	(0.004)	(0.003)	(0.008)	(0.004)	(0.030)	(0.001)	(0.007)
No. of obs.	1,199	1,199	8,674	8,674	559	559	9,314	9,314
Adjusted R^2	0.20%	0.60%	0.10%	0.40%	0.70%	1.40%	0.10%	0.30%

C.2. Market Attention

Another possible explanation is that the pilot program focused widespread attention on the pilot firms and these firms decreased earnings manipulation because of such market attention rather than any particular attention from short sellers. To examine this hypothesis, we construct three measures of market attention. Our first measure, *Search Volume Index* (*SVI*), comes from Da, Engelberg, and Gao (2011) and is based on the frequency with which a stock is searched on Google. This measure 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, institutions' demand for information will prompt analysts to collect more information, leading to more frequent earnings forecasts (e.g., Jacob, Lys, and Neale (1999)). As a third measure of market attention, we use the 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 focus on the trading volume in the period between the identification of the pilot firms and the implementation of the program. Presumably, an 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 program was implemented could reflect lower shorting costs.

If the announcement of the pilot program led to an increase in market attention, the effect should occur following the announcement of the SEC's first pilot order (i.e., July 28, 2004), when the pilot firms were first identified. We begin our test by restricting focus to 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 Table IA.VIII, Panel A of the Internet Appendix, none of the market attention DiD estimators are statistically significant, indicating that the announcement of the pilot program did not substantially raise market attention for pilot firms compared to nonpilot firms.

We next repeat the univariate DiD tests over our main sample period (2001 to 2003, 2005 to 2010). Table IA.VIII, Panel B, reports the results. As can be seen, most of the DiD estimators for changes in market attention remain statistically insignificant. There are only two statistically significant results, regarding changes in *SVI* and the number of analyst earnings forecasts between the pilot period and the post-pilot period. These results indicate that the increase in market attention, if any, was greater for nonpilot firms.

Finally, we reestimate equation (5) 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 2004 whereas our main sample period starts in 2001. The results, reported in Table IA.IX, remain similar to those in Table III.

Market attention is an elusive concept that is difficult to measure. Nonetheless, our various tests of the market attention hypothesis suggest that we cannot attribute the patterns we document in firms' earnings management to an increase in the overall attention paid to pilot firms during the pilot program.[22]

IV. The Effect of Regulation SHO's Pilot Program on Fraud Discovery

This section reports on tests of Hypothesis 2, which holds that the conditional likelihood of fraud detection is higher for the pilot firms. We first estimate the following probit model at the firm level:

$$Pre\text{-}2004\ fraud\ caught_i = \beta_0 + \beta_1 PILOT_i + \varepsilon_i, \tag{7}$$

where *Pre-2004 fraud caught* is a dummy variable that equals one if (i) a firm is identified in the Karpoff et al. (2016) database as having initiated financial misrepresentation before July 2004, and (ii) the misconduct is revealed after May 2005. The variable is set to zero for firms that have never been identified to engage in financial misconduct, or engaged in misconduct but were detected before July 2004.[23] Hypothesis 2 predicts β_1 to be positive.

Equation (7) represents our cleanest test of Hypothesis 2. If we assume that pilot and nonpilot firms are equally likely to commit fraud before the announcement of the pilot program, Hypothesis 2 implies that the unconditional likelihood of getting caught is higher for the pilot firms. Note that we do not require that fraud detection be limited only to the pilot period (i.e., between May 2005 and July 2007) because, as shown in equations (1) and (2), both the conditional and unconditional probability of detection can remain higher for pilot firms even after the pilot program ends.

Results estimating equation (7) are reported in column (1) of Table VIII. Consistent with Hypothesis 2, the coefficient on *PILOT* is positive and significant at the 1% level. In column (2) of Table VIII, we include the four controls used in our previous tests, namely, the natural logarithm of total assets (*SIZE*), market-to-book (*MB*), return on assets (*ROA*), and leverage (*LEV*). We measure the controls in fiscal year 2003, the year immediately before the announcement of the pilot program. Consistent with the random assignment of pilot firms, the results are barely affected when including these controls. The marginal effect in column (2) indicates that the unconditional probability of detection for fraud initiated before the announcement of the pilot program (i.e., July 2004) is one percentage point higher for the pilot firms than for the nonpilot firms, which

[22] We also note that the reverting pattern we observe for discretionary accruals and for the likelihood of marginally beating earnings targets 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., Chen, Noronha, and Singal (2004)).

