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Short Selling and Firms' Disclosure of Bad News: Evidence from Regulation SHO

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ABSTRACT

As informed traders, short sellers enhance the informativeness of stock prices, especially related to bad news, potentially reducing the benefits and increasing the litigation and reputational costs of withholding bad news by managers. We exploit a quasi-natural experimental setting provided by the introduction of SEC regulation SHO (Reg-SHO), which significantly reduced the constraints faced by short sellers for an effectively randomly selected subsample of U.S. firms (pilot firms). Relative to control firms, we find pilot firms increase the likelihood of voluntary bad news management forecasts; provide these forecasts in a more timely manner; and accelerate the release of quarterly bad earnings news. Each of these effects is stronger for subsamples of moderate (compared with extreme) bad news, firms facing high (relative to low) litigation risks, and firms with a forecasting history. Similar effects are not observed for voluntary good news forecasts. A range of robustness tests reinforce our results.

Keywords: short selling, voluntary disclosure, litigation risk

JEL Classification: G14, D22, K22, K41, M40

INTRODUCTION

In this paper we investigate whether short selling influences the disclosure of bad news by firms. We exploit a quasi-natural experimental setting provided by the introduction of SEC regulation SHO (Reg-SHO), which significantly reduced the constraints faced by short sellers for a subsample of U.S. firms, and investigate whether this was associated with changes in the voluntary disclosure of bad news by firms. We focus on three dimensions of voluntary disclosure: the likelihood that bad news is voluntarily disclosed by firms, the timing of any voluntarily disclosed bad news, and the timing of mandatory quarterly earnings releases when the earnings news is bad.

Managers have incentives to withhold and/or delay the release of bad news (e.g., Acharya, DeMarzo, and Kremer 2011; Bao, Kim, Mian, and Su 2018; Clinch and Verrecchia 2015; Dye 1985; Jung and Kwon 1988; Kothari, Shu, and Wysocki 2009; Verrecchia 1983, 2001). Short selling potentially affects these incentives by increasing the likelihood that bad news held privately by informed traders is reflected in stock prices. Short sellers represent informed traders and their actions assist in impounding negative news more quickly into stock prices directly via their trading activities (e.g., Diamond and Verrecchia 1987), and indirectly through the public availability of short interest information (e.g., Desai, Ramesh, Thiagarajan, and Balachandran 2002; Diether, Lee, and Werner 2009b; Senchack and Starks 1993). Because short sellers and managers likely have overlapping information sets (e.g., Massa, Qian, Xu, and Zhang 2015a; Massa, Zhang, and Zhang 2015b), this can affect both the benefits and costs to firms/managers of withholding the disclosure of bad news. Any benefit to managers from withholding bad information will likely be reduced since at least some of their information will already be reflected in price, or will soon be reflected, through the actions of short sellers. Regarding costs, prior research indicates that litigation and reputational costs play an important role in managers' bad news disclosure decisions (e.g., Field, Lowry, and Shu 2005; Kothari et al. 2009; Skinner 1994, 1997). And uncertainty whether firms possess private information assists them in withholding bad news (e.g., Dye 1985; Jung and Kwon 1988). To the extent that short selling causes prices to already, or imminently, reflect some bad news held by managers, it will be more difficult for managers to claim they do not possess that bad news. Consequently, if managers withhold bad news, such an action will be more likely to be detected and lead to higher litigation and reputational costs. Combining these two effects — a reduction in benefits and an increase in costs associated with withholding bad news — suggests that short selling will motivate greater and more timely disclosure of bad news by firms.

In this paper, we exploit an exogenous shock to short selling activities to investigate this prediction. In July 2004, the SEC adopted Rule 202T of Reg-SHO, which established procedures for the SEC to temporarily suspend any short sale price test (including the tick test for exchange-listed stocks and the bid test for Nasdaq National Market stocks) — a short sale constraint — on short selling in U.S. equity markets, in order for the SEC to study the effectiveness of the tests.[1] Under the Reg-SHO pilot program short sale constraints were suspended for a subset of (effectively) randomly selected firms (pilot firms), while short sale constraints remained unchanged for other firms (control firms).[2] As a result, around the announcement date short interest significantly increased for pilot firms compared to control firms (Grullon, Michenaud, and Weston 2015), consistent with Reg-SHO having a substantial effect on short selling activities for pilot firms. Managers also appeared sensitive to such effects on their firms. For instance, in a 2008 NYSE survey, the majority of top executives surveyed were in favour of re-instituting the price tests as soon as possible (Grullon et al. 2015). Therefore, the exogenous shock to short sale constraints along with the effective randomization of treatment/pilot firms under Reg-SHO provides an attractive quasi-natural experimental setting within which to examine the potential causal effect of short selling on corporate bad news disclosure.

We focus on the disclosure of bad earnings news by firms and investigate three aspects of voluntary disclosure via difference-in-differences (DiD) comparisons of pilot vs. control firms before, during and after the pilot program. During the pilot program, for the full sample of firms, we observe a significant increase, approximately 5%, in the likelihood of bad news management forecasts among the pilot firms. Managers also provide these forecasts in a more timely fashion,

[1] In 1938, the NYSE adopted an uptick rule, Rule 10a-1, known as the "tick test". The rule requires that a short sale cannot be completed if the current bid price is below the most recently traded price (plus tick). In 1994, the NASDAQ also adopted its own price test under Rule 3350, requiring a short sale to occur at a price one penny above the current bid price if the current bid price is a downtick from previous bid. The purpose of these tests is to prevent short sellers from participating in market manipulation that forces prices downward.

