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Short Sellers and Long-Run Management Forecasts*

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Abstract

We examine how short sellers affect long-run management forecasts using a natural experiment (Regulation SHO) that relaxes short-selling constraints on a group of randomly selected firms (referred to as pilot firms). We find that compared to other firms, the pilot firms issue more long-run good news forecasts but do not change the frequency of long-run bad news forecasts. The increase in good news forecasts is greater when the pilot firms have higher quality forecasts, greater uncertainty about firm value, or higher manager equity incentives. Overall, these results and the results of additional analyses indicate that the reduction in short-selling constraints and the increase in short-selling threat induce managers to enhance disclosures through more long-run good news forecasts to discourage short sellers.

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1. Introduction

We examine how short sellers affect the issuance of long-run management forecasts, defined as management forecasts with horizons over 90 days. Short sellers are becoming increasingly important in capital markets. For example, short sales account for more than 20% of the trading volume in the 2000−2004 period (Boehmer et al. 2013). As shown in Figure 1, short interest almost doubled from the 1990s to the 2000s.[1] Short sellers play an important role in information discovery, particularly in incorporating bad news into stock prices (e.g., Boehmer and Wu 2013). However, despite the importance and prevalence of short-selling, there is limited research on how short sellers affect disclosures. Li and Zhang (2015) find that in response to short-selling threat, managers reduce the precision of short-run bad news forecasts and the readability of bad news annual reports, to mitigate the negative price impact of bad news. What remains unclear is whether managers proactively change disclosure frequency to discourage short sellers.

[Insert Figure 1 here]

Short sellers are unwelcome not only because of the immediate downward pressure of short-selling on stock prices and managers' compensation, but also because of short-selling's potential adverse impact on investors' and other stakeholders' confidence in the firm and the

[1] Note that short interest is inherently a small proportion of the outstanding shares. Beneish et al. (2015) find that for stocks that are more difficult to borrow, less than 10 percent of the outstanding shares are lendable; even for stocks that are easier to borrow, less than 20 percent of the outstanding shares are lendable.

ensuing damaging effect on the firm's financing and operating activities (e.g., Khanna and Mathews 2012). Given that corporate disclosure is one of the most direct ways of influencing market perceptions, we study whether managers strategically enhance long-run forecasts to discourage short sellers.

We focus on long-run management forecasts for three reasons. First, managers have more discretion in issuing long-run forecasts compared to short-run forecasts. Prior research suggests that the issuance of short-run forecasts is mainly driven by period-specific performance and litigation concerns (e.g., Skinner 1994; Miller 2002). In contrast, due to the inherent difficulty of projecting long-run performance, managers have more latitude in deciding whether to issue long-run forecasts. As such, managers generally issue long-run forecasts to reduce information asymmetry and for strategic reasons (e.g., Rogers and Stocken 2005; Chen et al. 2008). Second, compared to short-run performance, long-run performance is associated with greater uncertainty and depends on investment and operating decisions known only to managers themselves (e.g., Gong et al. 2011). Thus, managers likely have information advantages over outsiders regarding long-run performance. Third, short sellers usually hold their positions for a short period, because of the cost of borrowing and the risk of unfavorable price movement (e.g., Diether 2008). After learning about short-run forecasts, short sellers who are not convinced may choose to short and hold the position until the actual performance is revealed. However, it is more difficult to do so after long-run forecasts, because holding the position for a long time is costly and risky for short sellers. In sum, managers can more effectively use long-run management forecasts to discourage short sellers.

To investigate the impact of the short-selling threat on long-run disclosures, we use the natural experiment of Regulation SHO to address potential endogeneity issues concerning short-selling threat and disclosures.[2] On June 23, 2004, the SEC adopted Regulation SHO, a pilot program that temporarily suspended the tick test for a group of randomly selected firms (i.e., the pilot firms).[3] During the pilot program, the short-selling constraints became lower for the pilot firms than for other firms (i.e., the control firms). The combination of this exogenous shock to short-selling constraints and the randomization of the treatment group presents a clean setting to examine the causal effect of short-selling threat on disclosures. Moreover, recent studies find that the pilot program led to a significant increase in short-selling for the pilot firms, consistent with the tick test being a significant constraint on short-selling and the increase in short-selling threat being significant and credible for the pilot firms (e.g., Boehmer et al. 2008; Diether et al. 2009a, 2009b; Grullon et al. 2015). Therefore, the pilot program is an economically significant setting for investigating the influence of short-selling threat on disclosures.

[2] The endogeneity arises because underlying firm performance or other factors can affect both short-selling threat and disclosures. For example, superior performance can deter short sellers and at the same time lead to good news disclosures.
[3] 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. The list of pilot firms was announced on July 28, 2004.

We predict that the pilot firms will increase good news disclosures in response to the increased short-selling threat, for the following reasons.[4] First, after good news disclosures, the share price will go up and short sellers will also revise their belief upward. Assuming that short sellers also have their own signal, how much short sellers revise their belief upward depends on the relative precision of the firm's disclosure versus short sellers' own signal. If the firm's disclosure is of high quality relative to short sellers' own signal, the firm's good news disclosure will dominate in influencing short sellers' beliefs. The difference between the updated share price and short sellers' updated belief will become smaller when the pilot firm discloses good news than when the pilot firm does not disclose. This smaller divergence reduces the potential gain from short-selling and short sellers' incentives to take a position. As discussed earlier, the firm's information advantage over short sellers is likely to be high in the case of long-run management forecasts. Hence, disclosing good news can help pilot firms discourage short sellers. Second, if short sellers expect the pilot firms to disclose good news in a more timely fashion, they will be less willing to take a position for the fear of losing out when they have to close their position. Thus, assuming that the costs of disclosing good news do not change, the pilot firms have stronger incentives to disclose good news than the control firms during the pilot program.[5]

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[4] We develop an analytical model to analyze the effect of Regulation SHO on firm disclosures. See Section 2.3 for discussion of the model.

[5] Issuing forward-looking good news involves costs, including proprietary costs and litigation costs (e.g., Verecchia 2001; Cheng and Lo 2006). Therefore, managers do not always disclosure all the good news they have and the pilot firms should

In contrast, we do not have a directional prediction for bad news disclosures. On the one hand, pilot firms may have incentives to increase bad news disclosures. When short sellers have a negative signal regarding the firm, they may short the firm if the firm does not disclose. Disclosing bad news leads to a drop in the share price and confirms short sellers' belief, reducing the potential gain from short-selling and short sellers' incentives to take a position. On the other hand, a lack of disclosure may be perceived by investors and short sellers as the firm holding bad news. Thus the pilot firms may not disclose bad news to save disclosure costs. Pilot firms may also have incentives to withhold bad news, because they may experience an increase in price sensitivity to bad news disclosure due to the increased threat of short-selling and bear raiders (Grullon et al. 2015; De Angelis et al. 2017). Thus, it is unclear whether the pilot firms will change bad news disclosures during the pilot program.

We test our predictions using 32,302 firm-quarters from 2,182 firms, including 738 pilot firms and 1,444 control firms, over 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 use a difference-in-differences design, by first measuring the change in the issuance of long-run forecasts between the pre and post periods and then comparing the change between the pilot and control firms.

We find that compared with the control firms, the pilot firms increase the frequency of

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have room for enhancing good news disclosures.

long-run good news forecasts from the pre to the post period, but do not experience a significant change in the issuance of long-run bad news forecasts. In addition, we find that the results for long-run good news forecasts are stronger when managers' forecasts are of higher quality, when there is greater uncertainty regarding firm value, and when managers have higher equity incentives and are thus more concerned about stock price drops. We also find that the strategy of increasing good news disclosures is effective in addressing the short-selling threat: pilot firms that issue more long-run good news forecasts during the pilot program are associated with a smaller increase in short interest.

Note that the increase in the pilot firms' good news disclosures is not because they have better performance. We find that the pilot firms have similar performance as the control firms during the pilot program, and that the results hold after controlling for firm performance. We find that there is no significant change in management forecast bias for the pilot firms. The results are also robust to using alternative approaches to classify forecasts. Therefore, the findings are consistent with the notion that managers bring good news forward by issuing more long-run good news forecasts. We further confirm this using the methodology developed in Roychowdhury and Sletten (2012). We find that when firms have good news, a greater proportion of the news is revealed *before* earnings announcements for the pilot firms during the pilot program than for the control firms.