[23] Firms excluded from this analysis are those that (1) engaged in fraud before July 2004 that was detected between July 2004 and May 2005, or (2) engaged in fraud after July 2004 that was detected after May 2005. We identify only 32 such cases. Our results are not affected by including these firms and coding them as having zeros for *Pre-2004 fraud caught*.

Table VIII

The Effect of Pilot Program on the Discovery of Financial Misrepresentation

This table reports probit regression results on differences in pilot and nonpilot firms' likelihood of being identified as having engaged in fraud in the Karpoff et al. (2016) database. The sample comes from the 2004 Russell 3000 index. 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: $\textit{Pre-2004 fraud caught}_i = \beta_0 + \beta_1 \textit{PILOT}_i + \varepsilon_i$ in column (1). We augment the model by including *SIZE*, *MB*, *ROA*, and *LEV* in column (2), with all controls measured at the end of fiscal year 2003. We repeat column (2) with *Pre-2005 fraud caught*, *Pre-2006 fraud caught*, and *Pre-2007 fraud caught* as the dependent variables in column (3), (4), (5), respectively. Variable definitions are provided in the Appendix. Standard errors clustered by year and firm are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	Pre-2004 Fraud Caught		*Pre-2005 Fraud Caught*	*Pre-2006 Fraud Caught*	*Pre-2007 Fraud Caught*
	(1)	(2)	(3)	(4)	(5)
PILOT	0.202***	0.202***	0.193***	0.185***	0.142*
	(0.066)	(0.061)	(0.062)	(0.070)	(0.076)
SIZE		0.086***	0.082***	0.076***	0.076***
		(0.027)	(0.026)	(0.024)	(0.016)
MB		−0.007	0.008	0.006	0.006
		(0.017)	(0.014)	(0.014)	(0.013)
ROA		−2.101	−2.856	−2.368	−2.214
		(1.706)	(1.737)	(1.889)	(1.917)
LEV		−0.004**	−0.004**	−0.003*	−0.002
		(0.002)	(0.002)	(0.002)	(0.002)
INTERCEPT	−2.078***	−2.461***	−2.442***	−2.428***	−2.449***
	(0.027)	(0.199)	(0.193)	(0.192)	(0.164)
Industry fixed effects	Included	Included	Included	Included	Included
No. of obs.	2,764	2,694	2,697	2,700	2,704
Pseudo R^2	4.94%	5.90%	5.92%	5.18%	4.44%

corresponds to a 38% increase in the unconditional probability of detection for such fraud cases (2.6% in our sample).

Next, we examine whether the gap between pilot and nonpilot firms' unconditional probability of detection shrinks as we move further into the pilot program. Specifically, we reestimate equation (7), with the four controls included replacing *Pre-2004 fraud caught* with *Pre-2005 fraud caught* in column (3), *Pre-2006 fraud caught* in column (4), and *Pre-2007 fraud caught* in column (5). These three variables are defined similarly to *Pre-2004 fraud caught* except that they equal one if the firm is identified in the Karpoff et al. (2016) database for having initiated fraud before July 2005, July 2006, and July 2007, respectively. We stop at July 2007 because the database includes only one case of fraud that was initiated after that.

The rationale behind this analysis is that, as we gradually expand the window to include more cases of fraud initiated after the pilot program was announced in July 2004, Hypothesis 1 implies that pilot firm managers will have begun to adjust to the pilot program by decreasing earnings management. This decrease in earnings management will at least partially offset the higher conditional probability of detection for these firms, implying a smaller gap between the two groups of firms' unconditional probabilities of detection. We therefore expect the coefficients on *PILOT* to decrease in magnitude and/or statistical significance as we include more fraud cases initiated during the pilot program. Note that we do not have a prediction for the rate at which β_1 decreases, because that would require that we know the exact distribution of (i) the lag between the time of fraud initiation and detection (n in equations (1) and (2)), (ii) the weights the manager puts on each period's short selling potential when making the reporting decision, and (iii) the weights each period's short selling potential contributes to the probability of detection (m_s in equation (1)).