[2] According to the Rule 202T's pilot program, stocks in the Russell 3000 index as of June 25, 2004 were ranked by average daily dollar volume of trade over the one year prior to the issuance of Reg-SHO from highest to lowest for the period. Within each exchange — American Stock Exchange, New York Stock Exchange or NASDAQ — every third ranked stock was drawn from the pool and assigned to the pilot group, resulting in a pilot group (comprising 986) stocks and a control group (comprised of all the remaining stocks in the Russell 3000 index). From May 2, 2005 to August 6, 2007, pilot stocks were exempted from short-sale price tests, and after August, 2007, the SEC repealed the price test rule on short selling for all stocks.

on average approximately 2 days earlier. Additionally, we find that managers accelerate the release of mandatory quarterly earnings news when the news is bad, on average approximately 0.2 days earlier. Moreover, the results are stronger for the subset of bad news disclosures representing less extreme bad news, where changes to short selling prohibitions are more likely to have an effect at the margin. The results are also generally stronger for the subset of firms drawn from higher litigation risk/cost industries (e.g., Field et al. 2005; Skinner 1994, 1997), as would be expected, although there is some sensitivity to the approach used to measure litigation risk. These results provide strong and consistent support for a link between short selling and the voluntary disclosure of bad news by firms.

We conduct several additional analyses. Previous research (e.g., Billings, Jennings, and Lev 2015; Field et al. 2005; Rogers and Van Buskirk 2013) indicates that firms' prior forecasting behavior is an important factor in explaining their subsequent voluntary forecasting decisions. To investigate the role of prior forecasting behavior on our results, we divide our sample into three groups: firms which did not forecast earnings in the period prior to the announcement of the Regulation SHO pilot program; firms which issued less than or equal to three earnings forecasts in the pre-Regulation SHO period; and firms which issued greater than three earnings forecasts in the pre-Regulation SHO period. Only firms who had issued more than three earnings forecasts exhibit statistically significant effects of Regulation SHO on three dimensions of voluntary forecasting we study. This is consistent with Regulation SHO only having an impact on firms whose prior forecasting behavior suggests were on the margin in terms of the costs and benefits associated with voluntary forecasting.

We also find that the effects of Reg-SHO on the likelihood and timing of bad news management forecasts are stronger for pilot firms that experienced stronger negative share price reactions when the Reg-SHO pilot program was first announced, consistent with the disclosure changes being causally linked to the regulatory change in short-selling. Finally, to rule out the possibility that our findings are due to a change in managerial disclosure in general (both good news and bad news), we examine the effects of Reg-SHO on good news disclosure. We find marginally significant evidence of a *decrease* in the likelihood of good news disclosure by pilot firms and no significant difference in the timing of good news disclosures between pilot and control firms during the pilot program period. Thus our findings for bad news disclosure do not appear attributable to a change in firms' disclosure of all news.

We demonstrate that our main results remain qualitatively unchanged for a series of robustness tests. First, following Rogers and Van Buskirk (2013), we use an alternative definition of management forecast news to classify bad news forecasts, correcting for the potential bias of conventional management forecasts news when applied to forecasts bundled with earnings announcements. Second, we control for the bundling factor in the regressions of the timing of bad news forecasts to mitigate the concern that a reduced delay in earnings announcements mechanically leads to more timely forecasts for those bundled. Third, to address the possibility that our main findings—the difference in disclosure behavior between pilot and control firms during Reg-SHO pilot program — are due to chance, we conduct sensitivity analyses using randomly generated pseudo "pilot firms" and a pseudo "pilot program period" and find no significant results. Finally, as Grullon et al. (2015) find that pilot firms significantly reduce equity issuance during Reg-SHO, it is possible that our results may be due to a decrease in external financing needs in pilot firms. To mitigate this concern, we partition sample firms based on whether firms issue equity in the subsequent year and find that our results hold in non-equity issuing firms.

In summary, our study provides evidence that short selling does influence the disclosure of bad news by firms – the likelihood of issuing bad news earnings forecasts, the timing of those forecasts, and the timing of bad news earnings releases. All three effects are apparent for our full sample, but concentrated in firms with moderately bad news, firms facing high litigation risk, and firms with an already relatively high (pre-Regulation SHO) likelihood of issuing earnings forecasts. Our research complements previous research on the influence of short selling on other major corporate decisions which suggests that short sellers mitigate manager-investor agency problems by curbing earnings management (e.g., Fang, Huang, and Karpoff 2016; Massa et al. 2015b), deterring financial misconduct (e.g., Karpoff and Lou 2010), and enhancing investment efficiency (e.g., Chang, Lin, and Ma 2014; He and Tian 2014). In addition, our findings of more bad news disclosure by pilot firms are consistent with higher auditor fees charged to these firms to hedge against down-side risk (Hope, Hu, and Zhao 2017). Finally, our finding of more timely disclosure of bad news by managers is also in line with the findings from related research regarding analysts' forecasting behaviour. Ke, Lo, Sheng, and Zhang (2018) show that the lifting of short-selling constraints in Regulation SHO improves analysts' forecast quality via both a "disciplining effect" (stronger incentives for analysts to uncover bad news) and an "information effect" (enhanced price informativeness via more active short selling).