We conduct several additional analyses to enrich the results. First, in July 2007 the SEC permanently removed the tick test for all stocks. As a result, after July 2007 the control firms experienced a shock to the short-selling constraints, while the pilot firms experienced no change. We find that the control firms are associated with a significant increase in long-run good news disclosures after the permanent removal of the tick test, similar to the pilot firms during the pilot program. Second, we find that the results are stronger for the pilot firms that had already provided forecasts in the pre period, likely because the cost of initiating forecasts is higher than that of increasing forecast frequency (Balakrishnan et al. 2014). Third, consistent with the notion that having more good news forecasts increases information environment quality, we find that during the pilot program, analyst forecast dispersion and error become lower for pilot firms that increase long-run good news forecast frequency.

Our paper contributes to the literature in the following ways. First, prior studies find that firms change disclosures in response to various market participants' demand for information. Our study complements those studies by examining the influence of short sellers on long-run forecasts, given managers' desire to discourage short sellers. Unlike other market participants such as institutional investors and financial analysts, who generally have a symmetric effect on good news and bad news disclosures, short sellers mainly affect the timeliness of good news disclosures.

Our study differs from and complements Li and Zhang (2015) in two important respects. First, the two papers examine different disclosure strategies and present distinct findings. Li and Zhang focus on how managers change disclosure precision to reduce the negative impact of bad news in the presence of short-selling threat. They find that managers obfuscate *short-run* bad news forecasts and annual reports. In contrast, we investigate how managers change disclosure frequency to discourage short sellers and find that managers increase *long-run* good news disclosures. Thus, the strategies examined in the two studies—changing the frequency of long-run forecasts and changing the precision of short-run forecasts—complement each other.[6] Second, the results of the two studies have different implications for information environment quality. While Li and Zhang's findings suggest that short sellers may indirectly lead to the worsening of the information environment, we find that managers' strategic disclosure responses to short-selling threat can improve information environment quality. Together, the two studies provide a more comprehensive understanding of the impact of short-selling threat on corporate disclosures and indicate that managers exploit multiple disclosure strategies in response to short-selling threat.

A concurrent study, Clinch et al. (2019), also uses the Regulation SHO setting and finds an increase in short-run bad news forecasts for the pilot firms during the pilot program. At

[6] We conduct additional tests to further confirm that the two studies present distinct findings. First, using the same research design as in Li and Zhang, we find that managers do not reduce the precision of long-run forecasts, suggesting that managers mainly obfuscate short-run bad news forecasts. Second, similar to Li and Zhang, we find that the frequency of short-run good or bad news forecasts does not change significantly for the pilot firms. This suggests that when bringing good news forward, the pilot firms primarily resort to long-run forecasts, where managers have more discretion and information advantage. These tests are not tabulated for brevity and are available upon request.

first glance, our results appear to contradict those of Clinch et al. (2019). However, detailed reconciliations indicate that the differences in results are mainly driven by different focuses—long-run forecasts in our study versus short-run forecasts in Clinch et al. (2019). The differences in control variables and the source of management forecast data (I/B/E/S versus First Call) do not affect the inferences.[7]

Second, this study enhances our understanding of short sellers' role in capital markets. We find that short sellers not only help incorporate bad news in stock prices, as documented in prior research (e.g., Boehmer and Wu 2013), but also indirectly help bring good news forward. While the former mechanism operates through short sellers' information acquisition and trading, the latter occurs due to managers' incentives to discourage short sellers.

Third, our findings enrich the evidence concerning the actions taken by firms to discourage short sellers. Lamont (2012) focuses on legal actions against short sellers. Laksanabunsong and Wu (2014) examine stock repurchases. Unlike these studies, which focus on firms' response to actual short sales, we focus on how firms respond to increased short-selling threat and investigate the use of corporate disclosures, one of the most direct ways of influencing market perceptions.

The remainder of the paper is organized as follows. Section 2 discusses the institutional

[7] In addition, we find that the results for short-run forecasts are sensitive to the definition of the pre period; they hold for some choices of the pre period (e.g., starting after December 1, 2002) but become insignificant if the pre period starts earlier. The results of the reconciliation tests are available upon request.

background, reviews the literature, and develops the hypotheses. Section 3 discusses the sample. Section 4 presents the main empirical results, and Section 5 discusses the additional analyses. Section 6 concludes the paper.

2. Institutional background, prior research, and hypothesis development

2.1 Institutional background on the pilot program

In 1938, the SEC adopted Rule 10a-1 to restrict short-selling by not permitting short sales on minus or zero-minus ticks.[8] In 1994, Nasdaq adopted a bid price test (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, impose constraints on short-selling. Prior studies find that stocks became 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), which includes a pilot program temporarily suspending the tick test for a group of randomly selected companies to assess the effectiveness and necessity of short-selling restrictions. On July 28, 2004, about 1,000 U.S. stocks were selected as pilot firms. The pilot program started on May 2, 2005, and ended on July 6, 2007, when the SEC permanently suspended the tick test for all

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

publicly traded U.S. stocks.[9]

During the pilot program, the pilot firms experienced a decrease in short-selling constraints and an increase in short-selling threat compared to the control firms. For example, the SEC's Office of Economic Analysis (OEA 2007) and Diether et al. (2009a) find that around the time of the implementation of the pilot program, short-selling volume in the pilot firms increased by 2% of total trading volume, or an 8% increase relative to the average short-selling volume before the pilot program. Consistent with the importance of REG SHO, recent studies find that the pilot program significantly affected pilot firms' corporate decisions, such as executive compensation (De Angelis et al. 2017), equity issuance and investments (Grullon et al. 2015), earnings management (Fang et al. 2016), and disclosure precision (Li and Zhang 2015). Thus, REG SHO is a powerful setting to examine how short-selling threat affects long-run management forecast issuance.

2.2 Prior research on short-selling

Prior research finds that short sellers are informed traders and contribute to efficient stock prices. For example, prior studies find that abnormal short interest is associated with negative future stock returns (e.g., Dechow et al. 2001; Jones and Lamont 2002; Pownall and Simko 2005; Boehmer et al. 2008; Boehmer and Wu 2013; Kecskes et al. 2013). Short sellers' trading profits may come from private information acquisition, skilled processing of

[9] In response to the criticism of the suspension of the tick test, on February 24, 2010, the SEC reinstated a revised tick test, which is applicable when a security's price drops by 10% or more from the last day's closing price.

public information, or front-running (e.g., Christophe et al. 2010; Drake et al. 2011; Engelberg et al. 2012; Khan and Lu 2013; Christensen et al. 2014).

At the same time, short sellers are viewed with considerable skepticism. First, through taking and covering short positions, short sellers can increase market volatility, potentially leading to higher perceived risk (Savor and Gamboa-Cavazos 2011; Hong et al. 2012). Second, manipulative short sellers can target a firm, encourage others to sell by spreading rumors about the prospects of the firm, and then cover their positions at a profit. Such behavior leads to a disorderly market. Third, short-selling can also be harmful to a firm because of its feedback effect on the firm's real decisions (e.g., Goldstein and Guembel 2008). Existing and potential stakeholders can regard short positions as a signal of poor future prospects, lose confidence in the firm, and stop dealing with it. This makes it difficult for the firm to attract investors and customers, and ultimately leads to a deterioration in performance. As Khanna and Mathews (2012) argue, 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 (page 229)."

2.3 Hypothesis development

As discussed above, short sellers can adversely affect managers and firms by increasing stock price volatility, depressing stock prices, and negatively influencing the firm's long-run performance. Not surprisingly, managers typically view short sellers unfavorably.[10] When facing increased short-selling threat, managers have incentives to deter short sellers from taking a position, because managers' welfare, such as compensation, job security, and reputation, is positively related to stock prices.

We develop a stylized theoretical model to analyze the effect of REG SHO on firm disclosures. The key prediction of the model (Proposition 1) is that under a given set of conditions, in response to an increase in short-selling threat, managers increase good news disclosures but do not change bad news disclosures, to discourage short sellers from taking a short position. We would like to emphasize that given the various factors affecting managers' disclosure decision, the model specifies the necessary, not sufficient, conditions for managers to increase good news disclosures in the post-REG SHO period. To save space, in this section we only discuss the key arguments and intuition of the model; we present the full model and detailed discussion in the online Appendix.[11]

Good news disclosures

Short sellers' shorting decision depends on whether the potential gain from short-selling is greater than the shorting cost, with the potential gain equal to the difference between the share price and short sellers' belief of firm value. Compared to the pre period, in the post period short-selling constraints are relaxed and shorting cost is lower for the pilot firm. *All*

[10] For example, short sellers are believed by some managers to be "evil and damaging to the firm" (Jensen 2005, page 16).
[11] Please see "A Model of Regulation SHO and Firms' Disclosure," as an addition to the online article.

else being equal, short sellers are more likely to take a short position, leading to higher short-selling threat in the post period. In particular, if short sellers privately observe a negative signal,[12] they are more likely to short the pilot firm's shares in the post period compared to the pre period, assuming that the pilot firm does not disclose.

