5-2014

Short sellers and corporate disclosures

Qiang CHENG
Singapore Management University, qcheng@smu.edu.sg

Xia CHEN
Singapore Management University, xchen@smu.edu.sg

Ting LUO
Tsinghua University

Heng YUE
Singapore Management University, hyue@smu.edu.sg

Short sellers and corporate disclosures*

Xia Chen
Singapore Management University
Qiang Cheng
Singapore Management University
Ting Luo
Tsinghua University
Heng Yue
Peking University

May 2014

Abstract

We examine how short sellers affect corporate disclosures using a natural experiment. From May 2005 to July 2007, the SEC implemented a pilot program by randomly selecting one third of Russell 3000 stocks and removing the short sale price tests for these stocks (referred to as pilot firms), leading to lower short-selling constraint, without changing the requirement for other firms (referred to as control firms). We compare the change in corporate disclosures between the pilot firms and the control firms during this period. We find that compared to the control firms, the pilot firms are more likely to issue good news management forecasts without changing the issuance of bad news forecasts. We also find that the decrease in short-selling constraint for the pilot firms (1) leads to an increased likelihood of bundling bad news forecasts with good news earnings announcements, and (2) does not lead to an increase in the optimistic bias in management forecasts. Overall, our evidence suggests that the reduction in short-selling constraint motivates managers to disclose good news in a more timely fashion.

Keywords: Short Sales; Corporate Disclosure; Management Forecasts; Earnings Recognition

---

* We thank Ashiq Ali, Sung Gon Chung, Xi Li, Holly Yang, Haifeng You, and workshop participants at Cheung Kong Graduate School of Business, Hong Kong University of Science and Technology, Singapore Management University, University of Hong Kong, and University of Texas-Dallas for helpful comments. We thank the financial support from the School of Accountancy Research Center (SOAR) at Singapore Management University. Please contact authors at xchen@smu.edu.sg (Xia Chen), qcheng@smu.edu.sg (Qiang Cheng), luot@sem.tsinghua.edu.cn (Ting Luo), and yueheng@gsm.pku.edu.cn (Heng Yue) for comments.

1. Introduction

In this study, we examine how short sellers affect corporate disclosures. The motivation for the research question is two-fold. First, short sellers are becoming an increasingly important group of traders in the capital markets. For example, short sales account for more than 20% of the trading volume in the period 2000-2004 (Boehmer et al. 2013). As shown in Figure 1, short interest, measured as the average monthly short interest scaled by the number of outstanding shares, almost doubled from the 1990s to the 2000s.[1] Short sellers play an important role in the information discovery process, particularly in incorporating bad news into stock prices (e.g., Boehmer and Wu 2013). However, despite the importance and prevalence of short-selling, we know little about whether and how short sellers affect corporate disclosures. The limited research on the impact of short sellers is in sharp contrast with the large number of studies on the impact of other market participants, such as institutional investors, family owners, and financial analysts, on corporate disclosures.

Second, unlike institutional investors or financial analysts, whose presence is generally welcome by managers, short sellers are typically not viewed favorably. Disputes between managers and short sellers often capture the headlines of the business press. Short sellers are not welcome not only because of the downward pressure of short-selling on stock prices, but also because of the potentially adverse impact on stakeholders' confidence in the firm and the ensuing long-run damaging effect on firms' financing and operation (e.g., Khanna and Mathews 2012).

[1] Note that short interest is inherently a small proportion of the outstanding shares. Beneish et al (2013) find that for stocks that are more difficult to borrow, less than 10 percent of the outstanding shares are relatively easy to locate and thus lendable; even for stocks that are easier to borrow, less than 20 percent of the outstanding shares are lendable.

Therefore, managers have incentives to discourage short sellers. Prior research finds that firms undertake some dramatic measures, such as legal actions, against short sellers (e.g., Lamont 2012). It is unclear whether firms change disclosure policy, one of the most direct ways through which manager can influence the market perceptions, to discourage short interest.

One challenge of examining the impact of short sellers on corporate disclosures is the potential endogeneity issue. The association between the *observed* level of short interest and corporate disclosures is subject to endogeneity because the causality can go either way. Managers might change disclosure policy in response to short interest and at the same time, short interest is affected by disclosures. To address this potential endogeneity, we utilize a natural experiment – the SEC's temporary suspension of the tick test for a randomly selected group of firms in 2005-2007 – to test the impact of short sellers on corporate disclosures.

Traditionally, short-selling was subject to SEC Rule 10a-1, NYSE's uptick rule, and Nasdaq's bid price test. These rules and tests, referred to as the tick test for convenience, imposed constraints on short-selling.[2] On June 23, 2004, the SEC announced a pilot program by adopting Regulation SHO to temporarily suspend the tick test for a group of randomly selected firms (i.e., the pilot firms) and subsequently announced the list of the pilot firms on July 28, 2004.[3] Starting from May 2, 2005, the pilot firms were exempt from the tick test for short sale orders. The temporary suspension expired on July 6, 2007 when the SEC decided to permanently suspend the tick test for all the publicly-traded U.S. companies. As such, during the pilot program, the

[2] Please see Section 2.1 for a detailed discussion of the tick test.
[3] Specifically, the SEC separated the U.S. firms in the 2004 Russell 3000 index into three groups based on the exchange on which the stocks were traded (NYSE, AMEX, or Nasdaq) and ranked them based on average trading volume within each group. The SEC then selected every third stock from each group.

short-selling constraint became lower for the pilot stocks and it was easier and less costly to take short positions in the pilot stocks than in the other stocks (referred to as the control stocks hereafter). Prior studies provide consistent evidence that short-selling increases significantly for the pilot stocks compared to the control stocks during the pilot program (e.g., Boehmer et al. 2008; Diether et al. 2009a, Grullon et al. 2013) . The combination of an exogenous shock to the short-selling constraint and the randomization of the treatment group provide us with an ideal setting to examine how short-selling, as affected by short-selling constraints, affects corporate disclosures.

We argue that managers of the pilot firms have incentives to change disclosures in response to the reduction in short-selling constraint and the increase in short-selling. The incentives are different for good news disclosures and bad news disclosures. We consider the disclosure of good news first. As discussed in detail in Section 2, managers' welfare is usually positively related to stock prices and managers prefer short sellers not to take position in their firm's stock. Prior research (e.g., Lamont and Stein 2004; Savor and Gamboa-Cavazos 2011; Hong et al. 2012) finds that the disclosure of good news leads to lower short interest. (We confirm that in our sample, the issuance of good news forecasts leads to lower short interest.) In addition, if short sellers expect firms to disclose good news in a more timely fashion, they are less willing to take a short position in the firm's stock for the fear of losing out when they have to close their position. Managers may also have incentives to disclose good news in order to boost the confidence of the stakeholders in the firm. It thus follows that the pilot firms will increase the disclosure of good news compared to the control firms.

With respect to bad news disclosures, there is prior evidence that short sellers' trading gains lead them to increase their position (Savor and Gamboa-Cavazos 2011). (We confirm that in our sample, the issuance of bad news forecasts leads to higher short interest.) This provides managers with incentives to withhold bad news. However, withholding bad news can backfire because short sellers are generally regarded as "extremely well informed" (Boehmer et al. 2013) and overpricing of stocks that result from withholding bad news can actually increase the short-selling profit and hence attract short interest. In addition, withholding bad news is subject to litigation risk (Skinner 1994, 1997), which potentially becomes greater for the pilot firms under the pilot program due to the increase in short-selling and the faster incorporation of bad news into the share prices. Thus, managers of the pilot firms have conflicting incentives related to bad news disclosures and it is unclear whether they will increase or decrease bad news disclosures relative to the control firms.

To summarize, while we expect that the pilot firms are more likely to disclose good news during the pilot program compared to the control firms, the prediction regarding bad news disclosures is non-directional. To test the predictions, we adopt a difference-in-differences approach. We first measure the change in corporate disclosures between the period prior to the pilot program (i.e., the pre period) and the period when the pilot program was in place (i.e., the post period); we then compare the change between the pilot stocks and the control stocks.

Our main findings are as follows. First, compared to the control firms, the pilot firms are significantly more likely to increase good news management forecasts from the pre period to the post period. At the same time, compared to the control firms, the pilot firms are not associated with a significant change in the frequency or likelihood of bad news management forecasts. Second, we find that the results for good news forecasts are stronger when managers are more concerned with stock price drops, such as when managers' wealth is more sensitive to stock price changes or when the firm's stock is more difficult to value because of large magnitude of accruals or volatile earnings. These cross-sectional variations reinforce the main inference. Third, we find that relative to the control firms, the pilot firms are more likely to bundle bad news forecasts with good news earnings announcements in the post period compared to the pre period. Thus the pilot firms appear to be more likely to time bad news forecasts to reduce the adverse price impact. Overall, our results indicate that in response to the reduction in short-selling constraint, the pilot firms increase the frequency of good news forecasts without decreasing the frequency of bad news forecasts, except that they are more likely to bundle bad news forecasts with good news earnings announcements. The lack of results on the frequency of bad news disclosures is possibly due to the conflicting incentives related to bad news disclosures, as discussed above.

We conduct several additional analyses to enrich the results. First, we find that there is no significant change in management forecast bias for the pilot firms from the pre period to the post period. This finding is important because it indicates that the increase in good news forecasts for the pilot firms is not due to managers becoming more optimistically biased. Second, in July 2007 the SEC permanently removed the tick test for all the stocks. As a result, the control firms experienced a shock to the short-selling constraint while the pilot firms experienced no change. We find that the control firms are associated with a significant increase in the likelihood and frequency of good news forecasts relative to the pilot firms after the permanent removal of the tick test. This reinforces our main results. Third, we investigate whether issuing good news forecasts can indeed reduce short interest. The analysis of the change in short interest surrounding management forecasts supports this.

Our paper contributes to the literature in the following ways. First, it contributes to the disclosure literature by providing evidence on how short sellers, an important group of players in the stock market, influence companies' disclosures. Thus, our study complements the existing literature that examines how various market participants, such as institutional investors, family owners, and financial analysts, influence corporate disclosures. Overall this line of research suggests that companies change their disclosures to reflect the market participants' preferences. The impact of short sellers on corporate disclosures differs from the other market participants in two ways. First, while the impact of the other market participants on disclosures is through their demand for information, the impact of short sellers is indirect and arises from managers' desire to discourage short sellers and maintain the stock price. Second, unlike the other market participants, who generally have symmetric effect on disclosures of good news and bad news, short sellers mainly affect good news disclosures.

Second, our study enhances the understanding of short sellers' role in the capital markets. The evidence suggests that short sellers not only help incorporate bad news into the share prices, as documented in prior research (e.g., Boehmer and Wu 2013), but also help bring good news forward. While the former is through short sellers' information acquisition and trading activities, the latter is through managers' disclosures in response to short selling or the threat of it. The evidence should be of interest to regulators as it sheds light on how the change in short-selling constraints affects firm disclosures, which regulators have not considered when debating whether and how to regulate short-selling. Together with the studies that examine the impact of Regulation SHO on short-selling, stock market quality, investment and financing activities, and the extent of earnings management (Diether et al. 2009a; Grullon et al. 2013; Fang et al. 2013), our study helps provide a more comprehensive picture of the effect of this important regulation.

Third, our findings add to the evidence on the actions taken by firms to discourage short-selling. Lamont (2012) uses a small sample, 266 firms over the period of 1977-2002, and focuses on legal actions against short sellers. Liu and Swanson (2011) examine whether firms use stock repurchases to reduce short interest and Laksanbunsong and Wu (2014) examine whether insiders counter the effect of short selling via insider purchases. Our findings complement these studies by examining corporate disclosures.

The rest of the paper is organized as follows. Section 2 discusses the institutional background, reviews prior literature, and develops the hypotheses. Section 3 discusses the sample. Section 4 presents the main empirical results and Section 5 presents the additional analyses. Section 6 concludes.

2. Institutional background, prior research, and hypothesis development

2.1 Institutional background on the pilot program

In 1938, the SEC adopted Rule 10a-1, often referred to as the uptick rule, to restrict short-selling activities. According to the SEC, "Rule 10a-1(a) (1) provided that, subject to certain exceptions, a listed security may be sold short (A) at a price above the price at which the immediately preceding sale was effected (plus tick), or (B) at the last sale price if it is higher than the last different price (zero-plus tick). Short sales were not permitted on minus ticks or zero-minus ticks, subject to narrow exceptions."[4] In 1994, the Nasdaq adopted a bid price test to determine whether short sales are allowed for shares traded on Nasdaq (Nasdaq Rule 3350). Short sales on Nasdaq are not allowed at or below the best bid when the current best bid is at or below the previous best bid. These rules and tests, referred to as the tick test for convenience, impose constraints on short-selling. Prior studies find that the stocks are more difficult to short after the introduction of short-selling restrictions (e.g., Jones and Lamont 2002).