The remainder of the paper is organized as follows. In section 2 we describe the relevant regulatory background and develop our hypotheses. We also discuss two recent papers (Li and Zhang 2015 and Chen, Cheng, Luo and Yue 2014) that employ the Reg-SHO pilot program to investigate the link between firms' voluntary disclosure policies and short selling, but differ from our research both in their predictions and in some of their results. Sections 3 and 4 describe the sample and our research design, while section 5 presents our results. We conclude in section 6.

RELATED LITERATURE AND HYPOTHESES

The background of Reg-SHO

In July 2004, the SEC adopted Rule 202T of Reg-SHO, which established procedures for the SEC to temporarily suspend any short sale price test for a subset of firms (the pilot firms), while short sale constraints remained unchanged for other firms (the control firms). According to Rule 202T's pilot program, stocks in the Russell 3000 index as of June 25, 2004 were ranked by average daily dollar volume of trade over the one year period prior to the issuance of Reg-SHO from highest to lowest for the period. Within each exchange — American Stock Exchange, New York Stock Exchange or NASDAQ — every third ranked stock was drawn from the pool and designated a member of the pilot group, resulting in a pilot group comprising 986 stocks and a control group comprised of all the remaining stocks in the Russell 3000 index.

From May 2, 2005 to July 6, 2007, pilot stocks were exempted from short-sale price tests, and after August, 2007, the SEC repealed the price test rule on short selling for all stocks. However, the removal of short-sale price tests was not welcomed by managers and exchanges. In a 2008 NYSE survey, 85% of top executives surveyed were in favour of re-instituting the price tests as soon as possible (Grullon et al. 2015). Various parties including law firms, members of congress and journalists blamed the SEC for the 2007/08 financial crisis in part because of increased short selling. Under this pressure, on February 24, 2010, SEC restored a modified uptick rule that is triggered when a security's price declines by 10% or more from the previous closing price.

The potential impact of Reg-SHO on bad news disclosure

Managers have various incentives to withhold bad earnings-related news and avoid the resulting negative stock price effects (e.g., Graham, Harvey, and Rajgopal 2005; Kothari et al. 2009). They also bear potentially large costs when investors are surprised by large negative news on earnings announcement dates (Skinner 1994). Managers can be sued for not releasing adverse earnings surprises promptly. They may also incur reputational costs for failing to disclose bad news in a timely manner. Those firms with poor reporting reputations are less likely to be followed by analysts and money managers, which reduces the price and/or liquidity of their firms' stocks (Skinner 1994).

Short selling likely influences both the benefits and costs to managers associated with their decision to disclose or withhold bad news. Prior studies suggest that short sellers are informed investors and their information set overlaps managers' private information. Greater short interest is associated with poor future performance, negative earnings surprises and managerial financial misconduct (e.g., Christophe, Ferri, and Angel 2004; Diether, Lee, and Werner 2009a; Karpoff and Lou 2010). Thus, an increase in abnormal short interest is often perceived as a bad signal to market participants (e.g., Diamond and Verrecchia 1987), and stock prices fall in response to the announcement of an increase in short interest (e.g., Aitken, Frino, McCorry, and Swan 1998). As a result, stock prices already likely incorporate at least partially the undisclosed upcoming bad news withheld by managers, reducing the benefits from withholding disclosure.[3]

[3] Consistent with this argument, Karpoff and Lou (2010) report evidence that for firms that are experiencing financial misconduct, those with high abnormal short interest are publicly revealed 8 months earlier than firms with low abnormal short interest.

On the cost side, since managers' ability to withhold unfavourable news is constrained by investors' beliefs about their possession of private information (e.g., Dye 1985; Jung and Kwon 1988), it will be harder for managers to hide bad news and claim "no news" when investors' beliefs are updated via new information from short selling. As a result, managers are likely to face larger litigation and reputational costs if they continue withholding bad news (Trueman 1997).

In summary, in the presence of short selling managers are likely to face lower incremental benefits from withholding bad news and higher potential litigation and reputational costs, leading to increased and more timely disclosure of bad earnings news.

The introduction of Reg-SHO acted as an exogenous shock to short selling constraints for the pilot firms. The reduction in constraints led to a marked increase in short selling for firms in the Reg-SHO pilot group. Diether et al. (2009b) report an 8 percent increase in intraday short sales immediately following the implementation date of the pilot program on May 2, 2005. Similarly, Grullon et al. (2015) document a 19 percent increase in monthly short interest for pilot firms around the announcement date of Reg-SHO compared to control firms. These findings provide evidence that the removal of short sale price tests under Reg-SHO was significant enough to materially affect the extent of short selling activity, and suggest that the increase in short selling pressure for pilot firms could affect managerial decisions. Relating to the disclosure of bad earnings news, we expect that the enhanced threat from short selling motivated managers of pilot firms to release bad news voluntarily through management earnings forecasts more so than for control firms. This leads to our first hypothesis:

H1: During the Reg-SHO pilot period, firms in the pilot group were more likely to disclose bad news through management earnings forecasts compared to those in the control group.