Now assume that the pilot firm observes good news and discloses it. Shareholders update their belief and the stock price goes up as a result. At the same time, short sellers also update their belief upward. Short sellers' potential gain from shorting is the difference between the updated share price and short sellers' updated belief. Whether good news disclosure can deter short sellers depends on whether the potential gain from short-selling is lower when the firm discloses good news compared to when the firm does not disclose. Because short sellers' posterior belief after good news disclosure is formed based on both the firm's disclosure and short sellers' own signal, the relative importance of each signal increases in its precision. When the firm's disclosure is more precise relative to short sellers' own signal, the firm's disclosure dominates in influencing short sellers' belief. As such, the divergence between the updated share price and short sellers' updated belief becomes smaller when the firm discloses good news than when the firm does not disclose. This smaller divergence reduces the potential gain from shorting and short sellers' incentives to short the firm. In contrast, when short sellers' own signal is more precise, the firm's disclosure has

[12] Short sellers will consider taking a short position *only* when they observe a negative signal. Thus, our discussion focuses on the case when short sellers observe a negative signal.

little effect on short sellers' belief, leading to a larger divergence between the updated share price and short sellers' updated belief. In this case, short sellers are more incentivized to take a short position.

In sum, whether good news disclosure can deter short sellers from taking a short position in the post period depends on the relative precision of the firm's disclosure and short sellers' own signal. When the firm's signal is of high quality relative to short sellers' signal, short sellers are less likely to take a short position when the firm discloses good news compared to when the firm does not disclose. As discussed in the introduction, managers' information advantage over short sellers is likely high in the case of long-run management forecasts. We thus argue that the disclosure of long-run good news forecasts can deter short sellers from taking a short position. Accordingly, we predict that during the pilot program, the pilot firms will have stronger incentives to disclose good news.

In addition, prior research finds that short sellers usually hold their position for a short period of time, and sometimes are forced to close their position at a loss after good news disclosures (e.g., Shleifer and Vishny 1997; Savor and Gamboa-Cavazos 2011; Hong et al. 2012).[13] As such, if short sellers expect the pilot firms to disclose good news in a more timely

[13] Shleifer and Vishny (1997) argue that when the stock price increases, short sellers will lose money and likely face redemption by the clients. Savor and Gamboa-Cavazos (2011) argue that because short sellers' compensation is linked to their investment performance, they are subject to myopic loss aversion. Hong et al. (2012) conjecture that leverage constraints, risk management, and agency problems arising from delegated money management also drive short sellers to reduce their positions after suffering losses. Savor and Gamboa-Cavazos (2011) provide evidence consistent with these arguments.

fashion, they will be less willing to take a position in the first place for fear of losing out when they have to close their position. These arguments reinforce managers' incentives to disclose good news to deter short sellers.[14]

One might argue that the pilot firms will not be able to increase good news disclosures if managers always disclose all the forward-looking good news they possess. However, it is well-established in the disclosure literature that disclosing forward-looking good news involves costs, such as information processing costs, proprietary costs, and litigation costs when forward-looking good news proves wrong ex post. All these costs prevent managers from disclosing all the good news in their possession (e.g., Verrecchia 2001; Cheng and Lo 2006). Therefore, the pilot firms will have room to enhance good news disclosures.

Bad news disclosures

The impact of increased short-selling threat on bad news disclosure is less clear. As discussed earlier, if short sellers privately observe a negative signal about the pilot firm, they are more likely to short in the post period compared to the pre period, when the pilot firm does not disclose. This motivates the pilot firm to disclose bad news. When the firm does so, the share price drops. Because the disclosure also confirms short sellers' own signal, the divergence between the updated share price and short sellers' updated belief becomes smaller compared to when the firm does not disclose, reducing short sellers' incentives to take a

[14] Ng et al. (2013) find that stock prices continue to drift upward after good news management forecasts. This empirical pattern further reduces short sellers' incentives to take a short position after good news forecasts.

position. This argument suggests that the pilot firms have stronger incentives to disclose bad news during the pilot program.[15]

However, at the same time, the pilot firms might have incentives not to disclose bad news during the pilot program. First, as discussed earlier, we predict that the pilot firms have stronger incentives to disclose good news during the pilot program. Thus, a lack of disclosure will be perceived by investors and short sellers as the firm having bad news. As such, the pilot firms may not disclose bad news to save disclosure cost. Second, De Angelis et al. (2017) argue that the pilot firms may experience an increase in price sensitivity to bad news disclosures during the pilot program because of the increased threat of short-selling and bear raiders. Grullon et al. (2015) find evidence consistent with this prediction for financially constrained pilot firms. As a result, the pilot firms have incentives to reduce bad news disclosures.

The foregoing discussion suggests that the pilot firms will increase good news disclosures during the pilot program, but have conflicting incentives for bad news disclosures. Thus, our hypothesis is directional for good news disclosures and non-directional for bad news disclosures:

HYPOTHESIS 1. *Ceteris paribus, the pilot firms increase good news disclosures compared to the control firms during the pilot program.*

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[15] These arguments are consistent with prior evidence that withholding bad news can lead to short-selling. For example, Christensen et al. (2014) find that because pro-forma disclosures can disguise bad news, short sellers are more likely to short firms with pro-forma disclosures.

HYPOTHESIS 2. *Ceteris paribus, the pilot firms do not change bad news disclosures compared to the control firms during the pilot program.*

As with any theoretical model, the predictions hold when a number of conditions are satisfied, with the key conditions related to the quality of short sellers' private information, firm's disclosure cost, and short-selling cost. The online Appendix's detailed analyses reveal that the conditions are more likely to be satisfied when the quality of the firm's disclosure is higher and when there is more uncertainty about the firm's fundamental value (Corollary 1). Later, we conduct cross-sectional analyses to test Corollary 1 and substantiate our arguments.

3. Sample

3.1 Sample selection

Panel A of Table 1 describes the sample selection process. We start with the Russell 3000 index firms in 2004, the set of firms from which the SEC selected the pilot firms. Following Diether et al. (2009a), we require that firms also be included in the Russell 3000 index in 2005. Following the SEC's selection criteria for the pilot firms, we exclude stocks that were not listed on NYSE, AMEX, or Nasdaq, and stocks that went public after April 30, 2004. We also exclude firms that changed tickers during the pilot program and firms that have missing financial, stock price, or analyst data in the pre or post period.

[Insert Table 1 here]

We use the difference-in-differences design to test our hypotheses, and Figure 2 presents the timeline. The pre period includes the fiscal quarters that start on or after January 1, 2002, and end before July 28, 2004, when the SEC announced the list of pilot firms. The post period covers the pilot program, including the fiscal quarters that start after May 2, 2005, when the pilot program started, and end before July 6, 2007, when the program ended. We eliminate the transition period between July 28, 2004, and May 2, 2005 to avoid potential confounding effects. To ensure a balanced sample, we require that firms have the same number of quarters in the pre and post periods.[16] The results are qualitatively similar if we do not impose this requirement. Our final sample consists of 32,302 firm-quarters from 2,182 firms, including 738 pilot firms and 1,444 control firms.[17]

[Insert Figure 2 here]

3.2 Descriptive statistics

Panel B of Table 1 presents the descriptive statistics separately for the pilot and control firms. The statistics are for fiscal year 2003, the year before the SEC selected the pilot firms. We report the statistics on firm size, market-to-book ratio, leverage, return on equity, trading volume, and analyst following. As reported, 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.

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[16] For firms with more quarters in the pre period than in the post period, we drop the earliest quarters in the pre period. However, if a firm has fewer quarters in the pre period than in the post period, we exclude the firm from the sample.

[17] Of the 2,182 firms, 78.9% have eight quarters, the maximum possible number of quarters, in both periods, 10.1% have seven quarters, 2.0% have six quarters, 2.2% have five quarters, and 6.8% have four or fewer quarters in both periods.

3.3 Management forecasts

We obtain data on management forecasts from First Call.[18] As discussed in the introduction, we focus on long-run forecasts—those with horizons greater than 90 days. To test Hypothesis 1 and Hypothesis 2, we compare the change in quarterly issuance of management forecasts from the pre to post period between the pilot and control firms, separately for good news and bad news forecasts. A forecast is classified as good (bad) news if the point forecast or the mid-point of the forecast range is higher (lower) than the consensus analyst forecast in the previous 90 days. For open-ended forecasts, we classify a forecast as good (bad) news when its lower (upper) bound is higher (lower) than the consensus analyst forecast. For qualitative forecasts, we follow Anilowski et al. (2007) and classify a 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." The other forecasts are regarded as neutral news and not included in the analyses.