The results in Table VIII show a downward trend in the difference between pilot and nonpilot firms' unconditional probabilities of detection. In fact, the coefficients on *PILOT* become smaller and less significant monotonically from column (2) (0.202, significant at the 1% level) to column (5) (0.142, marginally significant). A one-tailed Chi-squared test shows that the coefficient on *PILOT* in column (2) is significantly larger than that on *PILOT* in column (5) at the 5% level. The marginal effects of *PILOT* drop as well, as the unconditional probability of detection is 38% higher for the pilot firms in column (2) and only 32% higher for the pilot firms in column (5).

V. The Effect of Short Selling on Price Efficiency during the Pilot Program

The results in Sections III and IV show that pilot firms are less likely to engage in earnings management during the pilot period and that the probability of detection is higher for any misrepresentation that does occur among these firms. In this section we examine whether the pilot firms' earnings became more efficiently reflected in their stock prices. Previous research shows that price efficiency in general improves with short selling (e.g., Boehmer and Wu (2013)). Here, we conduct two tests on the impact of the pilot program on price efficiency with respect to earnings. The first test examines the extent to which future earnings are incorporated in to current stock prices. The second test examines the market's reaction to negative earnings news.

A. 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:

$$R_{i,t} = \beta_0 + \beta_1 X_{i,t-1} + \beta_2 X_{i,t} + \beta_3 X3_{i,t} + \beta_4 R3_{i,t} + \varepsilon_{i,t}, \tag{8}$$

where R_t is the annual buy-and-hold return for year t, measured over the 12-month period ending three months after the end of fiscal year t. The variables X_{t-1} and X_t denote annual earnings for fiscal years $t-1$ and t, calculated as income before extraordinary items in years $t-1$ and t scaled by the market value of equity three months after the end of fiscal year $t-1$. These measures proxy for unexpected earnings news during the year. The variable $X3_t$ is aggregate earnings over the three years following fiscal year t. It is calculated as the sum of income before extraordinary items in fiscal years $t+1$, $t+2$, and $t+3$, divided by the market value of equity three months after the end of year $t-1$. This measure proxies for the cumulative change in expectations of future earnings. Finally, $R3_t$ is the buy-and-hold return for the three-year period following year t, starting three months after the end of fiscal year t. This measure helps control for the unexpected shock to $X3_t$. As in Lundholm and Myers (2002), we refer to β_3, the coefficient on $X3_t$, as the coefficient of current returns on future earnings. It captures the degree to which the current price reflects future earnings, or in other words, the efficiency of the current price with respect to future earnings.

To assess the effect of the pilot program on the coefficient of current returns on future earnings, we augment equation (8) by including interactions of pilot-related variables with $X3_t$:

$$R_{i,t} = \beta_0 + \beta_1 X_{i,t-1} + \beta_2 X_{i,t} + \beta_3 X3_{i,t} + \beta_4 R3_{i,t} + \beta_5 X3_{i,t} \times PILOT_i \times DURING_t + \beta_6 X3_{i,t} \times PILOT_i + \beta_7 X3_{i,t} \times DURING_t + \varepsilon_{i,t}. \quad (9)$$

We then estimate equations (8) and (9) using the subsample of pilot and nonpilot firms that have data necessary to construct all variables for the six-year (rather than nine-year) period around the pilot program (i.e., 2001 to 2003 and 2005 to 2007). Including the three-year post-pilot period (2008 to 2010) would require annual returns and earnings beyond 2012, for which we do not have data.

The results from estimating equation (8) are reported in column (1) of Table IX. We find that the coefficients on X_{t-1} and X_t are of similar magnitude but opposite sign, suggesting that earnings are treated by the market as following a random walk. The significantly positive coefficient on aggregate future earnings, $X3_t$, demonstrates that the current return does incorporate information on future earnings. Although $X3_t$ is used as a proxy for the change in expectations of future earnings, it also contains unexpected shocks to future earnings (a measurement error). The future return $R3_t$ is included to remove the effect of this measurement error and exhibits a predictively negative coefficient. Overall, these results are consistent with those reported in Lundholm and Myers (2002).