Prior research also indicates that managers not only have incentives to withhold bad news, they also delay the voluntary disclosure of bad news (e.g., Kothari et al. 2009). As discussed above, since short selling is likely to reduce the incentives for managers to withhold bad news, we expect short selling to similarly reduce managers' incentive to delay the voluntary release of bad news:

H2: During the Reg-SHO pilot period, firms in the pilot group accelerated the release of bad news management earnings forecasts compared to those in the control group.

Finally, turning to mandatory earnings announcements, prior research documents that managers may deliberately delay the release of (mandatory) earnings news which is unfavorable (e.g., Bagnoli, Kross, and Watts 2002; Begley and Fischer 1998; Chambers and Penman 1984; Cohen, Dey, Lys, and Sunder 2007). As discussed above, Reg-SHO enhances the informativeness of stock prices with respect to negative news for pilot firms, which reduces the information asymmetry between investors and firm managers. Due to increased short-selling, investors of pilot firms are likely aware of/anticipate the upcoming bad earnings news. As a result, there is less incentive for managers to delay the earnings release. Instead, they may accelerate the earnings announcement to build a reputation for transparent reporting or to reduce potential litigation and/or reputational costs associated with late reporting. This leads to our third hypothesis:

H3: During the Reg-SHO pilot period, firms in the pilot group accelerated the release of bad earnings news compared to those in the control group.

Two related papers

Two recent papers (Li and Zhang 2015 and Chen et al. 2014) also employ the Reg-SHO pilot program to investigate the link between firms' voluntary disclosure policies and short selling, but differ from our research both in their predictions and in some of their results. Li and Zhang (2015) focus on the effect of short-selling on the precision of management bad news earnings forecasts and the readability of bad news annual reports. They first suggest that short selling increases the magnitude of stock price reaction to the public release of bad news management earnings forecasts, and provide empirical evidence consistent with this expectation. They argue that, as a result, firms face increased costs to voluntarily disclosing bad news forecasts (due to the greater negative price response to disclosure). Consequently, they predict that pilot firms will respond to Reg-SHO by *decreasing* their voluntary disclosure of bad news earnings forecasts and/or decreasing the precision of their bad news forecasts via wider range forecasts, and by decreasing the readability of bad news annual reports. They find no evidence of a change in likelihood of bad news forecasts by pilot firms, but report evidence consistent with their other two predictions.

In an unpublished paper, Chen, Cheng, Luo, and Yue (2014) focus on the effect of short-selling on the disclosure of good news management earnings forecasts. They predict that pilot firms will respond to Reg-SHO by increasing the voluntary disclosure of good news earnings forecasts in order to discourage short sellers. They argue that short selling imposes costs on firms/managers and that voluntarily disclosing good news causes short sellers to suffer losses and so discourages their attention. They report evidence consistent with their prediction. They also argue that the likely impact of Reg-SHO on the disclosure of bad news forecasts by firms is ambiguous, and find no evidence of an effect.

Our research differs from both Li and Zhang (2015) and Chen et al. (2014) in a number of respects. Like Li and Zhang (2015), but unlike Chen et al. (2014), we focus on bad news disclosure because prior research suggests that managers have incentives to withhold bad news, but not good news, and because short selling generally is associated with unfavourable news. Second, our predictions and results regarding the effect of Reg-SHO on the likelihood of bad news earnings forecast disclosure by firms differ from those of Li and Zhang (2015) and Chen et al. (2014). We predict and find that pilot firms will increase the likelihood of bad news disclosure during the Reg-SHO period. In contrast, Li and Zhang (2015) predict a decrease in the likelihood of bad news disclosure, while Chen et al. (2014) make no prediction. Both Li and Zhang (2015) and Chen et al. (2014) report insignificant results relating to this. Third, in addition to the likelihood of bad news forecast disclosure, we broaden the spectrum of corporate disclosure decisions to include the effect of Reg-SHO on the timing of such voluntary disclosures, and the timeliness of bad news (mandatory) earnings announcements. Our results indicate that Reg-SHO affected all of these dimensions of firm disclosure in a consistent manner. Fourth, we investigate several factors associated with how short selling affects firms' disclosure behavior. In particular, our results indicate that the effect of Reg-SHO on the disclosure of bad news by firms is concentrated in firms with moderately bad news, firms facing high litigation risk, and firms with an already relatively high (pre-Regulation SHO) likelihood of issuing earnings forecasts.