4. Main analyses

[18] Chuk et al. (2013) find that First Call does not include all management forecasts. However, this omission is unlikely to bias our analyses. First, our tests are based on a comparison between the randomly selected pilot firms and control firms. The omission should have a similar impact on these two groups of firms. Second, Chuk et al. find that the First Call coverage is less comprehensive prior to 1997, while our sample starts in 2002. Third, Chuk et al. find that the omission is more severe for small firms than for large firms and is more severe for qualitative forecasts. When we separately analyze small and large firms, the inferences hold for both groups. We also obtain the same inferences when we exclude qualitative forecasts.

4.1 *Main tests—Tests of Hypothesis 1 and Hypothesis 2*

We use the following regression to test Hypothesis 1 and Hypothesis 2:

$$MF\_N = \alpha_0 + \alpha_1 POST + \alpha_2 PILOT \times POST + \beta Control\,Variables + Firm\,Fixed\ Effects + \varepsilon \tag{1}$$

Firm and quarter subscripts are omitted for simplicity. *MF_N*, the frequency of long-run management forecasts, is measured as the number of long-run forecasts issued in the quarter, and is set as zero for firm-quarters without management forecasts.[19] We estimate equation (1) separately for good news and bad news forecasts, and construct *MF_N* accordingly.[20] We include firm fixed effects to control for the effect of time-invariant firm characteristics (such as litigation risk) on forecast issuance.

PILOT is an indicator variable for the pilot firms; it equals one for the pilot firms and zero for the control firms. *POST* is an indicator variable for the post period; it equals one for firm-quarters in the post period and zero for those in the pre period. The main variable of interest is the interaction of *PILOT* and *POST*. A positive (negative) coefficient on this interaction indicates that the pilot firms experience an increase (a decrease) in management forecast frequency during the pilot program, compared to the control firms. Note that we do not include *PILOT* in the regression because it is subsumed by firm fixed effects.

The selection of control variables is based on prior research. First, prior research finds

[19] Using the likelihood of management forecasts leads to the same inferences.
[20] We use OLS regression to estimate equation (1). We obtain the same inferences when we use Poisson regressions.

that managers are more likely to disclose when the demand for information is higher (e.g., Ajinkya et al. 2005; Jo and Kim 2007). We use analyst coverage, institutional ownership, firm size, and growth opportunities (market-to-book ratio) to capture the demand for information. Second, when the operating environment is uncertain, managers might be reluctant to issue forecasts because they might turn out to be incorrect, increasing litigation risk. We use earnings volatility and return volatility to capture 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). Last, we include an indicator variable for analyst optimism, because managers have incentives to issue forecasts to guide market expectations downward when analysts are optimistic (Richardson et al. 2004). The Appendix describes the variable measurements in detail.

Panel A of Table 2 presents the descriptive statistics for the regression variables. For the sample firms, the average number of long-run good news (bad news) forecasts per quarter is 0.239 (0.260). The average number of analysts following the firms is 10; the average institutional ownership is 63.5%; the average firm size (total assets) is $7,215 million; the average market-to-book ratio is 2.825; the average earnings volatility is 0.228; the average return volatility is 2.3%; and the average stock return in the past year is 7.7%. About 31.0% of the quarters have optimistic analyst forecasts as of the beginning of the quarter. Panel B of Table 2 reports the correlations among the independent variables. The correlations are usually small, except for the correlation between analyst following and firm size (0.38).

[Insert Table 2 here]

Table 3 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. Consistent with Hypothesis 1, the coefficient on *PILOT* × *POST* is significantly positive for good news forecasts ($p = 0.015$). This indicates that compared to the control firms, the pilot firms experience a significant increase in the frequency of long-run good news forecasts during the pilot program. The effect is economically significant; the pilot firms issue 0.043 more long-run good news forecasts in one quarter than the control firms. This represents a relative increase of 18% (= 0.043/0.239; 0.239 is the average long-run good news forecast frequency in the pre period).[21]

[Insert Table 3 here]

In contrast, the coefficient on the interaction term *PILOT* × *POST* is insignificant for bad news forecasts ($p = 0.697$), indicating that the pilot firms do not differ from the control firms in the change in the frequency of long-run bad news forecasts.

In sum, we find that compared to the control firms, the pilot firms increase long-run good news forecasts during the pilot program. In contrast, we do not find a significant change

[21] This magnitude is comparable to what is reported in prior research that examines the effect of capital market intermediaries on management forecasts. For example, Balakrishnan et al. (2014) find that upon an exogenous shock of analyst coverage loss, management forecast frequency is associated with a relative increase of 17.8%.

in the issuance of long-run bad news forecasts for the pilot firms relative to the control firms. These results are consistent with the pilot firms increasing the issuance of long-run good news forecasts when the short-selling threat increases.

4.2 Cross-sectional analyses for good news forecasts

In this section, we conduct cross-sectional analyses to substantiate our arguments and reinforce the main inferences. As analyzed in detail in the online Appendix, the proposition that the pilot firms increase good news disclosures but do not change bad news disclosures is more likely to hold when the firm's disclosure quality is higher and when uncertainty about firm value is greater. Intuitively, when the firm's disclosure is of higher quality, short sellers' beliefs are more likely to be influenced by the firm's disclosure. Similarly, when uncertainty about the firm's fundamental value is greater, the firm's disclosure is more important in affecting short sellers' beliefs about the firm's value. Hence we expect the main results to be more pronounced when the quality of the firm's disclosure is higher and when there is greater uncertainty about firm value. The cross-sectional analyses focus on good news forecasts. Additional analyses (untabulated) indicate that similar analyses for bad news forecasts do not yield significant results.

Quality of firms' disclosure

We first conduct cross-sectional tests conditional on the quality of firms' disclosure. Given that we focus on management forecasts, we use the accuracy of past management forecasts, *MF_PastAcc*, to proxy for the quality of disclosure. *MF_PastAcc* is calculated as the average accuracy of long-run management forecasts issued in the three years before the current quarter. We then add this variable and its interaction with *POST*, *PILOT*, and *PILOT* × *POST* to regression (1). *MF_PastAcc* is demeaned so that the coefficient on *PILOT* × *POST* captures the effect of REG SHO for an average pilot firm.

Table 4 reports the regression results. The coefficient on *PILOT* × *POST* remains significantly positive. Moreover, the coefficient on *PILOT* × *POST* × *MF_PastAcc* is significantly positive (p = 0.008), suggesting that as expected, the results are more pronounced when management forecasts are of higher quality.

[Insert Table 4 here]

Uncertainty about firm value

As discussed earlier, the model's prediction is more likely to hold when there is greater uncertainty about the firm's fundamental value.[22] We use three proxies for uncertainty about firm value: magnitude of accruals (|*Accruals*|), earnings volatility (*Earn_Volatility*), and growth opportunities (*Growth*, measured as market-to-book ratio).[23] Prior studies suggest that firms with higher uncertainty have a larger amount of accruals and more volatile

[22] Another reason why managers have stronger incentives to deter short sellers when there is greater uncertainty is that managers are concerned that investors and other stakeholders are more likely to interpret a short position as a signal of poor future prospects and lose confidence in the firm when uncertainty about firm value is greater.
[23] Note that these three variables may also capture managers' information advantage over investors and short sellers. When there is greater uncertainty about firm value, it is likely that managers' information will be more precise compared to short sellers and investors. We have the same prediction if these variables reflect managers' information advantage.

earnings, and it is more difficult to predict future earnings for these firms (e.g., Gleason et al. 2008). Prior research (e.g., Baker and Wurgler 2006) also suggests that growth stocks have higher uncertainty than other stocks.

Panels A, B, and C of Table 5 report the cross-sectional tests based on the three proxies, which are demeaned for ease of interpretation. In all three panels, the coefficient on *PILOT* × *POST* continues to be significantly positive. More importantly, as predicted, the coefficients on *PILOT* × *POST* × |*Accruals*|, *PILOT* × *POST* × *Earn_Volatility*, and *PILOT* × *POST* × *Growth* are significantly positive (p = 0.020, 0.035, and 0.009, respectively).