On June 23, 2004, the SEC adopted Regulation SHO (REG SHO) to provide a new regulatory framework for short-selling in the U. S. stock markets. Among other things, REG SHO temporarily suspended the tick test for a group of randomly selected listed companies in order to evaluate the effectiveness and necessity of short-selling restrictions. On July 28, 2004, about 1,000 U.S. stocks listed on NYSE, AMEX, and Nasdaq were selected as the pilot stocks. Starting from May 2, 2005, the pilot stocks were exempt from the tick test. The temporary suspension expired on July 6, 2007 when the SEC permanently suspended the tick test for all the publicly-traded U.S. companies. The permanent suspension of the tick test drew criticisms from firms and former regulators, including former SEC chairman Christopher Cox. The criticism intensified with the financial crisis in 2008-2009 due to the concern that financial stocks may be subject to market manipulations via short-selling. On February 24, 2010, the SEC reinstated the uptick rule, but only under the circumstance when a security's price drops by 10% or more from the last day's closing price.

---

[4] "Amendments to Exchange Act Rule 10a-1 and Rules 201 and 200(g) of Regulation SHO." SEC 2008-05-21.

Therefore, compared to the control stocks, the pilot stocks experience a decrease in short-selling constraints during the pilot program. Prior studies provide consistent evidence that short selling increases significantly for the pilot stocks relative to the control stocks during the pilot program (e.g., Boehmer et al. 2008, Diether et al. (2009a), Grullon et al. 2013). Depending on the measures used, the tick test is found to inhibit around one tenth to more than one fifth of short-selling. These studies also find that the pilot program somewhat worsens market quality, but there is mixed evidence on the impact of the short-selling constraints on price level and volatility. Recent studies find that the pilot program affects corporate decisions. Grullon et al. (2013) find that the financially constrained pilot firms are associated with reduction in equity issuance and investments during the pilot program. Fang et al. (2013) find that the pilot firms are less likely to engage in earnings management during the pilot program than the control firms. In sum, prior studies indicate that Regulation SHO has an important impact on short selling. Hence this is a powerful setting to examine how short sellers affect firms' disclosures.

2.2 Prior research on short selling

There is a long line of literature on short selling and stock returns. This literature generally find that short sellers are on average informed traders; short sellers as a whole unearth over-valued companies and abnormal short interest is associated with future negative stock returns (e.g., Dechow et al. 2001; Jones and Lamont 2002; Ofek and Richardson 2003; Pownall and Simko 2005; Desai et al. 2006; Boehmer et al. 2008; Hirshleifer et al. 2011; Boehmer and Wu 2013; Kecskes et al. 2013). Prior findings suggest that short sellers' information advantage comes from private information acquisition, fundamental analysis based on public information, as well as skilled processing of public information (e.g., Desai et al. 2006; Drake et al. 2011; Engelberg et al. 2012; Christensen et al. 2013; Desai et al. 2013).[5] Overall, prior studies suggest that short sellers are important contributors to efficient prices.

At the same time, short sellers are viewed with considerable skepticism for the following reasons. First, short sellers, through taking and covering short positions, can increase market volatility, potentially leading to higher perceived risk. Hong et al. (2012) and Savor and Gamboa-Cavazos (2011) find that the prices of heavily shorted stocks are excessively sensitive to new information relative to other stocks. They find that short sellers cover their positions after the announcement of good earnings news, pushing stock prices further up and leading to high volatility. Savor and Gamboa-Cavazos (2011) also find that short sellers increase their positions after trading gains from decreases in stock prices, pushing stock prices further down, again leading to high volatility.[6] Second, manipulative short sellers can target a stock, encourage others to sell, perhaps by spreading rumors about the prospect of the firm, and then cover their short positions at a profit. This leads to disorderly market.[7]

Third, short selling is also believed to be harmful to the firm because of the feedback effect on the firm's operations. Short interest can make existing (or potential) stakeholders lose confidence in the firm and stop dealing with it. It is difficult to tell whether a stock is overpriced or not; neither managers nor short sellers can confidently claim that a firm's share is overpriced or not. The stakeholders may take short position as a signal of overpricing and react accordingly,

[5] Some studies suggest that short sellers also benefit from front-running or tipping (Khan and Lu 2013,Christophe et al. 2010).
[6] Lamont and Stein (2004) examine the aggregate short interest during the dot-com bubble period and find that short-selling is not helpful in stabilizing the overall stock market.
[7] Many companies complain that short sellers can be manipulative and detrimental to shareholders. Examples include Overstock.com, Sedona Corp., Medizone International Inc.

making it more difficult for the firm to attract investors, capital and customers, leading to deterioration of performance. For example, Goldstein and Guembel (2008) show analytically that traders have incentives to short a firm's stock because of the feedback effect from the capital market to the real value of the firm. Khanna and Mathews (2012) argue that the damage of short selling "is caused not so much by the initial drop in stock price, but through its feedback effect on the real decisions of the firm's counterparties, since that not only amplifies the firm's price drop but also makes it more permanent." Grullon et al. (2013) find that during the pilot program, financially constrained pilot firms experience reduction in equity issues and investments.

2.3 Hypothesis development

We argue that managers have incentives to change disclosures in response to increase in short selling because short-selling affects managers' welfare and firms' prospects and disclosures, on the other hand, can be used to influence share prices, short interest, and investor perceptions. We elaborate below.

Managers' welfare, such as compensation, job security and reputation, is positively linked to the level of the stock price. Hence decreases in stock prices can adversely affect managers' welfare and managers are generally reluctant to correct stock over-pricing. Jensen (2005) observes that despite the importance of correcting over-valuation of equity, few managers are willing to do so, contributing to the so-called agency costs of overvalued equity.[8] In addition, as discussed above, short-selling can increase stock volatility, be speculative rather than based on fundamentals, and adversely affect the stakeholders' confidence and the long-run performance of

[8] Managers' reluctance to correct over-pricing is also reflected in their aversion to financial analysts' sell recommendations, which is well discussed in the literature.

the firm. Thus, managers typically view short-selling negatively. For example, short sellers are believed by some managers to be "evil and damaging to the firm (Jensen 2005, p.16)."

Thus, with the implementation of the pilot program, managers of the pilot firms will have incentives to discourage short sellers and to reduce the impact of short interest. We predict that they will resort to corporate disclosures, since disclosing is one of the most direct tools managers have at their disposal to influence share prices and market perceptions. We next discuss how managers of the pilot firms will change the disclosures. We follow the disclosure literature by assuming that managers' disclosure decision is based on the costs and benefits of disclosures and disclosures of the pilot firms may change during the pilot program because of changes in such costs and benefits. Below we separately discuss good news and bad news disclosures because they can affect short interest differently.

Good news disclosures. Prior studies argue that good news disclosures can decrease short interest. Shleifer and Vishny (1997) argue that the hedge funds that specialize in short-selling are usually open-ended. When the stock price increases, short sellers will lose money and likely face redemption by the clients. As a result, they will be forced to close their short positions. Savor and Gamboa-Cavazos (2011) argue that because short sellers' compensation is linked to investment performance, they are subject to myopic loss aversion. With myopic loss aversion, short sellers will become more loss-averse after suffering losses and will cover their short positions. Consistent with these arguments, Lamont and Stein (2004), Savor and Gamboa-Cavazos (2011), and Hong et al. (2012) find that the trading losses after good news disclosures drive short sellers to cover their positions.[9] We confirm that for our sample firms, the issuance of good news forecasts leads to lower short interest. In addition, if short sellers expect firms to disclose good news in a more timely fashion, they will be less willing to take a short position in the firm for the fear of losing out when they have to close their position. Another motivation for disclosing good news is to increase the confidence of the stakeholders.

Therefore, with the reduction in short-selling constraints and the increase in short-selling during the pilot program, all else equal, the benefit of good news disclosure becomes more important. Hence we predict that managers of the pilot firms will increase good news disclosure to discourage short sellers as well as to reduce the impact of short selling.[10]

Bad news disclosure. Savor and Gamboa-Cavazos (2011) find that the trading gain from shorting a stock leads short sellers to increase their short position. De Angelis et al. (2013) argue that the reduced short-selling constraints faced by the pilot stocks during the pilot program can increase the price sensitivity to bad news disclosures because of the increased incentives of bear raiders to manipulate the price of these stocks and Grullon et al. (2013) find that for financially constrained pilot stocks, their share prices become more sensitive to negative news during the pilot program. The above discussions suggest that managers of the pilot firms become less likely to disclose bad news. However, withholding bad news may backfire by increasing the likelihood of overpricing. If short sellers suspect that firms are hiding bad news, either through observing firms' deviations from past disclosures or through information acquisition and research, they can increase

[9] Using daily trading data, Diether et al. (2009b) find that short sellers are more active after positive stock returns. As discussed in Savor and Gamboa-Cavazos (2011), Diether et al.'s finding is largely driven by intra-day trading.

[10] Disclosing good news is also associated with costs, such as proprietary costs and litigation costs. For example, Cheng and Lo (2006), Cheng et al. (2013), and others argue that disclosing forward-looking good news is subject to litigation risk because such information might prove to be wrong ex post. Here we assume that the costs do not change with the pilot program.

their short position in the firm.[11] Another consideration is that withholding bad news is subject to potential litigation risk. As discussed in Skinner (1994, 1997), managers have fiduciary duties to disclose material information and failing to disclose can lead to litigation risk. The litigation risk related to bad news disclosures becomes potentially higher during the pilot program with the increase in short selling and the speedier incorporation of bad news in the share prices.

To summarize, the above discussions suggests that managers of the pilot firms are more likely to disclose good news, but they have conflicting incentives for the disclosure of bad news. Thus, our hypothesis is directional for good news disclosures and non-directional for bad news disclosures:

H1: Ceteris paribus, the pilot firms are more likely to disclose good news than the control firms during the pilot program.

H2: Ceteris paribus, the pilot firms are as likely to disclose bad news as the control firms during the pilot program.

3. Sample

3.1 Sample selection

To construct our sample, we start with the Russell 3000 index firms in 2004, the set of firms from which the SEC selected the pilot stocks. Panel A of Table 1 summarizes the sample selection process. First, following the SEC's selection criteria, we exclude stocks that were not listed on NYSE, AMEX, or Nasdaq and stocks that went public through IPOs after April 30, 2004. Second, following Diether et al. (2009a), we require that firms be included in the Russell 3000 index in 2005 as well. Firms that dropped out of the Russell index were usually involved in mergers and

[11] For example, Christensen et al. (2013) argue that pro-forma disclosure can disguise bad news and find that short-sellers are more likely to short stocks with pro-forma disclosure.

acquisitions or had other significant corporate events according to the Russell index manual.[12] We also exclude stocks that change tickers during the pilot program. These steps reduce the number of the sample firms by 21, 374, and 83, respectively.

In the empirical analyses, we focus on the issuance of management forecasts, one of the most frequently studied types of voluntary disclosures.[13] We obtain data on management forecasts from First Call. Because the pilot stocks were selected by the SEC on July 28, 2004 but the pilot program started on May 2, 2005, we eliminate the quarters between these two dates to increase the power of the tests.[14] We use the difference-in-differences approach. The pre period includes the fiscal quarters that start after January 1, 2002 and end before July 28, 2004, and the post period covers the duration of the pilot program, including the fiscal quarters that start after May 2, 2005 and end before July 6, 2007. See Figure 2 for the timeline.

We exclude firms that have missing financial, stock price, analyst data from Compustat, CRSP, or I/B/E/S in the pre or post period. To ensure a balanced sample, we require that firms have the same number of quarters in the pre and post periods.[15] The results are qualitatively similar if we do not impose this requirement and use an unbalanced sample. Our final sample includes 34,718 firm-quarters from 2,352 unique firms, of which 768 are pilot firms and the rest

---

[12] Note that the stocks selected as the pilot stocks remain so even if they are later excluded from the Russell 3000 index. We exclude them from our sample because of the confounding effect of mergers and acquisitions or other significant events (Diether et al. 2009a).

[13] Besides data availability, another reason we choose to examine management forecasts is that management forecasts are usually about near-term earnings. This reduces managers' incentives to use management forecasts to mislead the market. In an additional test, we explicitly examine whether management forecast bias changes for the pilot stocks during the pilot program.

[14] In a sensitivity test, we examine this period and do not find that there is a significant change in disclosures from the pre period for the pilot firms relative to the control firms.

[15] For firms with more quarters in the pre period than in the post period, we drop the earlier quarters in the pre period so that the number of quarters is the same between the two periods. For example, if we have data for six quarters in the post period, we also include six quarters from the pre period – the last six quarters with required data in the pre period. On the other hand, if a firm has fewer quarters in the pre period than in the post period, we exclude this firm from the sample.

1,484 are control firms.[16]

3.2 Descriptive statistics

Panel B of Table 1 presents the industry composition of the sample firms. The sample firms are from a broad spectrum of industries, with more firms in banking and business service industry than in the other industries.