In addition, we are able to resolve, to a significant extent, inconsistencies between our results and those in Li and Zhang (2015) and Chen et al. (2014). Specifically, for our full sample, we report a statistically significant effect of Regulation SHO on the likelihood of firms issuing a bad news earnings forecast. In contrast, both Li and Zhang (2015) and Chen et al. (2014) report a corresponding statistically insignificant effect for the likelihood of bad news management earnings forecasts. To investigate this difference, we conducted a detailed replication of both papers' sample selection procedures and research design choices.[4,5] The results indicate that the difference is primarily attributable to the inclusion or exclusion of firms' prior forecasting behaviour as an explanatory/control variable. Previous research has indicated that firms' prior forecasting behaviour is an important factor in explaining subsequent disclosure choices (e.g., Billings et al. 2015; Field et al. 2005; Rogers and Van Buskirk 2013). We find in our replication that when prior forecasting behavior is included as a control variable there is a statistically significant effect of Regulation SHO on the likelihood of firms issuing a bad news earnings forecast. When prior forecasting behavior is not included as a control (as in Li and Zhang 2015, and Chen et al. 2014) there is no statistically significant effect. However, the inclusion or exclusion of firms' prior forecasting behavior as a control does not affect our results for the timing of bad news earnings forecasts and bad news earnings releases, nor any of our subsample results.[6]

[4] Details of the full replication procedures and results are available in the online appendix.
[5] Chen et al. (2014) also report a significant increase in good news earnings forecasts for pilot firms. However, Li and Zhang (2015) report no difference between pilot and control firms in the likelihood of good news management forecasts during the Reg-SHO period. We also find no difference for our sample.
[6] Note that the importance of the omission/inclusion of prior forecasting behaviour on the replicated results suggests that the difference-in-difference research design we, Li and Zhang (2015), and Chen et al. (2014) employ does not result in completely effective ex post randomisation of some firm characteristics between pilot and control firms, and points to the importance of also including appropriate controls suggested by prior research.

Some differences do remain between our research and Li and Zhang (2015). They predict and find that pilot firms decrease the precision of range earnings forecasts and decrease the readability of bad news annual reports during the Reg-SHO period. These results appear to be inconsistent with the increased disclosure of bad news earnings forecasts by pilot firms that we document.[7]

SAMPLE SELECTION

Panel A of Table 1 summarizes our sample selection process. We begin with the Russell index as of June 2004. On July 28, 2004, the SEC announced that out of the Russell 3000, 986 stocks would trade without any price test restrictions applied to short sales during the term of pilot program.[8] Following the SEC requirement, we exclude stocks that are not listed on the NYSE, AMEX or Nasdaq, and also those that went private or had spin-offs after April 30, 2004. As a result we identify 986 pilot firm stocks (the pilot group) according to the published list of the SEC's pilot order and 1986 non-pilot firm stocks (the control group).[9]

We obtain financial statement data from COMPUSTAT, institutional ownership data (form 13F) from the Thomson-Reuters CDA/spectrum database, and analyst following information from the I/B/E/S detail files. The quarterly EPS management forecast data is provided by the I/B/E/S guidance feed database.[10] We define the Reg-SHO pilot period

[7] We investigated the precision of range earnings forecasts for our sample and found, contrary to Li and Zhang's (2015) results, no significant difference between pilot and control firms during the Reg-SHO period. One potential explanation for this is that Li and Zhang (2015) include the period between announcement of Reg-SHO and its implementation in the pre-regulation period. In contrast, we separate this transition period out from the pre-regulation period based on prior Reg-SHO research that indicates that short interest changed for pilot firms relative to control firms during the transition period (e.g., Grullon et al. 2015). When we include the transition period in the pre-regulation period our results change and are consistent with those reported by Li and Zhang (2015).
[8] Details of the pilot list are available at http://www.sec.gov/rules/other/34-50104.htm.
[9] The total number of firms in our sample at this step is 2960, comparable to the 2952 identified by Fang et al. (2016).
[10] Li and Zhang (2015) and Chen et al. (2014) both use the First Call CIG database as the source for management earnings forecasts. One practical difference between I/B/E/S and CIG is that I/B/E/S provides a data item for the

(DURSHO) as the period from May 2, 2005 to July 6, 2007, when the pilot program was in place. We choose December 1st 2002 – July 28th 2004 as the pre-treatment period (PRESHO) to match approximately the length of the two periods.[11] The period between the announcement of the pilot program and its implementation (July 29th 2004 through May 1st 2005) is the transition period (TRANSITION). We also examine whether the effect of Reg-SHO on management forecasts diminishes after August 2007, when the uptick rule was also suspended for the control group. We choose the post SHO implementation period (POSTSHO) as the period from August 2007 to July 2009 for the tests of the likelihood and timing of bad news management forecasts (hypotheses 1 and 2). For tests of the delay in bad news earnings announcements (hypothesis 3) we choose POSTSHO as August 2008 to July 2010 because we require earnings announcement dates for the same quarter in the previous year under the same regulation regime to calculate our expected delay metric (see below).[12,13]

Following related research (e.g., Diether et al. 2009b; Grullon et al. 2015), firms with a price less than $1 are excluded from the sample. We further require each firm-quarter to have all

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immediately preceding consensus analyst earnings forecast associated with each management forecast, while CIG does not. Thus, using CIG requires researchers to match management forecasts to consensus analyst forecasts. This is a potential source of difference in the classification of management forecasts into good and bad news (relative to the consensus analyst forecast). To investigate the effect of this on our results, we obtained management forecasts for the PRESHO, TRANSITION, and DURSHO periods from the CIG database. There were 8,325 firm-quarters in our sample that had management forecasts in both I/B/E/S and CIG for these periods. Of those, 6.7 percent yielded different bad and good news forecast classifications between I/B/E/S and CIG. We repeated our analyses using CIG management forecasts with no material effect on our reported results.