[Insert Table 5 here]

Managers' equity incentives

Last, we conduct cross-sectional tests related to managers' equity incentives. A key reason for managers wanting to deter short sellers is that managers' wealth is positively linked to stock prices, and managers are concerned about potential stock price drops (Jensen 2005). It thus follows that the results should be stronger for managers whose wealth is more sensitive to stock price changes than for other managers. We measure *Equity_Incentives* as the change in the value of top executives' stock and option holdings with a 1% increase in stock price. For ease of interpretation, *Equity_ Incentives* is demeaned. Table 6 reports the results. The coefficient on *PILOT* × *POST* continues to be positive. More importantly, the coefficient on *PILOT* × *POST* × *Equity_ Incentives* is significantly positive (p = 0.059). This suggests that as predicted, pilot firms' incentives to disclose good news are stronger when their managers' wealth is more sensitive to stock price changes.

[Insert Table 6 here]

Overall, the cross-sectional tests indicate that the results for good news forecasts, as documented in Section 4.1, are more pronounced when the firms' disclosure quality is higher, when there is greater uncertainty about firm value, and when managers' wealth is more sensitive to changes in stock prices.

4.3 Are good news forecasts effective in discouraging short sellers?

To strengthen the arguments for the main tests, we examine whether the pilot firms' strategy of increasing long-run good news disclosures is effective in addressing the short-selling threat. For this test, we focus on pilot firms that experience an increase in short interest shortly after the implementation of the pilot program (namely, in the first two quarters of the post period) compared to the pre period. For this group of pilot firms, the increase in short-selling threat is more significant, and finding a way to discourage short sellers is especially important.

We examine whether the change in short interest from the first two quarters in the post period to the rest of the post period is negatively associated with the issuance of long-run good news forecasts. Specifically, we regress this change in short interest on the number of long-run good news forecasts in the post period (excluding the first two quarters). For completeness, we also include the number of long-run bad news forecasts over the same period. Following prior research on the determinants of short interest (e.g., Christophe et al. 2004), we control for changes in the following variables: firm size, market-to-book ratio, return on equity, size-adjusted returns in the prior year, and analyst forecast error. These variables are measured as quarterly averages over the respective period before we calculate the changes. We also control for earnings news by including the change in the proportion of quarters when earnings meet or beat consensus analyst forecast.

Table 7 reports the results. We find that among the pilot firms that experience an increase in short interest from the pre period to the first two quarters of the post period, those issuing more long-run good news forecasts are associated with a smaller increase in short interest in the remainder of the pilot program ($p = 0.027$). In contrast, we do not find that issuing long-run bad news forecasts is associated with a significant change in short interest. Therefore, the strategy of increasing long-run good news disclosures appears to be effective in discouraging short sellers.[24]

[Insert Table 7 here]

4.4 Alternative explanations and sensitivity analyses

Do the pilot firms have more good news to disclose?

An alternative explanation for the main results is that the pilot firms have more good

[24] One caveat is that this analysis may be confounded by omitted correlated variables.

news to disclose during the pilot program. To address this concern, we directly compare stock and accounting performance during the pilot program, and the change in performance from the pre to the post period, between the pilot and control firms. However, we do not find any significant differences between the pilot and control firms; the two-sided p-values range from 0.237 to 0.916 for a wide range of performance measures (untabulated). In a sensitivity test, we further control for contemporaneous stock and accounting performance and obtain similar results (untabulated).

Do the pilot firms become more optimistically biased during the pilot program?

Another alternative explanation is that the pilot firms issue more optimistic forecasts to mislead investors. This, however, is not plausible because issuing optimistically biased forecasts, if suspected, can attract the interest of short sellers. Indeed, Christensen et al. (2014) find that this is the case for optimistic non-GAAP reporting. Nonetheless, to address this alternative explanation, we examine the change in management forecast bias during the pilot program. Untabulated analyses indicate that the pilot firms do not issue more optimistically biased forecasts during the pilot program compared to the control firms.

Controlling for financing and investment

Prior research (e.g., Grullon et al. 2015) finds that some pilot firms experience a reduction in investment and financing during the pilot program compared to the control firms. To ensure that the differences in investment and financing do not affect our results, we explicitly control for contemporaneous equity financing and capital expenditures and obtain similar results (untabulated).

Alternative approaches of classifying good news versus bad news management forecasts

It is possible that the consensus analyst forecast that is used to classify good and bad news management forecasts reflects negative sentiment from the increased short-selling threat, and is thus more pessimistic for pilot firms than for control firms. To address this concern, we use the seasonal random walk model to estimate the market's earnings expectation. The results (untabulated) are similar to those in Table 3.

In addition, in the main analyses, we compare the midpoint of range forecasts with analyst forecasts to classify good versus bad news forecasts, as is commonly done in prior research. However, Ciconte et al. (2014) suggest that due to managers' asymmetric loss functions regarding earnings surprises, they are more likely to place their earnings estimate as the upper bound (rather than the midpoint) of range forecasts. Following their suggestion, we use the upper bound of range forecasts as the proxy for managers' earnings estimate and obtain similar results (untabulated).

Last, Rogers and Van Buskirk (2013) find that management forecasts are often bundled with earnings announcements and this can lead to noise in the classification of good versus bad news forecasts. To ensure the robustness of our results, we follow the method proposed in Rogers and Van Buskirk; for bundled forecasts, we estimate a revised (unobservable) analyst expectation after the earnings announcement and then use this revised analyst expectation to classify forecasts as good or bad news. The results (untabulated) are similar to those in Table 3.

5. Additional analyses

5.1 Evidence of the pilot firms bringing good news forward

Our findings are consistent with the pilot firms bringing good news forward from future earnings announcements by issuing more long-run good news forecasts. The pilot firms' incentives for bringing good news forward should also apply to other communications with investors. In this section, we validate this conjecture using the methodology developed in Roychowdhury and Sletten (2012). Specifically, we run the following regression:

$$\begin{aligned} News\_Ratio = {} & \alpha_0 + \alpha_1 POST + \alpha_2 PILOT \times POST + \alpha_3 Good\_News + \alpha_4 PILOT \times Good\_News \\ & + \alpha_5 POST \times Good\_News + \alpha_6 PILOT \times POST \times Good\_News \\ & + \beta\, Control\, Variables + Firm\, Fixed\, Effects + \varepsilon \end{aligned}$$

News_Ratio is defined as the ratio of the absolute value of size-adjusted returns around earnings announcements (i.e., the [-1, +1] window) to the absolute value of cumulative size-adjusted returns over the current and preceding quarters inclusive of earnings announcements. *Good_News* is an indicator that equals one if the cumulative size-adjusted returns over the current and preceding quarters are positive. Thus, *Good_News* captures whether the firm overall has good news, and *News_Ratio* captures the proportion of news that is revealed through earnings announcements. If the pilot firms are more likely to bring good news forward during the pilot program, we expect a significantly negative coefficient on *PILOT* × *POST* × *Good_News*. Untabulated analyses support this prediction; the coefficient on *PILOT* × *POST* × *Good_News* is significantly negative (p = 0.019). This result validates our inferences based on management forecasts.

5.2 Removal of the tick test for all U.S. publicly listed firms on July 6, 2007

Upon the conclusion of the pilot program on July 6, 2007, the SEC removed the tick test for all U.S. exchange traded stocks. Conceptually, this is another event that can be used to test the impact of short-selling threats on disclosures. While the short-selling constraints faced by the pilot firms remained the same, the control firms now faced reduced short-selling constraints. Thus, we expect that after the removal of the tick test, the control firms would experience similar changes in disclosures as the pilot firms during the pilot program. However, this event is not as clean as the pilot program, because the SEC introduced additional rules about short-selling,[25] and the period after the removal of the tick test largely coincides with the financial crisis, potentially confounding the tests.[26]

Nevertheless, we analyze the change in long-run management forecast frequency for the control firms after this event for additional evidence. To reduce the confounding effect of

[25] For example, in July 2008 the SEC started to require that short sellers borrow shares before trading, instead of merely locating a lender.
[26] During the financial crisis, the SEC prohibited all market participants, except market makers, from shorting financial stocks from September 18 to October 8, 2008.

the financial crisis, we exclude financial firms (SICs between 6000 and 6999). The removal period covers the quarters that start after July 6, 2007, and end before February 24, 2010, when the SEC reinstated the revised tick test. The regression is modified from Regression (1): we replace *PILOT* with *NPILOT* and *POST* with *REMOVAL*. *NPILOT* is the indicator variable for the control firms. *REMOVAL* is the indicator variable for the removal period; it equals one for firm-quarters during the removal period, and zero for firm-quarters during the pilot program. The interaction of *NPILOT* and *REMOVAL* thus captures the change in forecast frequency for the control firms during the removal period relative to the pilot firms.