The results from estimating equation (9) are presented in column (2) of Table IX. The coefficients on the first four variables (X_{t-1}, X_t, $X3_t$, and $R3_t$) 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

Table IX

The Effect of Pilot Program on the Current Returns-Future Earnings Relation

This table examines differences in the annual current returns-future earnings relation across the pilot and nonpilot firms for the six-year period around Regulation SHO's pilot program (i.e., 2001 to 2003 and 2005 to 2007). The sample comes from the 2004 Russell 3000 index and contains firms that have earnings and returns information available. 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 using annual data: $R_{i,t} = \beta_0 + \beta_1 X_{i,t-1} + \beta_2 X_{i,t} + \beta_3 X3_{i,t} + \beta_4 R3_{i,t} + \varepsilon_{i,t}$ in column (1). We augment the model by including the interactions of *PILOT*×*DURING, PILOT,* and *DURING* with $X3_t$ in column (2) and by further including the interactions of *PILOT*×*DURING, PILOT,* and *DURING* with X_{t-1}, X_t, and $R3_t$ in column (3). Variable definitions are provided in the Appendix. Standard errors clustered by year and firm are displayed in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels using two-tailed tests.

	R_t		
	(1)	(2)	(3)
X_{t-1}	−0.723***	−0.676***	−0.522***
	(0.279)	(0.054)	(0.075)
X_t	0.534***	0.576***	0.618***
	(0.092)	(0.044)	(0.064)
$X3_t$	0.321***	0.270***	0.228***
	(0.050)	(0.027)	(0.029)
$R3_t$	−0.080**	−0.125***	−0.119***
	(0.034)	(0.007)	(0.009)
$X3_t$×*PILOT*×*DURING*		0.158***	0.210**
		(0.060)	(0.084)
$X3_t$×*PILOT*		−0.014	0.020
		(0.036)	(0.052)
$X3_t$×*DURING*		0.037	0.134***
		(0.030)	(0.039)
X_{t-1}×*PILOT*×*DURING*			−0.387
			(0.385)
X_{t-1}×*PILOT*			−0.621***
			(0.223)
X_{t-1}×*DURING*			−0.235*
			(0.141)
X_t×*PILOT*×*DURING*			0.145
			(0.394)
X_t×*PILOT*			0.092
			(0.193)
X_t×*DURING*			−0.324**
			(0.134)
$R3_t$×*PILOT*×*DURING*			−0.037
			(0.026)
$R3_t$×*PILOT*			0.011
			(0.015)
$R3_t$×*DURING*			−0.041**
			(0.018)
INTERCEPT	0.209*	0.347***	0.347***
	(0.111)	(0.010)	(0.010)
No. of obs.	13,844	13,844	13,844
Adjusted R^2	7.20%	10.90%	11.37%

during the three-year period of the pilot program, as evidenced by a positive coefficient on $X3_t \times PILOT \times DURING$. That is, pilot firms' 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 nonpilot firms during the pilot program ($0.270 + 0.037 = 0.307$). The difference between pilot and nonpilot firms is absent before the pilot program, as the coefficient on $X3_t \times PILOT$ is statistically insignificant. In column (3), we estimate the full model by also including the interaction terms between the pilot-related variables and X_{t-1}, X_t, and $R3_t$ and find that the results remain similar.

B. Post-Earnings Announcement Drift (PEAD)

The PEAD test builds on the notion that, when investors fail to fully capitalize on the information contained 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., Miller (1977)). We therefore expect a decrease in the cost of short selling to accelerate price discovery after negative earnings news. This implies that pilot firms' PEADs following negative earnings surprises should be smaller in magnitude than those of nonpilot firms during the pilot program.