[11] SEC announced the Reg-SHO on July 29th, 2004, while the complete management earnings forecast data is only available from December, 2002.

[12] We also reran the tests using August 2007- July 2009 as POSTSHO and found a significant difference between pilot and control firms in the delay of bad news earnings announcements. This may suggest the influence of measurement error in the “expected delay of earnings announcement”, and/or a prolonged effect of Reg-SHO on pilot firms.

[13] On September 19, 2008, the SEC imposed an emergency short selling ban on 799 financial firms. To investigate the potential effect of this on our results we excluded firm-quarters from July 2008 through June 2009 from our sample and repeated our analyses. The results were qualitatively unchanged from those we report below.

control variables available (as described in the following section). Following Anilowski, Feng, and Skinner (2007), we exclude non-EPS forecasts and require management forecasts to be made before the earnings announcement and not more than 90 days before the fiscal quarter end. To measure the analyst consensus, we use the item *Mean_at_date* (analyst forecast consensus at the time of guidance), provided by the I/B/E/S Guidance Feed database.

For tests of the timing of earnings announcements (hypothesis 3), we require each firm-quarter to have an actual quarterly earnings report date (RDQ) from COMPUSTAT for quarter *q* of year *t* and year *t-1*. Following Bagnoli et al. (2002), we eliminate earnings announcements with actual report dates more than 60 calendar days before or 90 calendar days after the expected announcement date (the earnings report date in the same quarter of the previous year). These restrictions reduce our sample by less than 1% and reduce the likelihood of data entry errors in our sample data.

[Insert Table 1 about here]

Panel B of Table 1 describes the industry membership of firms in our sample and indicates a broad cross-section of industries are represented. Only one industry represents more than 10 percent of firms in our sample (SIC code 73 Business Services at 10.4 percent of the sample).

RESEARCH DESIGN

We employ a difference-in-difference approach, using Reg-SHO as the quasi-natural experiment to identify the causal effect of the regulation's change in short selling constraints on firms' disclosure choices. Specifically, we compare the likelihood of bad news management earnings forecasts, the horizon of bad news forecasts, and the timing of bad news earnings announcements between pilot firms and control firms before, during and after the pilot program.

Measurement of disclosure properties

The likelihood of bad news management earnings forecasts

Following Anilowski et al. (2007), we classify a management earnings forecast as a bad news forecast if the forecast is less than the prevailing analysts' consensus earnings estimate.[14] The majority of our sample forecasts are issued in range and point forms. We use the mid-point of the range (for a range forecast) to measure the forecast value. For open-range management forecasts, the forecast is classified as bad news if the upper boundary is lower than the analyst consensus.[15] The analyst consensus is provided by the I/B/E/S Guidance Feed database (item *Mean_at_date*).[16] We use an indicator, *BADNEWS*, to capture whether firms disclose bad news in a firm-quarter. If managers provide at least one bad news forecast during a firm-quarter, then *BADNEWS* is one and zero otherwise.[17]

The timing of bad news management earnings forecasts

To reflect the timeliness of bad news forecasts issued by managers, we compute the forecast horizon. Following Rogers and Van Buskirk (2009), we define *MF_HORIZON_BAD* as

[14] We repeated our analyses after eliminating bad news forecasts in the top decile by magnitude of all negative management forecast errors. We also eliminated possible low ball management forecasts (defined as in Chen 2014). Our main results were qualitatively similar for these tests.
[15] Inferences are unchanged if we exclude management forecasts where the magnitude of the forecast news (scaled by stock price at the beginning of the quarter) is in the bottom quintile of the sample distribution.
[16] Using the I/B/E/S detail history file, we also computed the analyst consensus based upon individual analyst forecasts provided 90 days before the management forecast date. Using this measure of analyst consensus yields results qualitatively similar to those we report below.
[17] In our sample, 65% of management quarterly EPS forecasts are issued once in a quarter. Therefore, multiple management forecasts are a minority. Nevertheless, in untabulated robustness checks, we employed both the proportion of bad news forecasts to all forecasts in a quarter and the number of bad news forecasts in a quarter as alternative proxies for bad news. The results were qualitatively similar to those we report below.

the length of time between when the forecast was released and the end date of the fiscal period being forecasted. Increasing the horizon of a forecast is consistent with managers providing more timely information to the market, i.e., releasing the information earlier. If a firm issues multiple bad news forecasts for a fiscal quarter, we choose the first forecast to compute *MF_HORIZON_BAD*.[18]

The timeliness of bad news mandatory earnings announcements

Following previous research (e.g., Bagnoli et al. 2002; Brown, Christensen, and Elliott 2012; Givoly and Palmon 1982), we measure the timeliness of earnings announcements as $ER\_ANN\_DELAY_{iqt} = ANN\_lag_{iqt} - ANN\_Expect_{iqt}$, where ANN_lag_{iqt} is the number of trading days between the earnings announcement date for firm *i* in quarter *q* of fiscal year *t* and the fiscal end date of quarter *q*, and ANN_Expect_{iqt} is the reporting lag in days for the corresponding quarter of year *t-1*.[19]

Empirical Models

Management earnings forecasts

We employ the following regression equations to estimate the effect of Reg-SHO on the likelihood of bad news forecasts (*BADNEWS*) and the horizon of bad news forecast (*MF_HORIZON_BAD*):

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[18] We reran our tests with all bad news forecasts, or eliminated firm-quarters with multiple management forecasts. The results were qualitatively similar to those we report below.