Table 8 reports the results. For long-run good news forecasts, the coefficient on *NPILOT* × *REMOVAL* is significantly positive (p = 0.051). For long-run bad news forecasts, the coefficient on *NPILOT* × *REMOVAL* is insignificant (p = 0.582). That is, after the removal of the tick test for all firms, the control firms experience a significant increase in the frequency of long-run good news forecasts compared to the pilot firms, but the two groups of firms do not differ significantly in the change in the frequency of long-run bad news forecasts. These results are similar to those for the pilot firms during the pilot program, lending further support to the main inferences.

[Insert Table 8 here]

5.3 Guiders versus non-guiders

Balakrishnan et al. (2014) argue that the cost of initiating management forecasts is usually higher than the cost of increasing the frequency of forecasts. If this is the case, we expect our results to be stronger for the pilot firms that provide management forecasts in the pre period (guiders) than for those that do not (non-guiders). To test this, we construct an indicator variable, *Guider*, for the firms that issue at least one long-run management forecast in the pre period. We then use a similar design as the cross-sectional analyses in Section 4.2 to test whether the results are stronger for guiders than for non-guiders. Note that *Guider* and its interaction with *PILOT* are not included because they are subsumed by firm fixed effects.

Table 9 reports the results. While the coefficient on *PILOT* × *POST* is insignificant, the coefficient on *PILOT* × *POST* × *Guider* is significantly positive (p = 0.013). This result indicates that the significant increase in the frequency of long-run good news forecasts is driven by the pilot firms that have issued long-run forecasts in the past.

[Insert Table 9 here]

5.4 The pilot firms' enhanced good news disclosures and information environment

We find that in response to an increased short-selling threat, pilot firms enhance long-run good news disclosures to deter short sellers. Aside from deterring short sellers, such a strategy can increase the overall quality of the pilot firms' information environment. We test this empirically; consistent with our conjecture, we find that during the pilot program, analyst forecast dispersion and forecast error become significantly lower for pilot firms that increase the issuance of long-run good news forecasts, compared to the other pilot firms. This result indicates that pilot firms' strategic disclosure response to increased short-selling threat has significant implications for these firms' information environments.

6. Conclusion

In this paper, we examine how short sellers influence long-run management forecasts using a natural experiment—the SEC's pilot program of suspending the tick test for short orders for a group of randomly selected firms (the pilot firms). The pilot program significantly reduces the short-selling constraints and increases the short-selling threat for the pilot firms. We find that compared to the control firms, the pilot firms increase the issuance of long-run good news forecasts from the pre to the post period to discourage short sellers. The effect is economically significant. Compared to the control firms, the pilot firms experience a relative increase of 18% in long-run good news forecast frequency during the pilot program. In contrast, we find that the pilot firms do not change the issuance of long-run bad news forecasts relative to the control firms. The results for long-run good news forecasts are more pronounced when the pilot firms have higher quality forecasts, greater uncertainty about firm value, or higher manager equity incentives.

Our findings indicate that in response to the increase in short-selling threat, managers enhance long-run disclosures by bringing good news forward. Our paper contributes to the literature by shedding light on how short sellers, an increasingly important group of market players, influence long-run corporate disclosures. Given that other market players, such as institutional investors and financial analysts, also influence disclosures and can have different preferences from short sellers, future research can examine the interplay of various market players in influencing corporate disclosures.

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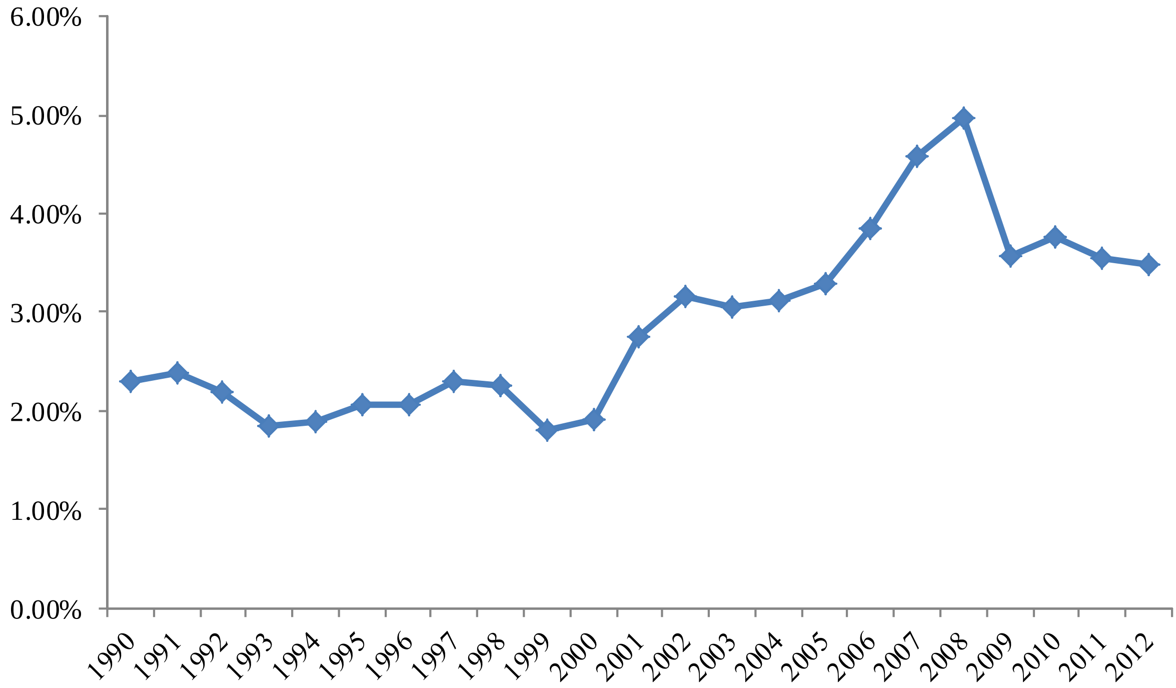
Appendix

Variable definitions

Variable	Definition
MF_N	The number of long-run management forecasts issued in the quarter; long-run forecasts include those with horizons greater than 90 days;
PILOT	Indicator for pilot firms, defined as one if a firm was selected by the SEC for the pilot program, and zero otherwise;
POST	Indicator for the post period, defined as one for the duration of the pilot program, including the fiscal quarters that start after May 2, 2005, and end before July 6, 2007; it is zero for the pre period, including the fiscal quarters that start on or after January 1, 2002, and end before July 28, 2004;
Analyst Following	The number of unique analysts who issued forecasts for the firm in the previous year;
Institutional Ownership	The percentage of outstanding shares held by institutional investors in the previous quarter;
Size	Total assets (in millions), measured at the end of the previous quarter; for regressions, we use the natural logarithm of this variable;
M/B	The ratio of market value to book value of equity, measured at the end of the previous quarter;
Earnings Volatility	Standard deviation of quarterly return on equity in the previous four years;
Return Volatility	Volatility of daily stock returns in the current quarter;
Prior Return	Cumulative size-adjusted returns in the previous year;
Analyst Optimism	Indicator for analyst optimism, defined as one if the consensus analyst forecast at the beginning of the quarter is optimistic relative to the realized earnings, and zero otherwise.

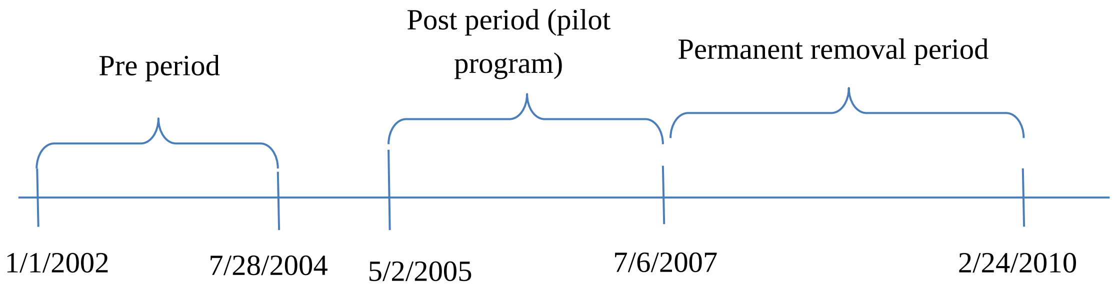
Figure 1 Time-series trend of short interest



Notes:

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 firms with available data on short interest and the number of outstanding shares from Compustat over the 1990−2012 period.

Figure 2 Timeline



Key dates:

6/23/2004	The SEC adopted Regulation SHO.
7/28/2004	The SEC announced the list of pilot firms.
5/2/2005	The pilot program started.
7/6/2007	The pilot program ended, and the SEC permanently suspended the tick test for all U.S. publicly listed stocks.
2/24/2010	The SEC reinstated the revised tick test, which only applies under limited circumstances.