Panel C of Table 1 presents the descriptive statistics for the pilot and control firms. The statistics are measured in fiscal year 2003, the year before the SEC selected the pilot firms. We report the statistics on total assets (*Size*), the market-to-book ratio (*M/B*), leverage (*Leverage*), return on equity (*ROE*), trading volume (*Trading Volume*), and analysts following (*Analyst Following*), separately for the pilot and control firms. We also report the p-values for testing the differences in means and medians between the pilot and control firms. As reported in the table, there are no significant differences between the pilot and control firms in any of these characteristics, consistent with the random selection of the pilot firms by the SEC.

4. Main analysis

4.1 Univariate tests

To explore the effect of the reduction in short-selling constraints on management forecasts, we first conduct a univariate test. We compare the change in the quarterly frequency of management forecasts from the pre to post period between the pilot and control firms, separately

[16] Of the 2,352 firms, 85.2% have eight quarters, the maximum possible number of quarters, in both periods, 9.4% have seven quarters, 1.5% have six quarters, 1.7% have five quarters, and 2.2% have four or fewer quarters in both periods.

for good news and bad news forecasts.[17] A management forecast is classified as good (bad) news if the forecast is higher (lower) than the consensus analyst forecast in the previous 90 days. For range management forecasts, we compare the mid-point of the forecast range with analyst forecast. For open-ended management forecasts, we classify the forecast as good (bad) news when its bottom (upper) bound is higher (lower) than analyst forecast. For qualitative forecasts, we follow Anilowski et al. (2007) and classify the forecast as good news if the forecast is coded as "meets or exceeds expectations" or "above expectations," and as bad news if the forecast is coded as "below expectations" or "may not meet expectations." All the other management forecasts are classified as neutral news and not included in the analyses.[18]

Panel A of Table 2 reports the frequency of good news forecasts. In the pre period, the pilot firms have a slightly lower frequency of good news forecasts than the control firms. The frequency of good news forecasts increases significantly during the pilot program for the pilot firms, from 0.261 to 0.295, an increase of 13%, significant at the 0.007 level. In contrast, the frequency of good news forecasts decreases over the same period for the control firms, from 0.280 to 0.269, a decrease of 4%. The difference in the change from the pre to post period between these two groups is significant at the 0.003 level.

Panel B reports the frequency of bad news forecasts. The pilot and control firms have a similar frequency of bad news forecasts in the pre period. During the pilot program, both groups

[17] We use the quarterly, instead of annual, frequency of management forecasts because both the starting date of the pilot program (May 2, 2005) and the ending date (July 6, 2007) are around mid-year. If we use the annual frequency and analyze fiscal years, we will lose about half of the post period. Using the quarterly frequency therefore significantly increases the length of the post period analyzed and the power of the tests. Examining the annual frequency leads to qualitatively similar results (untabulated).

[18] Rogers and Van Buskirk (2013) find that management forecasts are often bundled with earnings announcements, which can result in noises in the classification of good vs. bad news forecasts. To address this issue, we follow Rogers and Van Buskirk; for bundled forecasts, we estimate a revised (unobservable) analyst expectation after the earnings announcement and use this instead of the consensus analyst forecast to classify forecasts. The inferences remain the same (the results are untabulated to save space).

experience a significant and similar increase in the frequency of bad news forecasts. The difference in the change between the two groups is insignificant (p=0.448).

Overall, the univariate tests suggest that compared to the control firms, the pilot firms experience a significant increase in good news forecasts. The two groups of firms do not differ significantly in the change in bad news forecasts.

4.2 Multivariate tests

We next use multivariate analyses to control for the effect of other variables that have been documented to affect the issuance of management forecasts. We use the following regression:

$$MF = \alpha_0 + \alpha_1 PILOT + \alpha_2 POST + \alpha_3 PILOT \times POST + \beta Control\ \ Variables\ + \varepsilon \qquad (1)$$

Firm and quarter subscripts are omitted for simplicity. The dependent variable is *MF_N* or *MF_D*. *MF_N* captures the frequency of management forecasts and is measured as the number of management forecasts issued in a quarter. Since *MF_N* is bounded below at zero, we use the Tobit regression when *MF_N* is the dependent variable. *MF_D* captures the likelihood of management forecasts and it equals 1 if the firm issues at least one management forecast in the quarter, and 0 otherwise. We use the Logit regression when *MF_D* is the dependent variable. To test H1 and H2, we estimate Equation (1) separately for good news and bad news forecasts; the dependent variables (*MF_N* and *MF_D*) are constructed accordingly.

PILOT is an indicator variable for the pilot firms. It equals 1 if a firm's stock was designated as a pilot stock by the SEC and 0 for the other firms in the sample. *POST* is an indicator variable for the post period. It equals 1 for firm-quarters in the post period and 0 for those in the pre period. The main variable of interest is the interaction of *PILOT* and *POST*. A positive (negative) coefficient on the interaction indicates that the pilot firms experience an increase (a decrease) in the likelihood or frequency of management forecasts during the pilot program, compared to the control firms.

We include a set of control variables based on prior research. First, prior research indicates that managers are more likely to disclose when the demand for information is higher (Baginski and Hassel 1997; Ajinkya et al. 2005; Lennox and Park 2006). We use analyst coverage, firm size, and growth opportunities (proxied by the market-to-book ratio) to capture the demand for information. Second, when the operating environment is uncertain, managers are reluctant to disclose forecasts because the forecasts might turn out to be incorrect and managers could face lawsuits. We include earnings volatility and return volatility to control for the uncertainty in the operating environment. Third, we control for prior stock returns because firms with good performance are more likely to provide voluntary disclosures (Miller 2002).[19] Fourth, when analysts are optimistic, managers have incentives to issue forecasts to guide market expectations downward (Richardson et al. 2004). We therefore include an indicator variable for analyst optimism. Lastly, we include two indicator variables for firms in the high-tech industries and those in the regulated industries because managers' disclosures can be different in these industries. The Appendix describes the detailed variable measurements.

Panel A of Table 3 presents descriptive statistics on the regression variables. For the full sample of firm-quarters, the average number of good news (bad news) forecasts is 0.276 (0.324),

[19] In an untabulated sensitivity test, we also follow Chen et al. (2008) and control for contemporaneous stock performance. Specifically, we include an indicator variable for firm-quarters with market-adjusted stock returns above the sample median in the regressions. The results are quantitatively similar.

and 19.2% (21.7%) of the firm-quarters have at least one good news (bad news) forecast. The average number of analysts following is 10; the average firm size (total assets) is $6,621 million; the average market-to-book ratio is 2.828; the average earnings volatility is 0.249; the average return volatility is 2.4%; and the average stock return in the past year is 7.3%. About 30.8% of the firm-quarters have optimistic analyst forecasts as of the beginning of the quarter; 21.0% are from the high-tech industries; and 9.7% are from the regulated industries. Panel B of Table 3 reports the correlations among the independent variables. The correlations are usually small except that between firm size and analyst following.

Table 4 presents the regression results, separately for good news and bad news forecasts. The p-values are two-sided and are based on standard errors adjusted for firm-level clustering. The results are consistent with those from univariate analyses. The coefficient on *PILOT* is not significantly different from zero, indicating that the pilot and control firms do not differ in their disclosures in the pre period. The coefficient on *POST* is insignificant for good news forecasts and significantly positive for bad news forecasts, indicating that the sample firms experience an increase in bad news forecasts over time. Most importantly, the coefficient on the interaction of *PILOT* and *POST* is significantly positive for good news forecasts (p=0.013 and 0.018, respectively, for forecast frequency and likelihood). This result indicates that compared to the control firms, the pilot firms experience an increase in the frequency and likelihood of good news forecasts during the pilot program. In contrast, the coefficient on the interaction is insignificant for bad news forecasts (p=0.543 and 0.528, respectively, for forecast frequency and likelihood), indicating that the pilot firms do not differ significantly from the control firms in the change in the bad news forecasts. [20]

The results for the control variables are largely consistent with prior studies. Specifically, analyst coverage and growth opportunities are positively correlated with forecast issuance, consistent with the notion that firms with greater demand for information are more likely to issue forecasts. Both earnings volatility and return volatility are negatively correlated with forecast issuance, suggesting that managers are less likely to provide forecasts when the uncertainty is higher. Firms with higher past stock returns are more likely to issue good news forecasts. Firms with optimistic analyst forecasts are less likely to issue good news forecasts but are more likely to issue bad news forecasts, consistent with managers issuing forecasts to guide market expectations. Lastly, firms in regulated industries are less likely to issue forecasts.

In sum, we find that relative to the control firms, the pilot firms become more likely to issue good news forecasts during the pilot program. In contrast, we do not find a significant change in bad news forecasts for the pilot firms relative to the control firms. These results are consistent with that the pilot firms increase the issuance of good news forecasts when short-selling constraints become lower.

4.3 Cross-sectional analyses for good news forecasts

In this section, we explore whether the main results vary with manager and firm characteristics in a systematic way. Because we only find significant results for good news forecasts, we focus on the cross-sectional analyses for good news forecasts. Additional analyses (untabulated) indicate that similar analyses for bad news forecasts do not yield significant results.

[20] The results are quantitatively similar if we drop the firm-quarters with only bad news forecasts in the analysis of good news forecasts (instead of treating bad news forecasts as zero good news forecasts), and vice versa.

Managers' equity and option holdings

One of the main arguments underlying H1 is that managers are concerned with stock prices because their welfare is linked to stock prices (Jensen 2005). It thus follows that the results for good news forecasts should be stronger for managers whose wealth is more sensitive to stock price changes than for other managers. We test this prediction as follows. We first calculate the change in the value of CEO's equity and option holdings with 1% increase in stock prices, referred to as *Equity_Incentives*. We then include this variable and its interaction with *PILOT×POST* in regression (1). For ease of interpretation, *Equity_ Incentives* is demeaned (i.e., the sample mean is subtracted from its value) so that the coefficient on *PILOT×POST* can be interpreted as the effect for a pilot firm with average *Equity_Incentives.*.

Panel A of Table 5 reports the regression results. As reported, the coefficient on *PILOT×POST* continues to be positive. More importantly, the coefficient on the three-way interaction, *PILOT×POST× Equity_ Incentives*, is significantly positive ($p = 0.057$ and 0.053 for the frequency and likelihood of good news forecasts, respectively). This suggests that as predicted, the incentives for disclosing good news are stronger for the pilot firms when managers' wealth is more sensitive to stock price changes.

Difficulty of valuing the firm

Another argument underlying H1 is that managers have incentives to discourage short sellers because managers are concerned with the effect of short interest on the stakeholders' confidence in the firm, assuming that the stakeholders interpret short interest as a signal of stock overpricing. The stakeholders are more likely to use short interest as a signal when it is more difficult to value a firm based on public information. Thus, the results on good news forecasts should be stronger for such cases. We test this prediction by using two proxies for the difficulty of valuing a firm based on public accounting information: the magnitude of accruals (|*Accruals*| and earnings volatility (*Earn_Volatility*). Prior evidence suggests that firms with larger amount of accruals have lower earnings quality and it is more difficult to predict future earnings when earnings are volatile (e.g., Dechow et al. 1996; Sloan 1996; Gleason et al. 2008).

We use the same research design as the test based on managers' equity and option holdings. The cross-sectional tests using |*Accruals*| and *Earn_Volatility* are reported in Panel B and Panel C of Table 5, respectively. These two variables are also demeaned for ease of interpretation of the results. In both panels, the coefficient on *PILOT×POST* continues to be positive. More importantly, in Panel B, the coefficient on the three-way interaction, *PILOT×POST×* |*Accruals*|, is significantly positive (p-value = 0.054 and 0.048 for the frequency and likelihood of good news forecasts, respectively); in Panel C, the coefficient on the three-way interaction, *PILOT×POST× Earn_Volatility*, is also significantly positive (p-value = 0.051 and 0.064 for the frequency and likelihood of good news forecasts, respectively).

Overall, the cross-sectional tests indicate that the pilot firms are more likely to issue good news forecasts in response to the reduction in short-selling constraints in situations where managers are more concerned with the adverse impact of short-selling on stock prices and firm performance, such as when managers' wealth is more sensitive to changes in stock prices and when it is more difficult to value the firm based on public accounting information.

5. Additional and sensitivity tests

5.1 Timing of bad news management forecasts

The main analyses suggest that during the pilot program, the pilot firms become more likely to disclose good news, but do not differ from the control firms in disclosing bad news. This is consistent with the costs of withholding bad news. In this section, we explore whether the pilot firms become more likely to bundle bad news forecasts with good news earnings announcements. Doing so reduces the impact of bad news forecasts on stock prices, potentially discouraging short sellers (e.g., Graham et al. 2005; Segal and Segal 2013).