To test this hypothesis, we follow Boehmer and Wu's (2013) methodology and examine firms' returns following earnings surprises during the pilot program. To do so, we calculate a firm's earnings surprise as its reported EPS minus the latest analyst consensus EPS forecast before the earnings announcement date (both from I/B/E/S), scaled by the stock price two days before the earnings announcement date. Next, within each quarter, we sort the sample firms into 10 deciles based on their earnings surprises, with decile one (*D1*) consisting of stocks with the most negative earnings surprises and decile 10 (*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) trading-day window relative to the earnings announcement date.

Table X reports the average *PEAD* in each decile for pilot and nonpilot firms. For the nonpilot 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, *PEAD* is small in magnitude and statistically insignificant in the lowest earnings surprise decile, *D1*. The difference in *PEAD* between the pilot and nonpilot firms is significant at the 5% level for this decile. Furthermore, *D1* is the only decile for which pilot and nonpilot firms' *PEAD*s are significantly different from each other. This result supports the hypothesis that

Table X
The Effect of Pilot Program on Post-Earnings Announcement Drift (PEAD)

This table reports differences in PEAD across the pilot and nonpilot firms for the three-year period of Regulation SHO's pilot program (i.e., 2005 to 2007). The sample comes from the 2004 Russell 3000 index and contains firms that have earnings, analyst forecasts, and return information available. 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. In each quarter, the sample firms are sorted into 10 deciles (*D1-D10*) based on their earnings surprises. The earnings surprise is calculated as the firm's actual 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 *PEAD* (+2, +11)		
	Treatment Group (*PILOT = 1*)	Control Group (*PILOT = 0*)	*t*-statistic Treatment - Control
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

stock prices of the pilot firms more efficiently incorporated negative information about earnings during the pilot period relative to the stock prices of the nonpilot firms.

Tables IX and X document a higher coefficient of current returns on future earnings and the absence of significant *PEAD* following extreme negative earnings surprises for pilot firms during the pilot program. We conclude that a decrease in the cost of short selling facilitates short selling based on earnings-related private information, the increased prospect of short selling disciplines opportunistic reporting behavior and improves earnings quality (and thus the informativeness of pilot firms' earnings), or both.

VI. Conclusion

In this paper we exploit a randomized 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 in the Russell 3000 index ranked by trading volume within each exchange was 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 and increasing the prospect of short selling among these stocks. The costs of short selling in nonpilot stocks remained unchanged until July 6, 2007. We find that pilot and nonpilot 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 preprogram levels after the pilot program ends. These patterns are not explained by changes in these firms' investment around the program, firms' incentives to issue equity, or a general increase in the attention investors paid to the pilot firms. The effect of the pilot program on firms' tendency to manage earnings is also robust to the use of two alternative measures of earnings management, namely, the likelihood of meeting or marginally beating earnings targets and the likelihood of being classified as a misstating firm based on the *F*-score of Dechow et al. (2011).

A unique advantage of the pilot program is that it enables us to use pilot and nonpilot firms' unconditional probabilities of fraud detection to infer differences in their conditional probabilities of detection. We find that, for financial misconduct that occurred before the announcement of the pilot program, pilot firms are more likely to be caught after the pilot program starts than nonpilot firms. Furthermore, as we sequentially include cases of fraud initiated after the pilot program begins, the unconditional likelihood that pilot firms are caught for financial misconduct converges monotonically toward that for nonpilot firms. Previous research shows that short selling both anticipates and accelerates the public discovery of financial misconduct (e.g., Desai, Krishnamurthy, and Venkataraman (2006), Karpoff and Lou (2010)). Our result is, however, the first to reveal that an increase in the prospect of short selling increases the detection of financial misconduct. Overall, our results indicate that the pilot program lowered the cost of short selling sufficiently to increase potential short sellers' incentives to scrutinize pilot firms' earnings reports and uncover misconduct, and that managers responded to the prospect of increased scrutiny by decreasing earnings management.

Finally, we document that, during the pilot program, pilot firms' coefficients of current returns on future earnings increase, and the magnitude of 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 with respect to earnings information.

Although short selling remains a controversial activity, our results uncover important external benefits from short selling activity. Our results uncover important external benefits from short-selling activity. In particular, a decrease in the cost of short selling curbs managers' willingness to manipulate earnings, increases the likelihood of fraud detection, and increases the informativeness of stock prices with respect to earnings. We thus demonstrate one channel through which trading in secondary markets has an impact on firms' business decisions.