TABLE 1
Sample selection and comparison of the pilot and control firms

Panel A: Sample selection

Restrictions		Number of firms
Firms included in the Russell 3000 index in 2004		2,998
Less:		
Firms not in the Russell 3000 index in 2005	394	
Firms not listed on NYSE, AMEX, or Nasdaq, or firms with IPOs after April 30, 2004	19	
Firms that changed tickers during the pilot program	65	
Firms without required financial, stock price, or analyst data in the post period	168	
Firms without required financial, stock price, or analyst data in the pre period	62	
Firms without the same number of quarters in the pre and post periods	108	
Final sample		2,182
Pilot firms		738
Control firms		1,444

TABLE 1 (Continued)

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

	Pilot firms (N=738)			Control firms (N=1,444)			P-value for the differences between the pilot and control firms	
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median
Size (in millions)	5,651	1,092	16,037	7,172	1,210	22,304	0.10	0.45
M/B	3.10	2.29	2.96	3.05	2.30	3.01	0.73	0.81
Leverage	0.22	0.20	0.20	0.22	0.19	0.20	0.63	0.60
ROE	0.12	0.10	2.24	−0.49	0.10	20.83	0.27	0.35
Trading Volume	216,026	71,855	428,651	218,603	68,422	417,545	0.89	0.82
Analyst Following	10	7	9	10	8	9	0.85	0.52

Notes:
Panel A describes the sample selection process. Panel B 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 the firm was selected by the SEC for the pilot program, 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, *Trading Volume* is the average monthly trading volume (in number of shares), and *Analyst Following* is the number of analysts following the firm.

TABLE 2
Descriptive statistics

Panel A: Descriptive statistics on the regression variables

		Percentile					
	Mean	5%	25%	50%	75%	95%	Std. Dev.
MF_N (Good news forecast frequency)	0.239	0	0	0	0	1	0.582
MF_N (Bad news forecast frequency)	0.260	0	0	0	0	2	0.615
Analyst Following	10	1	4	8	15	27	9
Institutional Ownership	0.635	0.163	0.433	0.664	0.836	1	0.280
Size (in millions)	7,215	117	449	1,345	4,088	30,103	21,523
M/B	2.825	0.808	1.528	2.181	3.354	7.580	3.078
Earnings Volatility	0.228	0.024	0.038	0.054	0.096	0.768	0.764
Return Volatility	0.023	0.010	0.014	0.020	0.028	0.047	0.013
Prior Return	0.077	−0.453	−0.125	0.049	0.254	0.702	0.353
Analyst Optimism	0.310	0	0	0	1	1	0.463

TABLE 2 (Continued)

Panel B: Correlations among the independent variables

	PILOT	*POST*	*Analyst Following*	*Institutional Ownership*	*Size*	*M/B*	*Earnings Volatility*	*Return Volatility*	*Prior Return*
POST	0.00								
Analyst Following	0.00	0.05**							
Institutional Ownership	0.00	0.23**	0.26**						
Size	−0.03**	0.03**	0.38**	−0.01					
M/B	0.01*	0.07**	0.12**	0.06**	−0.04**				
Earnings Volatility	−0.01	0.00	−0.01**	−0.02**	−0.05**	0.10**			
Return Volatility	−0.01**	−0.28**	−0.07**	−0.11**	−0.17**	−0.03**	0.17**		
Prior Return	0.00	−0.17**	−0.07**	−0.10**	−0.04**	0.19**	0.05**	0.05**	
Analyst Optimism	0.00	0.06**	0.03**	0.06**	0.00	−0.03**	0.00	0.01	−0.10**

Notes:
The sample includes 32,302 firm-quarters from 2,182 firms, including 738 pilot firms and 1,444 control firms. Panel A presents descriptive statistics on the regression variables. The pre period includes the fiscal quarters that start on or 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 focus on long-run management forecasts, including forecasts with horizons greater than 90 days. 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 the 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." Panel B presents correlations among the independent variables. Please see the Appendix for variable definitions. *, ** indicate significance levels of 0.05 and 0.01, respectively, based on two-tailed tests.

TABLE 3
Short-selling and long-run management forecast frequency

	Good News Forecasts	**Bad News Forecasts**
POST	0.008	0.018*
	(0.468)	(0.092)
PILOT* × *POST	**0.043****	**0.007**
	(0.015)	**(0.697)**
Analyst Following	0.002	0.006***
	(0.125)	(0.000)
Institutional Ownership	−0.062***	0.091***
	(0.008)	(0.002)
Size	0.004	0.070***
	(0.801)	(0.000)
M/B	0.003**	0.001
	(0.033)	(0.580)
Earnings Volatility	−0.005	−0.012
	(0.680)	(0.212)
Return Volatility	−0.470	0.756**
	(0.107)	(0.038)
Prior Return	0.046***	0.018*
	(0.000)	(0.088)
Analyst Optimism	−0.045***	0.045***
	(0.000)	(0.000)
Firm Fixed Effects	Yes	Yes
N	32,302	32,302
Adjusted R^2	1.13%	2.12%

Notes:
This table reports results from the following regression:
$$MF\_N = \alpha_0 + \alpha_1 POST + \alpha_2 PILOT \times POST + \beta Control\,Variables + Firm\,Fixed\,Effects + \varepsilon$$
The sample includes 32,302 firm-quarters from 2,182 firms, including 738 pilot firms and 1,444 control firms. Please see Table 2 for the definition of the pre (post) period and the classification of good news (bad news) management forecasts and the Appendix for variable definitions. The p-values are in parentheses below the coefficient estimates. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively, based on two-sided tests using standard errors adjusted for firm-level clustering. Bold text indicates variable of interest.

TABLE 4
Short-selling and long-run good news forecast frequency—Conditional on management forecast quality

	Good News Forecasts
POST	−0.011
	(0.620)
PILOT* × *POST	**0.065***
	(0.063)
MF_PastAcc	0.051**
	(0.027)
PILOT × *MF_PastAcc*	−0.011
	(0.646)
POST × *MF_PastAcc*	−0.052**
	(0.027)
PILOT* × *POST* × *MF_PastAcc	**0.108*****
	(0.008)
Control Variables & Firm Fixed Effects	Yes
N	15,346
Adjusted R^2	1.03%

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

$$MF\_N = \alpha_0 + \alpha_1 POST + \alpha_2 PILOT \times POST + \alpha_3 MF\_PastAcc + \alpha_4 PILOT \times MF\_PastAcc + \alpha_5 POST \times MF\_PastAcc + \alpha_6 PILOT \times POST \times MF\_PastAcc + \beta Control\,Variables + Firm\,Fixed\,Effects + \varepsilon$$

The sample includes 15,346 firm-quarters from 1,196 firms, including 405 pilot firms and 791 control firms. Please see Table 2 for the definition of the pre (post) period and the classification of good news (bad news) management forecasts and the Appendix for variable definitions. *MF_PastAcc* is calculated as the average accuracy of long-run management forecasts issued in the three years before the current quarter, where accuracy is measured as negative one times the absolute value of the difference between management forecasts and actual earnings. This variable is demeaned (i.e., the sample mean is subtracted from the value). The p-values are in parentheses below the coefficient estimates. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively, based on two-sided tests using standard errors adjusted for firm-level clustering. Bold text indicates variable of interest.