To test this, we estimate the following regression for bad news forecasts:

$$Inconsistent = \alpha_0 + \alpha_1 PILOT + \alpha_2 POST + \alpha_3 PILOT \times POST + \beta Control\ Variables + \varepsilon \quad (2)$$

The dependent variable, *Inconsistent*, is an indicator variable. It equals 1 if the bad news forecast is issued with a good news earnings announcement, and 0 for unbundled bad news forecasts.[21] Since the test is about whether managers bundle the bad news forecast with good news earnings announcement, the unit of observation is bad news forecast. The regression is run for 8,615 bad news forecasts for the sample firms in the pre and post periods, including bad news forecasts bundled with inconsistent earnings news and unbundled bad news forecasts.[22] In the above regression, the coefficient on *PILOT* captures whether the pilot firms are more likely to bundle bad news forecasts with good news earnings announcements before the pilot program, the

[21] Whether the earnings announcement is good news or bad news depends on whether the actual earning is higher or lower than the average of analyst forecasts issued in the 90 days prior to the earnings announcement.

[22] We exclude from this regression the bad news forecasts bundled with consistent earnings news (i.e., bad news forecasts issued with bad news earnings announcements). The reason is that for such cases, it is possibly infeasible for managers to bundle bad news forecasts with good news earnings announcements and thus including those will add noises to the regressions. Note that including such cases in the regressions (with *Inconsistent* defined as 0) leads to quantitatively similar results. Separately, we also run the regression using bad news forecasts bundled with inconsistent earnings news and unbundled bad news forecasts that are subsequently followed by inconsistent earnings news (i.e., good earnings news). The inferences remain very similar.

coefficient on *POST* captures the change in the likelihood of doing so for the sample firms, and the coefficient on *PILOT*× *POST* captures the change in this likelihood for the pilot firms compared to the control firms. We expect the coefficient on *PILOT*× *POST* to be positive if the pilot firms become more likely to bundle bad news forecasts with good news earnings announcements during the pilot program.

The list of control variables is the same as in regression (1) with the addition of two variables. The first, *Past_Bundle*, is the average probability of management forecasts being bundled with earnings announcements across the quarters in the past four years. This variable controls for the influence of past disclosure policy. The second one is the Inverse Mills Ratio for the firm-quarter, estimated from the Logit regression of the likelihood of issuing bad news forecasts as in Table 4. It is included because Regression (2) is conditional on the issuance of bad news forecasts.

Table 6 presents the regression results. As reported, the coefficient on *PILOT* is insignificantly different from zero, indicating that the pilot firms do not differ from the control firms in bundling opposite news in the pre period. The coefficient on *POST* is positive, indicating that the sample firms become more likely to bundle opposite news over time. More importantly, the coefficient on *PILOT*× *POST* is significantly positive (p=0.042), suggesting that compared to the control firms, the pilot firms become more likely to bundle bad news management forecasts with good news earnings announcements during the pilot program.

One concern with the above result is that it might simply be due to the increase in bundling management forecasts with earnings announcements over time. While this does not explain the difference between the pilot and control firms, we conduct an additional test to rule out this alternative explanation. Specifically, we construct an indicator variable, *Consistent*, which equals 1 if the bad news forecast is issued with a bad news earnings announcement, and 0 for unbundled bad news forecasts. We run Regression (2) with *Consistent* as the dependent variable. The regression is based on 6,651 bad news forecasts, including bad news forecasts bundled with consistent earnings news and unbundled bad news forecasts.[23] The results, also reported in Table 6, indicate that while the sample firms become more likely to bundle similar news over time (the coefficient on *POST* is significantly positive), there is no significant difference in such bundling between the pilot and control firms during the pilot program (the coefficient on *PILOT*× *POST* is insignificantly different from zero).

Taken together, the analyses in this section suggest that compared to the control firms, the pilot firms become more likely to bundle bad news forecasts with good news earnings announcements during the pilot program, possibly to reduce the adverse impact of bad news forecasts on share prices and discourage short sellers. [24]

5.2 Management forecast bias

The main analysis suggests that the pilot firms become more likely to disclose good news forecasts during the pilot program than the control firms. Our interpretation is that managers disclose more good news to discourage short sellers and reduce the impact of short selling. A

[23] If we run the regression using bad news forecasts bundled with consistent earnings news and only unbundled bad news forecasts that are subsequently followed by consistent earnings news (i.e., bad earnings news), the inferences remain similar.

[24] The pilot firms potentially can also bundle good news forecasts with bad news earning announcements in order to reduce the adverse impact of earnings announcements. We investigate this empirically using similar research designs and good news forecasts. We do not find that the pilot firms become more likely to bundle good news forecasts with bad news earnings announcements during the pilot program compared to the control firms. The results are not tabulated to save space.

natural question is whether managers achieve this by issuing more optimistic forecasts and misleading investors. Disclosing optimistically biased news, if suspected, can attract the interest of short sellers. Indeed, Christensen et al. (2013) predict and find that this is the case for optimistic non-GAAP reporting. Therefore we do not expect that the pilot firms will issue more optimistically biased forecasts during the pilot program compared to the control firms.

We examine the change in management forecast bias during the pilot program to confirm this. Specifically, we replace the dependent variable in Regression (1) with two proxies for management forecast bias. The first (*Bias*) is a continuous variable, measured as forecasted *EPS* minus actual *EPS* scaled by the share price three days prior to the management forecast. The second (*Optimism*) is an indicator variable, which equals 1 if forecasted *EPS* is greater than actual *EPS*, and 0 otherwise. We add three additional control variables, management forecast horizon, an indicator variable for annual forecasts, and the Inverse Mills Ratio from the Logit regression of the likelihood of issuing management forecasts. Table 7 presents the regression results. We find that the pilot firms do not differ from the control firms in forecast bias in the pre period and the sample firms issue more optimistic forecasts over time. More importantly, the coefficient on the interaction *PILOT*× *POST* is insignificantly different from zero (p=0.332 and 0.853 for the two specifications, respectively). This suggests that the pilot firms do not issue more optimistically biased forecasts during the pilot program than the control firms. In an untabulated analysis, we also separately analyze good news and bad news forecasts, and the inferences remain the same.

5.3 Removal of the tick test for all publicly listed firms on July 6, 2007

Upon the conclusion of the pilot program, the SEC decided to remove the tick test for all the U.S. exchange traded securities, effective on July 6, 2007.[25] Conceptually this is another event that can be used to test the impact of short selling on corporate disclosures. While the short-selling constraints faced by the pilot firms remain the same, the control firms now face reduced short-selling constraints. Thus, we expect changes in disclosures for the control firms similar to what the pilot firms experienced during the pilot program.

At the same time, this event is not as clean as the pilot program for two reasons. First, the SEC introduced additional rules after the removal of the tick test that can affect short selling. For example, in July 2008 the SEC required short sellers to borrow shares before trading, instead of merely locating a lender. Second, the period after the removal of the tick test largely coincides with the financial crisis, potentially confounding the tests. Specifically related to short selling, the SEC prohibited all market participants, except market makers, from shorting financial stocks from September 18 to October 8, 2008.

As an additional analysis, we analyze the change in disclosures for the control firms for this event (*REMOVAL*). To mitigate the confounding effect from the financial crisis, we exclude financial firms (i.e., those with SICs between 6000 and 6999) from this analysis. We include in the removal period the quarters that start after July 6, 2007 and end before February 24, 2010.[26] The regression is a modified version of Regression (1): we replace *PILOT* with *NPILO*T and *POST* with *REMOVAL*. *NPILOT* is the indicator variable for the control firms; it equals 1 for the

[25] The announcement by the SEC is on June 13, 2007.
[26] On February 24, 2010, SEC reinstated the tick test for certain circumstances. Thus our removal period ends on February 24, 2010.

control firms and 0 for the pilot firms. *REMOVAL* is the indicator variable for the removal period; it equals 1 for firm-quarters during the removal period, and 0 for firm-quarters in the post period (i.e., during the pilot program). The regression is based on 20,626 firm-quarters in the post period and in the removal period. The interaction of *NPILOT* and *REMOVAL* in the regression captures the change in management forecasts for the control firms during the removal period relative to the pilot firms.

Table 8 reports the regression results. For good news forecasts, the interaction *NPILOT*× *REMOVAL* has a significantly positive coefficient (p=0.026 and 0.060 for the frequency and likelihood of forecasts, respectively.)[27] For bad news forecasts, the interaction is insignificant. That is, after the removal of the tick tests for all the firms, compared to the pilot firms, the control firms experience a significant increase in the frequency and likelihood of good news forecasts, and the two groups of firms do not differ significantly in the change in bad news forecasts. The results are consistent with those for the pilot firms during the pilot program, lending further support to the main results.

5.4 Do corporate disclosures affect short interest?

One of the underlying assumptions for our analyses is that corporate disclosures can affect short interest, and more specifically, the disclosure of good news can reduce short interest. In this section, we directly test this by examining the change in short interest around management forecasts. We use three complementary measures of change in short interest, two based on

[27] The positive coefficient on *REMOVAL* indicates that with the removal of the tick test, the pilot firms do not cut back their disclosures down to the level in the pre period. This is consistent with a long run impact of reducing short-selling constraints on corporate disclosures. This contrasts with Fang et al. (2013), who find that the impact of the pilot program on earnings management is short-lived and earnings management goes back to the pre period level for the pilot stocks once the pilot program is over.

monthly short interest and one based on daily short sales.

We obtain data on monthly short interest from Compustat, which reports the level of short interest on the 15th of every month (or the preceding trading day if the 15th is not a trading day). The sample period is from the beginning of 1999 to September 30, 2010.[28] To measure the change in monthly short interest around a management forecast, we first calculate the raw change in monthly short interest as the difference between short interest reported after and before the forecast, deflated by the trading volume between the two reporting dates.[29] We then calculate the average change in monthly short interest using all the other months of the same firm over the sample period (excluding months with management forecasts or earnings announcements). The abnormal change in monthly short interest around a forecast, $SHORT_{[t-1,t]}$, is the difference between the raw change and the average change in monthly short interest. The measurement of $SHORT_{[t-1,t]}$ is similar to prior research (e.g., Christophe et al. 2004).

One potential concern with $SHORT_{[t-1,t]}$ is that it may not fully capture short sellers' reaction to the forecast. Therefore, we use an alternative measure, $SHORT_{[t-1,t+1]}$, by extending the window after the forecast by one month. The measurement is similar to $SHORT_{[t-1,t]}$ except for the longer window.[30] The drawback of this alternative measure is that it is more likely to be confounded by contemporaneous events.

Our third measure is based on daily short sales. We obtain data on intraday short sales

---

[28] First Call stopped its coverage of management forecasts on September 30, 2010.
[29] For example, for a management forecast issued on June 4, we take the difference in the short interest reported on June 15 and May 15 and then divide by the trading volume between May 15 and June 15.
[30] For example, for a management forecast issued on June 4, we take the difference in the short interest reported on July 15 and May 15 and then divide by the trading volume between May 15 and July 15. We then subtract the average change in short interest over a two-month window.

during the pilot program from NYSE and Nasdq. We aggregate the intraday short sales to obtain daily short sales, which is then deflated by the trading volume in the day. To obtain abnormal short sales around a forecast, $SHORT_{[0,2]}$, we calculate the average daily short sales in the three-day window (day 0 to day 2, where day 0 is the day of the forecast) then subtract the average daily short sales in the other days, i.e., days outside the three-day windows surrounding management forecasts or earnings announcements. The advantage of this measure is that it is based on an event window and hence less likely to be affected by confounding events. The disadvantage includes (i) the daily short sales data is only available from the exchanges during the pilot program as part of the requirements of Regulation SHO, and (ii) we do not observe the covering of short positions.

We use the following regression to examine the abnormal change in short interest around management forecasts:

$$SHORT = \alpha_0 + \alpha_1 Good\_News + \alpha_2 Bad\_News + \boldsymbol{\beta}\, Control\ \ Variables + \varepsilon \qquad (3)$$

The unit of analysis is a management forecast. To identify a benchmark group, we first calculate the forecast news as the difference between the forecast (the point forecast or the mid-point of the range forecast) and the average analyst forecast issued in the preceding 90 days, deflated by the share price three days prior to the forecast. We use neutral news forecasts as the benchmark group, including those forecasts with the absolute value of forecast news in the bottom 20% of the sample distribution. *Good_News* (*Bad_News*) equals 1 if the forecast news is positive (negative) and its magnitude is greater than the bottom 20% of the sample distribution, and 0 otherwise. The regression is run for the point and range forecasts in order to calculate the forecast news. If management forecasts affect short interest as predicted, we expect the coefficient on *Good_News* to be negative and that on *Bad_News* to be positive.