[19] We also employed a time-series model to estimate the expected earnings announcement lag as in Brown et al. (2012). Briefly, we used the median announcement date for each firm quarter as the proxy for the expected announcement date and computed the median announcement date for the prior two, three and four years, respectively. The results were qualitatively similar to those we report below.

$$
\begin{aligned}
BADNEWS_{it} = {} & a_0 + a_1 PILOT_i + a_2 TRANSITION_t + a_3 DURSHO_t + a_4 POSTSHO_t \\
& + a_5 PILOT_i \times TRANSITION_t + a_6 PILOT_i \times DURSHO_t \\
& + a_7 PILOT_i \times POSTSHO_t + \alpha CONTROLS_{it} + \varepsilon_{it}
\end{aligned} \tag{1}
$$

$$
\begin{aligned}
MF\_HORIZON\_BAD_{it} = {} & b_0 + b_1 PILOT_i + b_2 TRANSITION_t + b_3 DURSHO_t \\
& + b_4 POSTSHO_t + b_5 PILOT_i \times TRANSITION_t + b_6 PILOT_i \times DURSHO_t \\
& + b_7 PILOT_i \times POSTSHO_t + \beta CONTROLS_{it} + \varepsilon_{it}
\end{aligned} \tag{2}
$$

where $BADNEWS_{it}$ is an indicator variable equal to one if for firm *i* there is at least one management forecast less than the consensus of analyst forecasts at the time of announcement for the quarter and zero otherwise; $MF\_HORIZON\_BAD_{it}$ is the number of days between the forecast date and the fiscal quarter being forecasted, conditional on there being a bad news forecast (*BADNEWS* =1); $PILOT_i$ is an indicator that equals 1 if firm *i* is in the pilot group and 0 otherwise; $DURSHO_t$ is an indicator that equals 1 if a quarter is during the period of the Reg-SHO program and 0 otherwise; $TRANSITION_t$ is an indicator that equals 1 if a quarter is during the transition period between the announcement and implementation of the Reg-SHO program and 0 otherwise; $POSTSHO_t$ is an indicator that equals 1 if a quarter is after the period of the Reg-SHO program and 0 otherwise; $CONTROLS_{it}$ are a set of firm and industry characteristics that may affect management forecasts (discussed below).

The coefficient estimates on $PILOT$ x $DURSHO$ in (1) and (2), a_6 and b_6, capture the causal effect of the relaxation of short selling constraints via Reg-SHO on the likelihood and horizon of bad news forecasts, respectively. Subsequent to August 2007, the Reg-SHO pilot program expired and all stocks in the Russell Index were exempted from the “price check” rule.

Therefore we expect there to be no difference in the likelihood of bad news forecasts, the bad news forecast horizon, and the delay in earnings announcements between the pilot group and control group during the post-SHO period. To test this we include an interaction term in our regressions, *PILOT* x *POSTSHO*, to capture the difference between pilot and control firms in the post-SHO period. We include industry and fiscal quarter fixed effects to allow for possible differences in disclosure incentives across firm-quarters related to industry and reporting cycle.

Following prior research, we control for several factors related to managers' incentives for voluntary disclosure. We include firm size (*SIZE*), measured as the logarithm of market value of equity (in millions of dollars) at the beginning of the relevant quarter. Lang and Lundholm (1993) and Kasznik and Lev (1995) document a positive association between firm size and the frequency of voluntary disclosure. We also consider firms' underlying performance in determining disclosure choices. Prior studies find that firms' propensity to issue bad news forecasts is associated with the underlying earnings news (e.g., Field et al. 2005; Kothari et al. 2009; Skinner 1994). To control for earnings news, we include a loss indicator, *LOSS_D*, equal to one if actual quarterly earnings is less than zero and zero otherwise, and an unexpected earnings indicator, *UE_D,* equal to one if actual earnings is greater than the analyst consensus forecast at the beginning of the quarter and zero otherwise. We also include return on assets (*ROA*) as in Miller (2002). This measure is computed as earnings before extraordinary items scaled by lagged total assets at the end of each fiscal quarter. We use the prior quarter stock return (*CRET*) as an additional (lagged) performance measure. *CRET* is computed as the 90 day value-weighted market adjusted return accumulated ending at the previous fiscal quarter-end. We also control for proprietary costs, proxied by research and development expense (*RD*) computed as research and development expenditures scaled by total assets at the beginning of the fiscal quarter.

We control for uncertainty associated with and growth of firms' operations by including return volatility (*RETVOLATILITY*) and the market-to-book ratio (*MTB*). Firms facing greater uncertainty in their future earnings realizations may discourage managers from providing earnings forecasts (e.g., Feng and Koch 2010; Graham et al. 2005; Waymire 1985). Alternatively, a volatile and fast growth business environment may motivate managers to disclose more forecasts to avoid/reduce potential litigation costs (Skinner 1994, 1997). *RETVOLATILITY* is computed as the standard deviation of daily market-adjusted returns during a fiscal quarter. *MTB* is measured as the ratio of market value of equity to book value.