Initial submission: September 12, 2013; Accepted: January 23, 2015
Editors: Bruno Biais, Michael R. Roberts, and Kenneth J. Singleton

Appendix: Variable Definitions

Variable Name	Definition
Primary measures of earnings management	
Discretionary accruals$_t$	Performance-matched discretionary accruals in fiscal year t, calculated as a firm's discretionary accruals minus the corresponding discretionary accruals of a matched firm from the same fiscal year and Fama-French 48 industry with the closest return on assets. 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 follows Dechow, Sloan, and Sweeney (1995) and is specified as $\frac{TA_{i,t}}{ASSET_{i,t-1}} = \beta_0 + \beta_1 \frac{1}{ASSET_{i,t-1}} + \beta_2 \frac{\Delta REV_{i,t}}{ASSET_{i,t-1}} + \beta_3 \frac{PPE_{i,t}}{ASSET_{i,t-1}} + \varepsilon_{i,t}$. Total accruals $TA_{i,t}$ are defined as earnings before extraordinary items and discontinued operations (*IBC*) minus operating cash flows (*OANCF*−*XIDOC*), $ASSET_{i,t-1}$ is total assets at the beginning of year t (*AT*), $\Delta REV_{i,t}$ is the change in sales revenue (*SALE*) from the preceding year, and $PPE_{i,t}$ is gross property, plant, and equipment (*PPEGT*). The fitted normal accruals are computed as $NA_{i,t} = \widehat{\beta_0} + \widehat{\beta_1} \frac{1}{ASSET_{i,t-1}} + \widehat{\beta_2} \frac{(\Delta REV_{i,t} - \Delta AR_{i,t})}{ASSET_{i,t-1}} + \widehat{\beta_3} \frac{PPE_{i,t}}{ASSET_{i,t-1}}$, with the change in accounts receivable (*RECT*) subtracted from the change in sales revenue. Firm-year-specific discretionary accruals are calculated as $DA_{i,t} = (TA_{i,t} \,/\, ASSET_{i,t-1}) - NA_{i,t}$.
Discretionary accruals_MBadj$_t$	Similar to *Discretionary accruals*$_t$, except that market-to-book (*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 a pilot stock in Regulation SHO's pilot program and zero otherwise.
PRE$_t$	A dummy variable that equals one if the end of a firm's fiscal year t falls between January 1, 2001 and December 31, 2003 and zero otherwise.
DURING$_t$	A dummy variable that equals one if the end of a firm's fiscal year t falls between January 1, 2005 and December 31, 2007 and zero otherwise.
POST$_t$	A dummy variable that equals one if the end of a firm's fiscal year t falls between January 1, 2008 and December 31, 2010 and zero otherwise.
Firm characteristics	
ASSET$_t$	Book value of total assets (*AT*) at the end of fiscal year t; *SIZE* is the natural logarithm of *ASSET*.
MB$_t$	Market-to-book ratio in fiscal year t, calculated as the market value of equity (*PRCC_F*×*CSHO*) divided by the book value of equity (*CEQ*) at the end of the year.
ASSETGR$_t$	Total assets at the end of fiscal year t divided by that at the beginning of the year minus one.
CAPEX$_t$	Capital expenditures (*CAPX*) during fiscal year t scaled by total assets at the beginning of the year. $CAPEX_t^2$ is the squared term of $CAPEX_t$.