In Regression (3), we control for management forecast characteristics, including the magnitude of the forecast news (|*FN*|), forecast optimism (*Optimism*), forecast errors (*Error*), forecast range (*Range*), forecast horizon (*Horizon*), and an indicator variable for annual forecasts (*Annual*). We also control for variables that might affect the change in short interest, including analyst forecast dispersion (*Analyst Dispersion*), stock return in the prior quarter (*Prior Return*), firm size (*Size*), growth opportunity (*M/B*), and return on equity (*ROE*). Since some management forecasts are bundled with earnings announcements, we control for earnings news by including an indicator for when the earnings meet or beat analyst forecast (*Meet*). To control for the potential confounding effect of using trading volume as the scalar in calculating the change in short interest, we include concurrent trading volume (*Trade Volume*). Lastly, following Christensen et al. (2013), we include the short interest before the forecast as an additional control when $SHORT_{[0,2]}$ is the dependent variable. Please see Table 9 for detailed variable measurements.

Table 9 reports the regression results. For change in monthly short interest, we find that good news forecasts are associated with a significant decrease in short interest and bad news forecasts are associated with a significant increase in short interest. For daily short sales, we find that good news forecasts are associated with a significant reduction in daily short sales and bad news forecasts are associated with an insignificant change in daily short sales. These findings provide general support for our argument that good news disclosure can discourage short sellers while bad news disclosures can encourage short sellers.

5.5 Do pilot firms have more good news to disclose?

An alternative explanation for the results is that the pilot firms simply have more good news to disclose during the pilot program. One reason is that these firms have better performance during this period, and another possibility is that analyst forecast, the benchmark we use to identify good and bad news forecasts, reflect the negative sentiment as a result of increased short selling for the pilot firms. We conduct additional analyses to rule out these possibilities.

First, we directly compare the stock and accounting performances during the pilot program between the pilot and control firms. We also compare the change in the performance measures between the pre and post periods. We do not find any significant differences between the pilot and control firms; the two-sided p-values range from 0.319 to 0.802. In a sensitivity test, as reported in Panel A of Table 10, we further control for contemporaneous stock and accounting performances; the results are very similar to those reported in Table 4.

Second, instead of using analyst forecast as the market expectation to classify good vs. bad forecast news, we use the seasonal random walk model. The results, as reported in Panel B of Table 10, are quantitatively similar. We also explicitly examine whether analyst forecasts are more pessimistic for the pilot firms than for the control firms during the pilot program. The analyses (untabulated) indicate that there are no significant differences between the pilot and control firms.

Third, prior research find that some pilot firms experienced a reduction in investment and financing activities during the pilot program period compared to control firms (Grullon et al. 2013). To ensure that such differences do not affect the results, we explicitly control for current equity financing and capital expenditures. The results, as reported in Panel C of Table 10, are quantitatively similar.

Overall, these additional analyses indicate that the results are unlikely to be driven by differences in performances or analyst forecasts between the pilot and control firms.

6. Conclusion

In this paper, we examine how short sellers influence corporate disclosures using a natural experiment – the SEC's pilot program of suspending the tick test for the short orders over the period 2005-2007 for a group of randomly selected firms (i.e., the pilot firms). The pilot program reduces the short-selling constraints and increases short-selling for the pilot firms.

We find that compared to the other firms (i.e., the control firms), the pilot firms are more likely to increase the frequency and likelihood of good news forecasts from the pre period to the post period. With respect to bad news forecasts, we find that the pilot firms do not change the frequency or likelihood of bad news forecasts relative to the control firms; the pilot firms, though, are more likely to bundle bad news forecasts with good news earnings announcements from the pre period to the post period.

When the pilot program ended in July 2007, the SEC permanently removed the tick test for all the firms. In an additional analysis, we examine whether the control firms, now facing the reduced short-selling constraints similar to the pilot firms during the pilot program, experience similar changes in disclosures. Consistent with the main tests, we find that the control firms increase the frequency and likelihood of good news forecasts relative to the pilot firms, after the removal of the tick test. In sum, we find that the pilot and control firms respond to increases in short-selling by enhancing good news disclosures; the timing of their response corresponds to their respective changes in short-selling constraints – the implementation of the pilot program for the pilot firms and the subsequent permanent removal of the tick test for the control firms.

Our findings suggest that in response to the increase in short-selling, managers improve corporate disclosures through more good news disclosures. Our paper contributes to the literature by shedding light on how short sellers, an increasingly important group of market players, influence corporate disclosures.

References

Ajinkya, B., S. Bhojraj, and P. Sengupta. 2005. The association between outside directors, institutional investors and the properties of management earnings forecasts. *Journal of Accounting Research* 43: 343-376.

Anilowski, C., M. Feng, and D. J. Skinner. 2007. Does earnings guidance affect market returns? The nature and information content of aggregate earnings guidance. *Journal of Accounting and Economics* 44, 36-63.

Baginski, S. P., and J. M. Hassell. 1997. Determinants of management forecasts precision. *The Accounting Review* 72: 303-312.

Ball, R., and L. Shivakumar. 2006. The role of accruals in asymmetrically timely gain and loss recognition. *Journal of Accounting Research* 44 (2): 207-242.

Beneish, M.D., C. M.C. Lee, and C. Nichols. 2013. In short supply: equity overvaluation and short selling, working paper, Stanford University.

Blocher, J., A. V. Reed, and E. D. Van Wesep. 2013. Connecting two markets: An equilibrium framework for shorts, longs, and stock loans. *Journal of Financial Economics* 108: 302-322.

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

Boehmer, E., C. M. Jones, and X. Zhang. 2008. Which shorts are informed? *The Journal of Finance 63 (2)*: 491-527.

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

Brunnermeier, M. K., and O. Martin. 2013. Predatory Short Selling. *Review of Finance*, forthcoming.

Chen, S., X. Chen, and Q. Cheng. 2008. Do family firms provide more or less voluntary disclose? *Journal of Accounting Research* 46 (3): 499-536.

Cheng, Q., and K. Lo. 2006. Insider trading and voluntary disclosures. *Journal of Accounting Research* 44: 815-848.

Cheng, Q., T. Luo, and H. Yue. 2013. Managerial incentives and management forecast precision. *The Accounting Review* 88 (5): 1575-1602.

Christensen, T. E., M. S. Drake, and J. R. Thornock. 2013. Optimistic reporting and pessimistic investing: do pro forma earnings disclosures attract short sellers? *Contemporary Accounting Research*, forthcoming.

Christophe, S., M. Ferri, and J. Angel. 2004. Short-selling prior to earnings announcements. *Journal of Finance* 59: 1845-1875.

Christophe, S., M. Ferri, and J. Hsieh. 2010. Informed trading before analyst downgrades: Evidence from short sellers. *Journal of Financial Economics* 95 (1): 85–106.

De Angelis, D., G. Grullon, and S. Michenaud. 2013. Downside risk and the design of CEO incentives : evidence from a natural experiment. Working paper, Rice University.

Dechow, P. M., A. P. Hutton, L. Meulbroek, and R. G. Sloan. 2001. Short-sellers, fundamental analysis, and stock returns. *Journal of Financial Economics* 61: 77-106.

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

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

Desai, H., S. Rajgopal, and J. J. Yu. 2013. Did information intermediaries see the warning signals of the banking crisis from leading indicators in banks' financial statements? Working paper, Southern Methodist University.

Diether, K. B., K. Lee, and I. M. Werner. 2009a. It's SHO time ! short-sale price tests and market quality. *Journal of Finance* 64 (1): 37-73.

Diether, K. B., K. Lee, and I. M. Werner. 2009b. Short-sale strategies and return predictability. *Review of Financial Studies* 22 (2): 575-607.

Drake, M., L. Rees, and E. Swanson. 2011. Should investors follow the prophets or the bears? Evidence on the use of public information by analysts and short sellers. *The Accounting Review* 86 (1): 101–130.

Dye, R. A. 2001. An evaluation of "Essays on disclosure" and the disclosure literature in accounting. *Journal of Accounting and Economics* 32 (1-3): 181-235.

Engelberg, J. E., A. V. Reed, and M. C. Ringgenberg. 2012. How are shorts informed? Short sellers, news, and information processing. *Journal of Financial Economics* 105: 260-278.

Fang, V. W., A. Huang, and J. Karpoff. 2013. Short selling and earnings management: A controlled experiment. Working paper, University of Minnesota.

Gleason, C., N. Jenkins, and W. Johnson. 2008. The contagion effects of accounting restatements. *The Accounting Review* 83 (1): 83-110.

Goldstein, I., and A. Guembel. 2008. Manipulation and the allocational role of prices. *Review of Economic Studies* 75: 133-164.

Graham, J. R., C. R. Harvey, and S. Rajgopal. 2005. The economic implications of corporate financial reporting. *Journal of Accounting and Economics* 40: 3-73

Grullon, G., S. Michenaud, and J. Weston. 2013. The real effects of short-selling constraints. Working paper, Rice University.

Henry, T. R., and J. L. Koski. 2008. Short selling around seasoned equity offerings. Working paper, University of Georgia.

Hirshleifer, D., S. Teoh, and J. Yu. 2011. Short arbitrage, return asymmetry and the accruals anomaly. *Review of Financial Studies* 24: 2429-2461.

Hong, H., J. D. Kubik, and T. Fishman. 2012. Do arbitrageurs amplify economic shocks. *Journal of Financial Economics* 103: 454-470.

Jensen, M. C. 2005. Agency costs of overvalued equity. *Financial Management* 34: 5-19.

Jones, C. M., and O. A. Lamont. 2002. Short-sale constraints and stock returns. *Journal of Financial Economics* 66: 207-239.

Kecskes, A., S. A. Mansi, and A. Zhang. 2013. Are short sellers informed? Evidence from the bond market. *The Accounting Review* 88 (2): 611-639.

Khan, M., and H. Lu. 2013. Do short sellers front-run inside sales? *The Accounting Review* 88 (5): 1743-1768.

Khanna, N., and R. D. Mathews. 2012. Doing battle with short sellers: The conflicted role of blockholders in bear raids. *Journal of Financial Economics* 106: 229-246.

Laksanabunsong, C., and W. Wu. 2014. Insider purchases amid short interest spikes: A semi-pooling equilibrium. Working paper, University of Chicago.

Lamont, O. A. 2012. Go down fighting: Short sellers vs. firms. *Review of Asset Pricing Studies* 2: 1-30.

Lamont, O. A., and J. Stein. 2004. Aggregate short interest and market valuations. *American Economic Review Papers and Proceedings* 94 (2): 29-32.

Lennox, C. S., and C. W. Park. 2006. The informativeness of earnings and management's issuance of earnings forecasts. *Journal of Accounting and Economics* 42: 439-458.

Liu, H., and E. P. Swanson. 2011. Do corporate managers trade against short sellers? Working paper, Texas A&M University.

Miller, E. M. 1977. Risk, uncertainty, and divergence of opinion. *The Journal of Finance* 32 (4): 1151-1168.

Miller, G. 2002. Earnings performance and discretionary disclosure. *Journal of Accounting Research* 40 (1): 173-204

Ofek, E., and M. Richardson. 2003. Dotcom mania: The rise and fall of Internet stock prices. The *Journal of Finance* 58 (3): 1113-1138.

Pownall, G., and P. J. Simko. 2005. The information intermediary role of short sellers. *The Accounting Review* 80 (3): 941-966

Richardson, S., S. H. Teoh, and P. D. Wysocki. 2004. The walk-down to beatable analyst forecasts: the role of equity issuance and insider trading incentives. *Contemporary Accounting Research* 21: 885-924.

Rogers, J. L., and A. Van Buskirk. 2013. Bundled forecasts in empirical accounting research. Journal of Accounting and Economics 55: 43-65.

Safieddine, A., W. J. Wilhelm, Jr. 1996. An empirical investigation of short-selling activity prior to seasoned equity offerings. *The Journal of Finance* 51 (2): 729-749.

Savor, P., and M. Gamboa-Cavazos. 2011. Holding on to your shorts: When do short sellers retreat? Working paper, University of Pennsylvania.

Segal, B., and D. Segal. 2013. The opportunistic reporting of material events and the apparent misconception of investors' reaction. Working paper, INSEAD.

Shleifer, A., and R. W. Vishny. 1997. The limits of arbitrage. *The Journal of Finance* 52 (1): 735-53.

Skinner, D. 1994. Why firms voluntarily disclose bad news. *Journal of Accounting Research* 32: 38-60.

Skinner, D. 1997. Earnings disclosures and stockholder lawsuits. *Journal of Accounting and Economics* 23: 249-262.

Sloan, R. 1996. Do stock prices fully reflect information in accruals and cash flows about future earnings? *The Accounting Review* 71 (3): 289-315.

Verrecchia, R. E. 2001. Essays on disclosure. *Journal of Accounting and Economics* 32 (1-3): 97-180.