TABLE 5
Short-selling and long-run good news forecast frequency—Conditional on uncertainty about firm value

Panel A: The magnitude of accruals

	Good News Forecasts
POST	0.013
	(0.265)
PILOT **×** ***POST***	**0.043****
	(0.021)
|*Accruals*|	0.147
	(0.124)
PILOT × |*Accruals*|	−0.370**
	(0.027)
POST × |*Accruals*|	−0.609***
	(0.000)
PILOT **×** ***POST*** **×** **|*Accruals*|**	**0.635****
	(0.020)
Control Variables & Firm Fixed Effects	Yes
N	29,032
Adjusted R^2	1.88%

Panel B: Earnings volatility

	Good News Forecasts
POST	0.008
	(0.467)
PILOT **×** ***POST***	**0.043****
	(0.014)
Earn_Volatility	0.024
	(0.195)
PILOT × *Earn_Volatility*	−0.073***
	(0.010)
POST × *Earn_Volatility*	-0.021
	(0.143)
PILOT **×** ***POST*** **×** ***Earn_Volatility***	**0.047****
	(0.035)
Control Variables & Firm Fixed Effects	Yes
N	32,302
Adjusted R^2	1.04%

TABLE 5 (Continued)

Panel C: Growth versus value stocks

	Good News Forecasts
POST	0.008
	(0.448)
PILOT* × *POST	**0.042****
	(0.015)
Growth	0.009***
	(0.001)
PILOT × *Growth*	−0.012**
	(0.019)
POST × *Growth*	−0.009**
	(0.012)
PILOT* × *POST* × *Growth	**0.016*****
	(0.009)
Control Variables & Firm Fixed Effects	Yes
N	32,302
Adjusted R^2	1.23%

Notes:

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

$$MF\_N = \alpha_0 + \alpha_1 POST + \alpha_2 PILOT \times POST + \alpha_3 Conditional\_Variable + \alpha_4 PILOT \times Conditional\_Variable$$
$$+\alpha_5 POST \times Conditional\_Variable + \alpha_6 PILOT \times POST \times Conditional\_Variable + \beta Control\ Variables$$
$$+ Firm\ Fixed\ Effects + \varepsilon$$

The full sample includes 32,302 firm-quarters from 2,182 firms, including 738 pilot firms and 1,444 control firms. Please see Table 2 for the definition of the pre (post) period and the classification of good news (bad news) management forecasts and the Appendix for variable definitions. *Conditional_Variable* is |*Accruals*| in panel A, *Earn_Volatility* in panel B, and *Growth* in panel C. |*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. *Growth* is the market-to-book ratio at the end of the previous quarter. These variables are demeaned (i.e., the sample mean is subtracted from the value). The p-values are in parentheses below the coefficient estimates. ** and *** indicate significance levels of 0.05 and 0.01, respectively, based on two-sided tests using standard errors adjusted for firm-level clustering. Bold text indicates variable of interest.

TABLE 6
Short-selling and long-run good news forecast frequency—Conditional on managers' equity incentives

	Good News Forecasts
POST	−0.001
	(0.964)
PILOT* × *POST	**0.049****
	(0.049)
Equity_Incentives	0.009
	(0.538)
PILOT × *Equity_Incentives*	−0.011
	(0.629)
POST × *Equity_Incentives*	−0.021*
	(0.098)
PILOT* × *POST* × *Equity_Incentives	**0.048***
	(0.059)
Control Variables & Firm Fixed Effects	Yes
N	19,915
Adjusted R^2	1.00%

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

$$MF\_N = \alpha_0 + \alpha_1 POST + \alpha_2 PILOT \times POST + \alpha_3 Equity\_Incentives + \alpha_4 PILOT \times Equity\_Incentives$$
$$+ \alpha_5 POST \times Equity\_Incentives + \alpha_6 PILOT \times POST \times Equity\_Incentives + \beta Control\ Variables + Firm\ Fixed\ Effects + \varepsilon$$

The sample includes 19,915 firm-quarters from 1,396 firms, including 483 pilot firms and 913 control firms. Please see Table 2 for the definition of the pre (post) period and the classification of good news (bad news) management forecasts and the Appendix for variable definitions. *Equity_Incentives* is the change in the value of managers' stock and option holdings with a 1% increase in stock price, scaled by managers' salary. This variable is demeaned (i.e., the sample mean is subtracted from the value). The p-values are in parentheses below the coefficient estimates. * and ** indicate significance levels of 0.10 and 0.05, respectively, based on two-sided tests using standard errors adjusted for firm-level clustering. Bold text indicates variable of interest.

TABLE 7
Long-run management forecasts and change in short interest

	Change in Short Interest
Intercept	0.354***
	(0.000)
Good_News	**−0.185****
	(0.027)
Bad_News	**0.055**
	(0.569)
Δ*Size*	3.210***
	(0.004)
Δ*M/B*	0.089
	(0.367)
Δ*ROE*	−0.007***
	(0.004)
Δ*Prior Return*	−0.003
	(0.370)
Δ*Analyst Forecast Error*	0.000
	(0.648)
Δ*Meet*	−0.087
	(0.259)
N	501
Adjusted R^2	2.97%

Notes:
This table reports the results from regressing the change in short interest on the frequency of long-run management forecasts based on the following regression:

$$\Delta SHORT = \alpha_0 + \alpha_1 \text{Good\_}News + \alpha_2 \text{Bad\_}News + \beta Control\,Variables + \varepsilon$$

Δ*SHORT* is the percentage change in short interest from the first two quarters in the post period to the later quarters in the post period. *Good_News* (*Bad_News*) is the number of long-run good news (bad news) management forecasts issued in the post period (excluding the first two quarters). Please see Table 2 for the classification of good news (bad news) management forecasts. Δ*Size* is the change in the natural logarithm of total assets. Δ*M/B* is the change in the market-to-book ratio. Δ*ROE* is the change in return on equity. Δ*Prior Return* is the change in prior annual size-adjusted stock returns. Δ*Analyst Forecast Error* is the change in analyst forecast error. Δ*Meet* is the change in the incidence of meeting or beating analyst forecasts. All changes are measured from the first two quarters in the post period to the later quarters in the post period. The sample includes 501 pilot firms with an increase in short interest in the first two quarters of the post period compared to the pre period. The p-values are in parentheses below the coefficient estimates. ** and *** indicate significance levels of 0.05 and 0.01, respectively, based on two-sided tests. Bold text indicates variable of interest.

TABLE 8
Short-selling and long-run management forecast frequency—Analysis of the permanent removal period

	Good News Forecasts	Bad News Forecasts
REMOVAL	−0.014	−0.049**
	(0.453)	(0.011)
NPILOT* × *REMOVAL	**0.041***	**−0.012**
	(0.051)	**(0.582)**
Analyst Following	0.001	0.004
	(0.605)	(0.110)
Institutional Ownership	−0.027**	0.057*
	(0.027)	(0.094)
Size	0.036	0.090***
	(0.142)	(0.000)
M/B	0.001	0.002
	(0.343)	(0.433)
Earnings Volatility	−0.005	0.008
	(0.673)	(0.354)
Return Volatility	−1.071***	0.054
	(0.000)	(0.826)
Prior Return	0.052***	0.016
	(0.000)	(0.202)
Analyst Optimism	−0.053***	0.059***
	(0.000)	(0.000)
Firm Fixed Effects	Yes	Yes
N	19,260	19,260
Adjusted R^2	3.74%	2.94%

Notes:
This table reports results from the following regression:

$$MF\_N = \alpha_0 + \alpha_1 REMOVAL + \alpha_2 NPILOT \times REMOVAL + \beta Control\,Variables + Firm\,Fixed\,Effects + \varepsilon$$

The sample includes 19,260 firm-quarters for 1,401 firms, including 491 pilot firms and 910 control firms for 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. *NPILOT* equals one for the control firms and zero for the pilot firms. *REMOVAL* equals one for firm-quarters in the permanent removal period and zero for firm-quarters during the pilot program. Please see Table 2 for the classification of good news (bad news) management forecasts and the Appendix for the definitions of other variables. The p-values are in parentheses below the coefficient estimates. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively, based on two-sided tests using standard errors adjusted for firm-level clustering. Bold text indicates variable of interest.

TABLE 9
Short-selling and long-run good news forecast frequency—Guiders versus non-guiders

	Good News Forecasts
POST	0.039***
	(0.000)
PILOT × POST	**−0.001**
	(0.870)
POST× Guider	−0.055***
	(0.002)
PILOT × POST × Guider	**0.077****
	(0.013)
Analyst Following	0.002
	(0.141)
Institutional Ownership	−0.063***
	(0.008)
Size	0.005
	(0.755)
M/B	0.003**
	(0.029)
Earnings Volatility	−0.005
	(0.655)
Return Volatility	−0.478
	(0.102)
Prior Return	0.045***
	(0.000)
Analyst Optimism	−0.045***
	(0.000)
Firm Fixed Effects	Yes
N	32,302
Adjusted R^2	0.29%

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

$$MF\_N = \alpha_0 + \alpha_1 POST + \alpha_2 PILOT \times POST + \alpha_3 POST \times Guider + \alpha_4 PILOT \times POST \times Guider$$
$$+ \beta Control\,Variables + Firm\,Fixed\,Effects + \varepsilon$$

The sample includes 32,302 firm-quarters from 2,182 firms, including 738 pilot firms and 1,444 control firms. *Guider* equals one for firms that issue at least one long-run management forecast in the pre period and zero otherwise. Please see Table 2 for the definition of the pre (post) period and the classification of good news (bad news) management forecasts and the Appendix for the definitions of other variables. The p-values are in parentheses below the coefficient estimates. ** and *** indicate significance levels of 0.05 and 0.01, respectively, based on two-sided tests using standard errors adjusted for firm-level clustering. Bold text indicates variable of interest.