Variable Name	Definition
$R\&D_t$	Research and development expenditures (*XRD*) during fiscal year t scaled by total assets at the beginning of the year, set to zero if missing. ${R\&D_t}^2$ is the squared term of $R\&D_t$.
$INVESTMENT_t$	The sum of $R\&D_t$ and $CAPEX_t$. ${INVESTMENT_t}^2$ is the squared term of $INVESTMENT_t$.
ROA_t	Return on assets in fiscal year t, calculated as income before depreciation and amortization (*OIBDP*) during year t scaled by total assets at the beginning of the year.
CFO_t	Operating cash flow (*ONACF*) in fiscal year t scaled by total assets at the beginning of the year.
LEV_t	Leverage in fiscal year t, calculated as 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 the year.
$CASH_t$	Cash and short-term investment (*CHE*) at the end of fiscal year t scaled by total assets at the beginning of the year.
$DIVIDENDS_t$	Common share dividends (*DVC*) plus preferred share dividends (*DVP*) during fiscal year t scaled by total assets at the beginning of the year.
Meeting or beating earnings target measures and related controls	
$BEAT\_ALY_q$	A dummy variable that equals one if reported EPS falls between the analyst consensus forecast and that plus one cent in fiscal quarter q and zero otherwise.
$BEAT\_EPS_q$	A dummy variable that equals one if reported EPS falls between prior-year same-quarter EPS and that plus one cent in fiscal quarter q and zero otherwise.
$DURING_q$	A dummy variable that equals one if the earnings announcement of a firm's fiscal quarter q falls between May 2, 2005 and August 6, 2007 and zero otherwise.
$POST_q$	A dummy variable that equals one if the earnings announcement of a firm's fiscal quarter q is after August 6, 2007 and zero otherwise.
MV_q	Natural logarithm of the market value of equity at the beginning of fiscal quarter q.
MB_q	Market-to-book ratio in fiscal quarter q.
ROA_q	Return on assets in fiscal quarter q, calculated as income before depreciation and amortization (*OIBDPQ*) during quarter q scaled by total assets at the beginning of the quarter.
ALY_N_q	Natural logarithm of one plus the number of analysts following a firm during fiscal quarter q.
$ALY\_HORIZON_q$	Natural logarithm of one plus the mean forecast horizon, where the forecast horizon is the number of days between an analyst forecast date and the earnings announcement date for fiscal quarter q.
ALY_DISP_q	Analyst forecast dispersion, calculated as the standard deviation of analyst forecasts divided by the absolute value of the consensus analyst forecast, all measured for fiscal quarter q.
$\triangle R\&D_q$	Research and development expenditures (*XRDQ*) during fiscal quarter q minus those during fiscal quarter $q-4$ scaled by total assets at the beginning of the quarter, set to zero if missing.
$\triangle CAPEX_q$	Capital expenditures (inferred from *CAPXY*) during fiscal quarter q minus those during fiscal quarter $q-4$ scaled by total assets at the beginning of the quarter, set to zero if missing.

Variable Name	Definition
F-scores	
HF1 (HF2, HF3)	*HF1* is a dummy variable that equals one if the firm's *F1* is greater than or equal to the 99th percentile of the sample, and zero otherwise. *F1* is calculated using the set of coefficient estimates provided in Dechow et al. (2011) based on their Model (1), which includes balance sheet items to capture accruals quality and financial performance. *HF2* and *HF3* are defined similarly, with *F2* calculated using coefficient estimates from Dechow et al.'s (2011) Model (2), which also includes nonfinancial measures, and *F3* calculated using coefficient estimates from their Model (3), which further includes market-based measures.
Variables used in the fraud discovery analysis	
Pre-n fraud caught (n = 2004, 2005, 2006, 2007)	*Pre-2004 fraud caught* is a dummy variable that equals one if (i) a firm is identified in the Karpoff et al. (2016) database as having initiated a reporting fraud before July 2004 and (ii) the fraud is revealed after May 2005, and zero if a firm has never been identified to engage in fraud or engaged in fraud but were detected before July 2004. *Pre-2005 fraud caught*, *Pre-2006 fraud caught*, and *Pre-2007 fraud caught* are defined similarly to *Pre-2004 fraud caught* except that they equal one if a firm is identified as having initiated a reporting fraud before July 2005, July 2006, and July 2007, respectively.
Variables used in the price efficiency analysis	
X_t	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 the market value (*PRC*×*SHROUT*) three months after the end of year $t-1$. $X3_t$ is the aggregate earnings for the three years following fiscal year t, calculated as the sum of income before extraordinary items in fiscal years $t+1$, $t+2$, and $t+3$ scaled by market value three months after the end of year $t-1$.
R_t	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.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.