Appendix Variable definitions

MF_N=	The number of management forecasts issued in the quarter;
MF_D=	The likelihood of issuing management forecasts in the quarter; it equals 1 if managers issue at least one forecast in the quarter and 0 otherwise;
PILOT=	Indicator for the pilot firms, defined as 1 if a firm was selected by the SEC for the pilot program, 0 otherwise;
POST=	Indicator for the post period, defined as 1 for the duration of the pilot program, including fiscal quarters that start after May 2, 2005 and end before July 6, 2007; it is 0 for the pre period, including the fiscal quarters that start after January 1, 2002 and end before July 28, 2004;
Analyst Following=	The number of analysts who issue forecasts for the firm in the previous year;
Size=	Total assets (in millions), measured at the end of the previous quarter; for regressions, we take the natural logarithm;
M/B=	Market value to book value of equity, measured at the end of the previous quarter;
Earnings Volatility=	Standard deviation of quarterly ROE (return on equity) in the previous four years;
Return Volatility=	Volatility of daily stock returns in the previous quarter;
Prior Return=	Cumulative size-adjusted returns in the previous four quarters;
Analyst Optimism=	Indicator for analyst optimism, defined as 1 if the consensus analyst forecast at the beginning of the quarter is optimistic relative to the realized earnings and 0 otherwise;
High Tech=	Indicator for high-tech firms, defined as 1 for firms in the industries with SICs of 2833-2836, 8731-8734, 7371-7379, 3570-3577, or 3600-3674;
Regulated=	Indicator for regulated firms, defined as 1 for firms in the industries with SICs of 4812-4813, 4833, 4841, 4811-4899, 4922-4924, 4931, 4941, 6021-6023, 6035-6036, 6141, or 6311.

Figure 1 Time-series Trend of Short Interest

This graph depicts the time-series trend of short interest, measured as the average monthly short interest scaled by the number of outstanding shares. The graph is based on all the firms with available data on short interest and the number of outstanding shares from Compustat over the period 1990-2012.

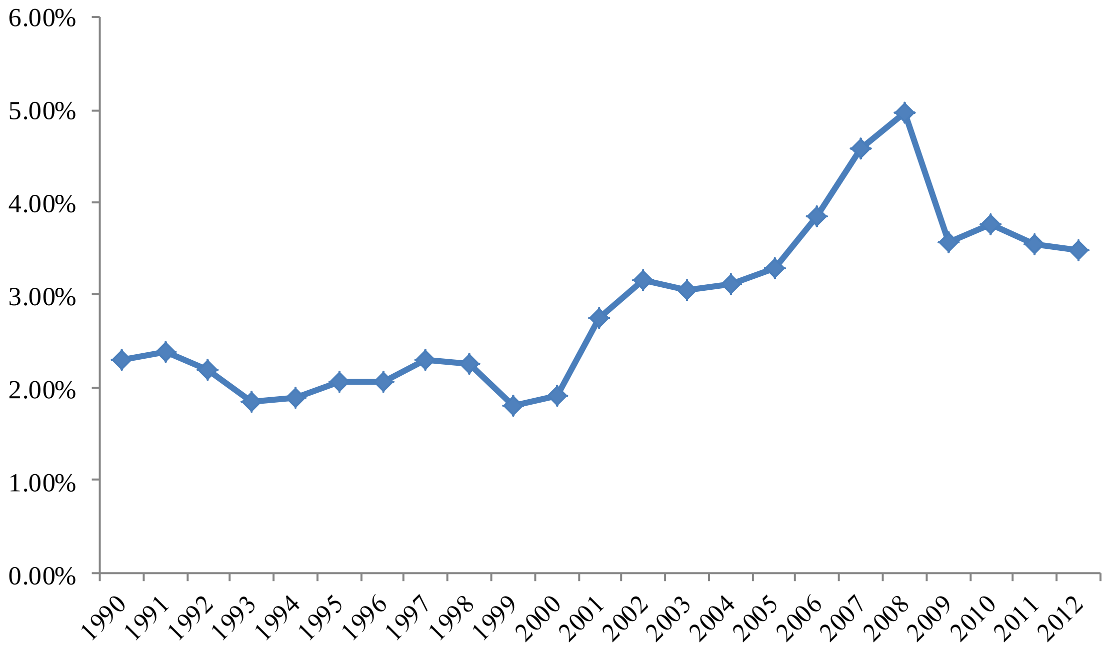
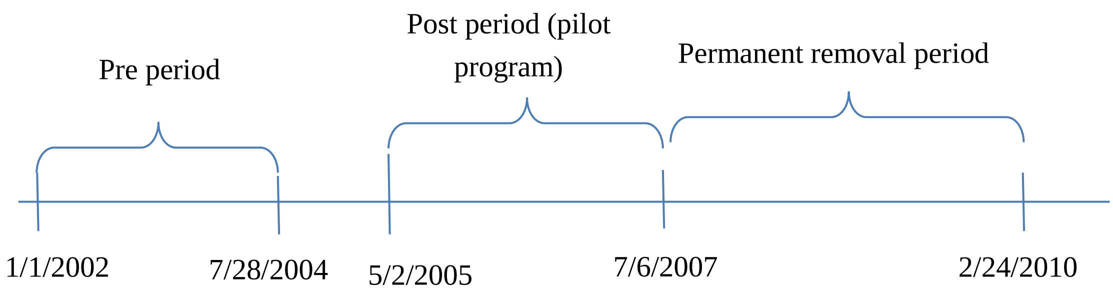


Figure 2 Timeline

This graph depicts the timeline.



Key dates:

6/23/2004 The SEC adopted Regulation SHO.

7/28/2004 The SEC announced the list of the pilot stocks.

5/2/2005 The pilot program started.

7/6/2007 The pilot program ended and the SEC permanently suspended the tick test for all publicly listed stocks.

2/24/2010 The SEC reinstated the revised tick test, which only applies under limited circumstances.

Table 1 Sample Selection, Industry Composition, and Comparison of the Pilot and Control Firms

This table describes the sample selection process, the industry composition of the sample, and compares the pilot and control firms in terms of key firm characteristics.

Panel A: Sample selection

Restrictions		The number of firms
Firms included in the Russell 3000 index in 2004*		3,206
Less:		
Firms not listed on NYSE, AMEX or Nasdaq, or firms with IPOs after April 30, 2004	21	
Firms not in the Russell 3000 index in 2005	374	
Firms that change tickers during the pilot program	83	
Firms without required financial, stock price, analyst data in the post period	181	
Firms without required financial, stock price, analyst data in the pre period	78	
Firms without the same number of quarters in the pre and post periods	117	
Final sample		2,352
Pilot firms		768
Control firms		1,484

* Note that to construct the Russell 3000 index, the 4000 firms listed on the U.S. exchanges with the largest market capitalizations are first selected. Among those, the U.S. firms are included in the Russell 3000 index. Therefore, the number of firms in the Russell 3000 index, usually around 3000, can be different from 3000. It happens to be higher than 3000 in 2004.

Table 1 (Cont'd)

Panel B: Industry composition

This panel presents the industry composition for the sample of 2,352 firms.

Industry	Number of firms	Percentage (%)	Industry	Number of firms	Percentage (%)
Banking	257	10.9%	Chemicals	43	1.8%
Business Services	250	10.6%	Construction Materials	38	1.6%
Trading	159	6.8%	Automobiles and Trucks	33	1.4%
Pharmaceutical Products	152	6.5%	Food Products	32	1.4%
Electronic Equipment	140	5.9%	Restaurants, Hotels, Motels	31	1.3%
Retail	134	5.7%	Electrical Equipment	30	1.3%
Insurance	94	4.0%	Consumer Goods	29	1.2%
Utilities	92	3.9%	Apparel	29	1.2%
Computers	84	3.6%	Healthcare	27	1.1%
Machinery	76	3.2%	Steel Works	27	1.1%
Petroleum and Natural Gas	72	3.1%	Construction	26	1.1%
Medical Equipment	71	3.0%	Business Supplies	25	1.1%
Communication	67	2.8%	Entertainment	23	1.0%
Wholesale	53	2.3%	Personal Services	23	1.0%
Measuring and Control Equipment	51	2.2%	Others *	150	6.0%
Transportation	44	1.9%			

* These include 18 other industries, such as printing and publishing, recreation, rubber and plastic products, agriculture, and aircraft. These industries have the lowest number of sample firms.

Table 1 (Cont'd)

Panel C: The comparison between the pilot and control firms in key firm characteristics before the pilot program

This panel presents descriptive statistics on firm characteristics in fiscal year 2003, the year before the SEC selected the pilot firms. The statistics are presented separately for the pilot and control firms. A sample firm is a pilot firm if its stock is designated as a pilot stock by the SEC and is a control firm otherwise. *Size* is total assets (in millions), *M/B* is the market-to-book ratio, *Leverage* is the ratio of total debt to total assets, *ROE* is the ratio of earnings before extraordinary items to book value of stockholders' equity, *Trade Volume* is the average monthly trading volume (in number of shares), and *Analyst Following* is the number of analysts following the firm.

	Pilot firms				Control firms				P-value for the differences between the pilot and control firms in	
	N	Mean	Median	Std.	N	Mean	Median	Std.	Mean	Median
Size	768	5,322	997	14,687	1,484	6,052	1,023	17,731	0.29	0.53
M/B	768	3.11	2.29	3.02	1,484	3.16	2.31	3.31	0.69	0.90
Leverage	768	0.22	0.20	0.20	1,484	0.21	0.18	0.20	0.34	0.23
ROE	768	0.11	0.10	0.73	1,484	0.08	0.10	0.31	0.38	0.15
Trade Volume	768	208,631	65,236	420,839	1,484	202,244	60,211	401,414	0.73	0.53
Analyst Following	768	10	7	9	1,484	10	7	8	0.65	0.85

Table 2 Short Selling and Management Forecast Frequency – Univariate Analysis

This table reports the average quarterly frequency of management forecasts in the pre and post periods. The sample includes 34,718 firm-quarters from 2,352 firms, including 768 pilot firms and 1,484 control firms. The pre period includes the fiscal quarters that start after January 1, 2002 and end before July 28, 2004, and the post period includes the fiscal quarters that start after May 2, 2005 and end before July 6, 2007. A management forecast is classified as good (bad) news if the point estimate, or the mid-point of the range forecast, is above (below) the average of analyst forecasts issued in the 90 days before the management forecast. For open-ended management forecasts, the forecast is classified as good (bad) news when its bottom (upper) bound is higher (lower) than average analyst forecast. For qualitative forecasts, the forecast is classified as good news if the forecast is coded as "meets or exceeds expectations" or "above expectations," and as bad news if the forecast is coded as "below expectations" or "may not meet expectations." The p-values are based on two-tailed t-tests.

Panel A: Good news management forecasts

	Pre period	Post period	Difference (P-value)
Pilot firms	0.261	0.295	0.034
			(0.007)
Control firms	0.280	0.269	-0.011
			(0.185)
Difference	-0.019	0.026	**0.045**
(P-value)	(0.080)	(0.013)	**(0.003)**

Panel B: Bad news management forecasts

	Pre period	Post period	Difference (P-value)
Pilot firms	0.307	0.373	0.065
			(0.001)
Control firms	0.290	0.343	0.053
			(0.001)
Difference	0.016	0.029	**0.012**
(P-value)	(0.149)	(0.016)	**(0.448)**

Table 3 Descriptive Statistics

Panel A: Descriptive statistics on the regression variables

This panel presents descriptive statistics on the regression variables. The sample includes 34,718 firm-quarters from 2,352 firms, including 768 pilot firms and 1,484 control firms. The pre period includes the fiscal quarters that start after January 1, 2002 and end before July 28, 2004, and the post period includes the fiscal quarters that start after May 2, 2005 and end before July 6, 2007. Please see Table 2 for the classification of good news (bad news) management forecasts and the Appendix for variable definitions.

		Percentile					
	Mean	5%	25%	50%	75%	95%	Std. Dev.
MF_N (Good news forecast frequency)	0.276	0	0	0	0	2	0.663
MF_N (Bad news forecast frequency)	0.324	0	0	0	0	2	0.738
MF_D (Good news forecast likelihood)	0.192	0	0	0	0	1	0.394
MF_D (Bad news forecast likelihood)	0.217	0	0	0	0	1	0.412
Analyst Following	10	0	4	8	14	27	8
Size (in millions)	6,621	86	369	1,151	3,680	28,464	19,975
M/B	2.828	0.774	1.506	2.163	3.354	7.743	3.177
Earnings Volatility	0.249	0.024	0.039	0.055	0.103	0.877	0.821
Return Volatility	0.024	0.010	0.015	0.020	0.029	0.049	0.014
Prior Return	0.073	-0.496	-0.134	0.047	0.258	0.730	0.372
Analyst Optimism	0.308	0	0	0	1	1	0.462
High Tech	0.210	0	0	0	0	1	0.407
Regulated	0.097	0	0	0	0	1	0.296

Table 3 (Cont'd)

Panel B: Correlations among the independent variables

This panel presents correlations among the independent variables. The sample includes 34,718 firm-quarters from 2,352 firms, including 768 pilot firms and 1,484 control firms. The pre period includes the fiscal quarters that start after January 1, 2002 and end before July 28, 2004, and the post period includes the fiscal quarters that start after May 2, 2005 and end before July 6, 2007. Please see the Appendix for variable definitions. *, ** indicate significance at the 0.05 and 0.01 levels, respectively, based on two-tailed tests.

	PILOT	*POST*	*Analyst Following*	*Size*	*M/B*	*Earnings Volatility*	*Return Volatility*	*Prior Return*	*Analyst Optimism*	*High Tech*
POST	0.00									
Analyst Following	0.01*	0.00								
Size	-0.02**	0.03**	0.41**							
M/B	0.01	0.05**	0.09**	-0.04**						
Earnings Volatility	-0.02**	0.00	-0.02**	-0.05**	0.10**					
Return Volatility	-0.03**	-0.27**	-0.09**	-0.18**	-0.01*	0.20**				
Prior Return	0.00	-0.17**	-0.03**	-0.03**	0.19**	0.04**	0.03**			
Analyst Optimism	0.00	0.06**	0.03**	0.00	-0.03**	0.00	0.00	-0.09**		
High Tech	-0.02**	0.00	0.16**	-0.10**	0.12**	0.13**	0.31**	0.00	-0.04**	
Regulated	-0.03**	0.00	0.01*	0.13**	-0.11**	-0.01	-0.10**	-0.03**	0.03**	-0.17**

Table 4 Short Selling and Management Forecasts – Multivariate Analysis

This table reports results from the following regression:

$$MF = \alpha_0 + \alpha_1 PILOT + \alpha_2 POST + \alpha_3 PILOT \times POST + \beta Control\ \ Variables\ + \varepsilon$$

The sample includes 34,718 firm-quarters from 2,352 firms, including 768 pilot firms and 1,484 control firms. The pre period includes the fiscal quarters that start after January 1, 2002 and end before July 28, 2004, and the post period includes the fiscal quarters that start after May 2, 2005 and end before July 6, 2007. We use Tobit (Logit) regression when *MF_N* (*MF_D*) is the dependent variable. Please see Table 2 for the classification of good news (bad news) management forecasts and the Appendix for variable definitions. The p-values are two-sided and are based on standard errors adjusted for firm-level clustering.

	Good News Forecasts				**Bad News Forecasts**			
	MF_N		*MF_D*		*MF_N*		*MF_D*	
	Coef.	P-value	Coef.	P-value	Coef.	P-value	Coef.	P-value
Intercept	-1.976	0.001	-1.461	0.001	-2.037	0.001	-1.431	0.001
PILOT	-0.141	0.134	-0.113	0.141	0.035	0.709	0.026	0.725
POST	-0.033	0.554	0.005	0.913	0.141	0.006	0.113	0.007
PILOT* × *POST	**0.222**	**0.013**	**0.170**	**0.018**	**0.050**	**0.543**	**0.042**	**0.528**
Analyst Following	0.782	0.001	0.626	0.001	0.851	0.001	0.643	0.001
Size	-0.013	0.662	-0.016	0.483	-0.036	0.217	-0.037	0.112
M/B	0.026	0.007	0.019	0.016	0.022	0.038	0.016	0.069
Earnings Volatility	-0.163	0.006	-0.158	0.003	-0.151	0.010	-0.133	0.010
Return Volatility	-19.26	0.001	-16.52	0.001	-15.52	0.001	-13.20	0.001
Prior Return	0.533	0.001	0.429	0.001	0.059	0.367	0.011	0.821
Analyst Optimism	-0.549	0.001	-0.444	0.001	0.311	0.001	0.217	0.001
High Tech	0.042	0.681	0.060	0.481	-0.273	0.007	-0.175	0.034
Regulated	-0.578	0.001	-0.445	0.001	-0.603	0.001	-0.404	0.001
N	34,718		34,718		34,718		34,718	
Adjusted R^2	3.93%		5.34%		3.40%		4.83%	

Table 5 Short Selling and Management Forecasts – Cross-sectional Analyses for Good News Forecasts

This table reports results from the following regression for good news forecasts:

$$MF = \alpha_0 + \alpha_1 PILOT + \alpha_2 POST + \alpha_3 PILOT \times POST + \alpha_4 Conditional\_Variable$$
$$+ \alpha_5 PILOT \times Conditional\_Variable + \alpha_6 POST \times Conditional\_Variable +$$
$$\alpha_7 PILOT \times POST \times Conditional\_Variable + \boldsymbol{\beta} ControlVariables + \varepsilon$$

The full sample includes 34,718 firm-quarters from 2,352 firms, including 768 pilot firms and 1,484 control firms. The pre period includes the fiscal quarters that start after January 1, 2002 and end before July 28, 2004, and the post period includes the fiscal quarters that start after May 2, 2005 and end before July 6, 2007. The sample size varies across panels because of additional data requirement. We use Tobit (Logit) regression when *MF_N* (*MF_D*) is the dependent variable. Please see Table 2 for the classification of good news (bad news) management forecasts and the Appendix for variable definitions. *Conditional_Variable* is *Equity_Incentives* in Panel A, |*Accruals*| in Panel B, and *Earn_Volatility* in Panel C. *Equity_Incentives* is the natural logarithm of the change in the value of CEO's stock and option holdings with a 1% increase in stock price. |*Accruals*| is the absolute value of total accruals (earnings minus operating cash flows) scaled by average total assets. *Earn_Volatility* is the standard deviation of quarterly return on equity in the previous four years. These three variables are demeaned (i.e., the sample mean is subtracted from the value). The p-values are two-sided and are based on standard errors adjusted for firm-level clustering.

Panel A: Managers' equity and option holdings

	MF_N		*MF_D*	
	Coef.	P-value	Coef.	P-value
Intercept	-0.719	0.014	-0.525	0.030
PILOT	-0.111	0.305	-0.098	0.276
POST	-0.050	0.420	-0.005	0.922
PILOT* × *POST	**0.158**	**0.122**	**0.123**	**0.148**
Equity_Incentives	0.142	0.003	0.118	0.004
PILOT× Equity_Incentives	-0.104	0.150	-0.097	0.105
POST× Equity_Incentives	-0.064	0.154	-0.058	0.126
PILOT × POST× Equity_Incentives	**0.151**	**0.057**	**0.122**	**0.053**
Analyst Following	0.532	0.001	0.431	0.001
Size	-0.094	0.010	-0.082	0.006
M/B	0.016	0.195	0.011	0.284
Earnings Volatility	-0.059	0.391	-0.072	0.227
Return Volatility	-19.44	0.001	-17.11	0.001
Prior Return	0.349	0.001	0.287	0.001
Analyst Optimism	-0.609	0.001	-0.500	0.001
High Tech..	0.169	0.145	0.170	0.082
Regulated	-0.236	0.123	-0.171	0.183
N	20,308		20,308	
Adjusted R^2	2.46%		3.93%	

Table 5 (Cont'd)

Panel B: The magnitude of accruals

	MF_N		*MF_D*	
	Coef.	P-value	Coef.	P-value
Intercept	-2.127	0.001	-1.627	0.001
PILOT	-0.155	0.103	-0.126	0.107
POST	-0.047	0.396	-0.002	0.959
PILOT **×** ***POST***	**0.222**	**0.014**	**0.170**	**0.021**
|*Accruals*|	0.483	0.416	0.284	0.553
PILOT × |*Accruals*|	-1.662	0.115	-1.260	0.152
POST × |*Accruals*|	-3.313	0.001	-2.654	0.001
PILOT **×** ***POST*** **×** **|*Accruals*|**	**2.996**	**0.054**	**2.555**	**0.048**
Analyst Following	0.663	0.001	0.535	0.001
Size	0.051	0.087	0.036	0.143
M/B	0.023	0.014	0.017	0.028
Earnings Volatility	-0.160	0.005	-0.157	0.003
Return Volatility	-19.92	0.001	-17.09	0.001
Prior Return	0.503	0.001	0.409	0.001
Analyst Optimism	-0.549	0.001	-0.450	0.001
High Tech	0.016	0.877	0.041	0.623
Regulated	-0.633	0.001	-0.490	0.001
N	31,246		31,246	
Adjusted R^2	3.98%		3.93%	

Table 5 (Cont'd)

Panel C: Earnings volatility

	MF_N		*MF_D*	
	Coef.	P-value	Coef.	P-value
Intercept	-2.011	0.001	-1.491	0.001
PILOT	-0.150	0.109	-0.122	0.110
POST	-0.047	0.400	-0.014	0.760
PILOT* × *POST	**0.233**	**0.009**	**0.186**	**0.011**
Earn_Volatility	-0.002	0.978	-0.022	0.714
PILOT× Earn_Volatility	-0.257	0.022	-0.189	0.057
POST× Earn_Volatility	-0.278	0.011	-0.264	0.017
PILOT × POST × Earn_Volatility	**0.310**	**0.051**	**0.284**	**0.064**
Analyst Following	0.781	0.001	0.626	0.001
Size	-0.012	0.662	-0.017	0.477
M/B	0.027	0.004	0.020	0.011
Return Volatility	-19.34	0.001	-16.66	0.001
Prior Return	0.521	0.001	0.417	0.001
Analyst Optimism	-0.550	0.001	-0.445	0.001
High Tech	0.043	0.677	0.062	0.472
Regulated	-0.574	0.001	-0.443	0.001
N	34,718		34,718	
Adjusted R^2	3.98%		5.41%	

Table 6 Short Selling and Timing of Bad News Management Forecasts

This table reports Logit regression analysis of the timing of bad news management forecasts based on the following model:

$$Inconsistent = \alpha_0 + \alpha_1 PILOT + \alpha_2 POST + \alpha_3 PILOT \times POST + \beta Control\,Variables + \varepsilon$$

$$Consistent = \alpha_0 + \alpha_1 PILOT + \alpha_2 POST + \alpha_3 PILOT \times POST + \beta Control\,Variables + \phi$$

Inconsistent equals 1 if the bad news management forecast is issued with a good news earnings announcement, and 0 for unbundled bad news forecasts. *Consistent* equals 1 if the bad news management forecast is issued with a bad news earnings announcement, and 0 for unbundled bad news forecasts. Please see Table 2 for the classification of good news (bad news) management forecasts. Whether the earnings announcement is good news or bad news depends on whether the actual earnings is higher or lower than the average of analyst forecasts issued in the 90 days prior to the earnings announcement. *Past_Bundle* is the average probability of management forecasts being bundled with earnings announcements across the quarters in the past four years. *Inverse Mills Ratio* for the firm-quarter is estimated from the Logit regression of the likelihood of bad news forecasts as in Table 4. Please see the Appendix for the measurement of the other variables. The sample includes 11,262 bad news forecasts issued by 768 pilot firms and 1,484 control firms in the pre and post periods. When *Inconsistent* (*Consistent*) is the dependent variable, the regression is based on bad news forecasts bundled with inconsistent (consistent) earnings announcements and unbundled bad news forecasts. The p-values are two-sided and are based on standard errors adjusted for firm-level clustering.

	Dependent variable = *Inconsistent*		Dependent variable = *Consistent*	
	Coef.	P-value	Coef.	P-value
Intercept	-1.773	0.001	-1.418	0.001
PILOT	-0.049	0.586	0.155	0.154
POST	0.492	0.001	0.618	0.001
PILOT* × *POST	**0.232**	**0.042**	**-0.095**	**0.485**
Analyst Following	-0.036	0.001	-0.017	0.028
Size	0.061	0.021	-0.026	0.395
M/B	0.030	0.007	0.000	0.993
Earnings Volatility	0.014	0.771	-0.135	0.038
Return Volatility	-0.530	0.892	5.198	0.249
Prior Return	0.047	0.620	-0.783	0.001
High Tech	0.186	0.040	-0.016	0.882
Regulated	0.022	0.874	0.064	0.671
Past_Bundle	1.360	0.001	0.931	0.001
Inverse Mills Ratio	0.594	0.001	0.465	0.001
N	8,615		6,651	
Adjusted R^2	6.38%		5.63%	

Table 7 Short Selling and Management Forecast Bias

This table reports the following regression results:

$$Bias / Optimism = \alpha_0 + \alpha_1 PILOT + \alpha_2 POST + \alpha_3 PILOT \times POST + \boldsymbol{\beta} Control \quad Variables \quad + \varepsilon$$

The regression is run for 21,732 management forecasts issued by 768 pilot firms and 1,484 control firms in the pre and post periods. We use OLS (Logit) regression when *Bias* (*Optimism*) is the dependent variable. *Bias* is forecasted EPS minus actual *EPS*, scaled by the share price three days prior to the forecast. *Optimism* is 1 if forecasted *EPS* is greater than actual *EPS*, and 0 otherwise. *Horizon* is the natural logarithm of the number of days between the management forecast and the earnings announcement. *Annual* equals 1 for annual forecasts and 0 for quarterly forecasts. *Inverse Mills Ratio* for the firm-quarter is estimated from the Logit regression of the likelihood of issuing management forecasts using the model specification in Table 4. Please see the Appendix for the measurement of the other variables. The p-values are two-sided and are based on standard errors adjusted for firm-level clustering.

	Bias		*Optimism*	
	Coef.	P-value	Coef.	P-value
Intercept	-0.071	0.001	-0.327	0.001
PILOT	0.000	0.913	0.006	0.770
POST	0.003	0.097	0.040	0.012
PILOT* × *POST	**0.003**	**0.332**	**-0.005**	**0.853**
Analyst Following	0.000	0.071	-0.003	0.003
Size	0.001	0.142	-0.007	0.182
M/B	0.000	0.847	-0.005	0.010
Earnings Volatility	-0.002	0.127	-0.009	0.377
Return Volatility	0.673	0.001	4.466	0.001
Prior Return	-0.023	0.001	-0.206	0.001
High Tech	-0.003	0.150	-0.065	0.001
Regulated	0.001	0.731	-0.030	0.291
Horizon	0.011	0.001	0.130	0.001
Annual	-0.003	0.029	0.001	0.960
Inverse Mills Ratio	-0.009	0.001	-0.053	0.002
N	21,732		21,732	
Adjusted R^2	4.06%		7.26%	

Table 8 Short Selling and Management Forecasts – Analysis of the Permanent Removal Period

This table reports results from the following regression:

$$MF = \alpha_0 + \alpha_1 NPILOT + \alpha_2 REMOVAL + \alpha_3 NPILOT \times REMOVAL + \boldsymbol{\beta}\, Control\quad Variables\ + \varepsilon$$

The sample includes 20,626 firm-quarters from 2,352 firms, including 768 pilot firms and 1,484 control firms from the post period and the permanent removal period. The post period includes the fiscal quarters that start after May 2, 2005 and end before July 6, 2007, and the permanent removal period includes the fiscal quarters that start after July 6, 2007 and end before February 24, 2010. We use Tobit (Logit) regression when *MF_N* (*MF_D*) is the dependent variable. Please see Table 2 for the classification of good news (bad news) management forecasts. *NPILOT* equals 1 for the control firms and zero for the pilot firms. *REMOVAL* equals 1 for firm-quarters in the permanent removal period and 0 for firm-quarters in the post period. Please see Appendix A for the measurement of the other variables. The p-values are two-sided and are based on standard errors adjusted for firm-level clustering.

	Good News Forecasts				**Bad News Forecasts**			
	MF_N		*MF_D*		*MF_N*		*MF_D*	
	Coef.	P-value	Coef.	P-value	Coef.	P-value	Coef.	P-value
Intercept	-2.731	0.001	-2.201	0.001	-3.030	0.001	-2.239	0.001
NPILOT	-0.184	0.053	-0.138	0.105	-0.069	0.492	-0.063	0.466
REMOVAL	0.239	0.003	0.261	0.001	0.203	0.008	0.205	0.002
NPILOT × REMOVAL	**0.201**	**0.026**	**0.152**	**0.060**	**0.004**	**0.962**	**0.029**	**0.705**
Analyst Following	0.531	0.001	0.441	0.001	0.623	0.001	0.491	0.001
Size	0.091	0.003	0.070	0.012	0.099	0.004	0.063	0.034
M/B	0.006	0.584	0.006	0.573	0.008	0.490	0.004	0.691
Earnings Volatility	-0.243	0.001	-0.274	0.001	-0.208	0.001	-0.194	0.004
Return Volatility	-22.41	0.001	-21.83	0.001	-16.98	0.001	-16.47	0.001
Prior Return	0.351	0.001	0.298	0.001	-0.117	0.109	-0.137	0.025
Analyst Optimism	-0.634	0.001	-0.573	0.001	0.029	0.517	-0.018	0.644
High Tech	0.001	0.990	0.037	0.693	-0.396	0.001	-0.301	0.001
Regulated	-0.530	0.004	-0.483	0.003	-0.554	0.003	-0.397	0.016
N	20,626		20,626		20,626		20,626	
Adjusted R^2	5.46%		7.64%		3.86%		6.29%	

Table 9 Change in Short Interest around Management Forecasts

This table reports the regression analysis of the change in short interest around the management forecasts based on the following regression:

$$SHORT = \alpha_0 + \alpha_1 Good\_News + \alpha_2 Bad\_News + \boldsymbol{\beta}\, Control\, Variables + \varepsilon$$

SHORT is the change in short interest around the management forecast and is one of the following three variables: $SHORT_{[t\text{-}1,t]}$, $SHORT_{[t\text{-}1,t+1]}$, and $SHORT_{[0,\,2]}$. $SHORT_{[t\text{-}1,t]}$ ($SHORT_{[t\text{-}1,t+1]}$) is the abnormal change in monthly short interest from month t-1 to month t (t+1) around the forecast. $SHORT_{[0,\,2]}$ is the average daily abnormal short sale in the three-day window around the forecast ([0, 2], where day 0 is the day of the forecast). The regression for $SHORT_{[t\text{-}1,t]}$ and $SHORT_{[t\text{-}1,t+1]}$ is based on 56,348 management forecasts (point and range forecasts) issued between January 1, 1999 and September 30, 2010. The regression for $SHORT_{[0,\,2]}$ is based on 15,230 management forecasts (point and range forecasts) issued during the pilot program for stocks traded on NYSE and Nasdaq. The p-values are two-sided and are based on standard errors adjusted for firm-level clustering.

	$SHORT_{[t\text{-}1,t]}$		$SHORT_{[t\text{-}1,t+1]}$		$SHORT_{[0,2]}$	
	Coef.	P-value	Coef.	P-value	Coef.	P-value
Intercept	-0.878	0.092	-2.267	0.001	2.369	0.001
Good_News	**-0.115**	**0.093**	**-0.153**	**0.094**	**-0.341**	**0.096**
Bad_News	**0.115**	**0.071**	**0.192**	**0.021**	**0.029**	**0.891**
|FN|	-6.520	0.125	-5.445	0.288	17.771	0.003
Optimism	0.122	0.052	0.324	0.001	0.012	0.941
Error	-0.072	0.959	-1.305	0.553	0.090	0.890
Range	-3.491	0.001	-8.769	0.001	26.381	0.488
Horizon	0.303	0.001	0.666	0.001	-0.114	0.372
Annual	-0.205	0.003	-0.433	0.001	0.108	0.533
Analyst Dispersion	-0.248	0.801	-2.046	0.157	1.646	0.713
Prior Return	-1.452	0.001	-1.732	0.001	1.170	0.088
Size	-0.006	0.854	-0.010	0.792	-0.042	0.391
M/B	-0.005	0.272	0.001	0.864	-0.123	0.001
ROE	-0.460	0.091	-0.345	0.455	1.874	0.195
Meet	0.036	0.609	0.099	0.339	-0.340	0.037
Trade Volume	-0.025	0.527	-0.028	0.556	-0.368	0.001
Prior short interest					-0.587	0.723
N	56,348		56,348		15,230	
Adjusted R^2	0.44%		0.62%		1.00%	

Table 9 (Cont'd)

Variable definitions:

SHORT$_{[t-1,t]}$ (*SHORT*$_{[t-1,t+1]}$) =	the difference between the raw change and the normal change in monthly short interest, where the raw change is the difference in the short interest reported after the forecast (one month after the forecast) and the short interest reported before the forecast, deflated by the trading volume between the two reporting dates, and the normal change is the average change in short interest over one month (two months) for all the months without management forecasts or earnings announcements, deflated by the trading volume between the two reporting dates;
SHORT$_{[0,\ 2]}$ =	the average daily short sales in the three day window [0,+2] (deflated by daily trading volume) minus the average daily short sales outside the three day windows surrounding management forecasts or earnings announcements (also deflated by daily trading volume), where day 0 is the day of the management forecast;
Good_News (*Bad_News*) =	an indicator variable that equals 1 if the forecast news is positive (negative) and the absolute value of the forecast news is greater than the bottom 20% of the sample distribution; management forecasts with the absolute value of the forecast news in the bottom 20% of the sample distribution are treated as neutral news and serve as the benchmark group;
|*FN*| =	the absolute value of the forecast news, where the forecast news is the difference between forecasted *EPS* and the average analyst forecast issued in the preceding 90 days, scaled by the share price three days before the forecast;
Optimism =	1 if forecasted *EPS* is higher than actual *EPS*, and 0 otherwise;
Error =	the absolute value of the difference between forecasted *EPS* and actual *EPS*, scaled by the share price three days before the forecast;
Range =	the forecast range for range forecasts, scaled by the share price three days before the forecast; 0 for point forecasts;
Horizon =	the natural logarithm of the number of days between the forecast and the corresponding earnings announcement;
Annual =	1 for annual management forecasts, and 0 for quarterly management forecasts;
Analyst Dispersion =	the standard deviation of analyst forecasts issued in the 90 days before the forecast, scaled by the share price three days before the forecast;
Prior Return =	cumulative size-adjusted stock returns in the prior 90 days;
Size =	the natural logarithm of total assets (in millions) at the end of the year before the forecast;
M/B =	the market-to-book ratio at the end of the year before the forecast;
ROE =	the ratio of earnings before extraordinary items to common shareholders' equity for the prior year;
Meet =	1 if the actual earnings are the same as, or higher than, the consensus analyst forecast, and 0 otherwise, for bundled management forecasts; 0 for unbundled management forecasts;
Trade Volume =	the natural logarithm of the concurrent trading volume; it is the total trading volume between the two reporting dates of short interest when *SHORT*$_{[t-1,t]}$ or *SHORT*$_{[t-1,t+1]}$ is the dependent variable; it is the total trading volume from day 0 to day 2 when *SHORT*$_{[0,\ 2]}$ is the dependent variable;
Prior Short Interest =	the monthly short interest reported before the forecast, scaled by the number of shares outstanding in the previous month.

Table 10 Sensitivity Tests

This table reports results from the following regression:

$$MF = \alpha_0 + \alpha_1 PILOT + \alpha_2 POST + \alpha_3 PILOT \times POST + \beta Control\ \ Variables\ + \varepsilon$$

The sample includes 34,718 firm-quarters from 2,352 firms, including 768 pilot firms and 1,484 control firms. The pre period includes the fiscal quarters that start after January 1, 2002 and end before July 28, 2004, and the post period includes the fiscal quarters that start after May 2, 2005 and end before July 6, 2007. We use Tobit (Logit) regression when *MF_N* (*MF_D*) is the dependent variable. Please see Table 2 for the classification of good news (bad news) management forecasts and the Appendix for variable definitions. The p-values are two-sided and are based on standard errors adjusted for firm-level clustering.

	Good News Forecasts				**Bad News Forecasts**			
	MF_N		*MF_D*		*MF_N*		*MF_D*	
	Coef.	P-value	Coef.	P-value	Coef.	P-value	Coef.	P-value
Panel A: Controlling for contemporaneous performance measures								
Intercept	-2.148	0.001	-1.629	0.001	-1.850	0.001	-1.315	0.001
PILOT	-0.146	0.115	-0.116	0.131	0.030	0.744	0.024	0.749
POST	-0.043	0.432	-0.003	0.945	0.094	0.069	0.078	0.066
PILOT* × *POST	**0.227**	**0.010**	**0.174**	**0.016**	**0.055**	**0.496**	**0.044**	**0.504**
Control variables	Yes		Yes		Yes		Yes	
N	34,718		34,718		34,718		34,718	
Adjusted R^2	4.89%		6.60%		4.15%		5.90%	
Panel B: Using seasonal random walk model to classify good news and bad news								
Intercept	-1.657	0.001	-1.107	0.001	-3.585	0.001	-2.263	0.001
PILOT	-0.106	0.307	-0.056	0.465	0.072	0.583	0.043	0.655
POST	0.086	0.149	0.105	0.016	0.081	0.336	0.055	0.388
PILOT* × *POST	**0.228**	**0.013**	**0.146**	**0.033**	**-0.055**	**0.707**	**-0.025**	**0.822**
Control variables	Yes		Yes		Yes		Yes	
N	34,718		34,718		34,718		34,718	
Adjusted R^2	4.36%		7.71%		2.94%		2.55%	
Panel C: Controlling for contemporaneous investment and financing activities								
Intercept	-2.126	0.001	-1.561	0.001	-2.227	0.001	-1.557	0.001
PILOT	-0.147	0.116	-0.118	0.124	0.024	0.792	0.019	0.797
POST	-0.047	0.394	-0.007	0.880	0.115	0.026	0.094	0.025
PILOT* × *POST	**0.223**	**0.012**	**0.172**	**0.017**	**0.054**	**0.510**	**0.045**	**0.491**
Control variables	Yes		Yes		Yes		Yes	
N	34,718		34,718		34,718		34,718	
Adjusted R^2	4.00%		5.41%		3.55%		4.99%	