We include analyst following (*LNANALYST*) and institutional ownership (*INSTITUTION*) to control for demand for credible earnings guidance from managers. Prior research finds that greater analyst following and higher institutional ownership are both associated with more disclosure (Ajinkya, Bhojraj, and Sengupta 2005; Lang and Lundholm 1996). We use a logarithmic transformation of one plus the number of analysts to capture analyst following. Institutional ownership is the percentage of aggregated institutionally owned shares over total outstanding shares for each quarter, based on data from Form 13F.[20]

Finally, prior research suggests that firms' disclosure policies tend to be "sticky"; that is, some firms consistently provide earnings guidance while others rarely do and thus prior forecast history is an important determinant of firm's disclosure policy (e.g., Bushee, Matsumoto, and

[20] Because Form 13F is provided every calendar quarter, for each firm whose fiscal quarter does not align with the calendar quarter, we round the fiscal quarter to the calendar quarter. For example, if a firm's fiscal quarter end is January 2004, the institutional holdings published in March 2004 is allocated to this firm quarter. In our sample, around 20% observations are subject to this adjustment.

Miller 2003; Graham et al. 2005; Skinner 2003). Following Field et al. (2005) and Cao and Narayanamoorthy (2011), we use an indicator variable, *MF_D*, to capture firms' past forecasting behavior. The indicator variable is equal to 1 if a management forecast was issued in the previous quarter and 0 otherwise.[21] We expect *MF_D* to be positively associated with bad news disclosure.

The timeliness of earnings announcements

We employ the following regression equation to estimate the effect of Reg-SHO on the delay in earnings announcements (*ER_ANN_DELAY*):

$$\begin{aligned} ER\_ANN\_DELAY_{it} = {} & c_0 + c_1 PILOT_i + c_2 TRANSITION_t + c_3 DURSHO_t \\ & + c_4 POSTSHO_t + c_5 PILOT_i \times TRANSITION_t + c_6 PILOT_i \times DURSHO_t \\ & + c_7 PILOT_i \times POSTSHO_t + \gamma CONTROLS_{it} + \varepsilon_{it} \end{aligned} \quad (3)$$

Where $ER\_ANN\_DELAY_{it}$ is the difference (in trading days) between the earnings announcement date and the announcement date from the corresponding quarter in the prior year. $CONTROLS_{it}$ are a set of firm and industry characteristics that prior research suggests are associated with earnings announcement timing (discussed below). Industry and fiscal quarter fixed effects are also included. The coefficient on $PILOT$ x $DURSHO$, c_6, captures the effect of Reg-SHO on the delay in earnings announcements.

Following prior research, we control for several factors that are associated with the delay in earnings announcements. Bagnoli et al. (2002) and Brown et al. (2012) document that firms with negative news are more likely to delay earnings announcements. Therefore, we control for

[21] We also defined *MF_D* equal to one if the firm issued a management forecast in at least one of the four previous quarters and zero otherwise. The results were qualitatively unchanged from those we report below.

earnings surprise, *SURPRISE* --- the difference between actual earnings and the analyst consensus immediately prior to the earnings announcement. We also include an indicator variable *NEG_SURPRISE_D* equal to 1 if *SURPRISE* is negative and 0 otherwise. Finally, we include an indicator variable *LOSS_D* equal to 1 if actual earnings is negative and 0 otherwise. We expect the coefficient on *SURPRISE* to be negative and coefficients on *NEG_SURPRISE_D* and *LOSS_D* to be positive.

In addition, we control for firm size (*SIZE*), market to book ratio (*MTB*), return on assets (*ROA*), institutional ownership (*INSTITUTION*), computed as in equations (1) and (2). Finally, we control for several other determinants of the timing of firms' earnings announcements, including earnings volatility (*STDROA*), occurrence of one-time events proxied by the reporting of special items in Compustat (*SPECIAL_D*), and an indicator for whether firms provide management earnings forecasts (*GUIDANCE_D*). *STDROA* is computed as the standard deviation of return on assets over at least three of the past eight quarters. *SPECIAL_D* is an indicator variable set equal to 1 if a firm reports non-zero special items in the current quarter and 0 otherwise. *GUIDANCE_D* is an indicator variable set equal to 1 for those firm-quarters with at least one outstanding management earnings forecasts as of the earnings announcement date and 0 otherwise.

In order to mitigate the effect of extreme observations, we winsorize all continuous variables at the first and 99^{th} percentiles.

RESULTS

Descriptive statistics

In Table 2, we provide descriptive statistics comparing the primary research variables between pilot and control firms before the announcement of Reg-SHO (Panel A) and during the implementation of Reg-SHO (Panel B). Because pilot firms are chosen by (effectively) random selection from Russell 3000 index firms there should be no systematic difference between these two groups before SHO. However, after requiring relevant data for our tests, there are several differences between pilot and control firms before the announcement of Reg-SHO. For instance, pilot firms are slightly larger, exhibit higher ROA, have lower market/accounting return volatility, are more likely to report special items, and have a longer delay in earnings announcements, relative to control firms.[22] As a result, we control for these firm-specific characteristics in our difference-in-difference analyses.

[Insert Table 2 about here]

Difference-in-Difference analyses

The effect of Reg-SHO on the likelihood of bad news management earnings forecasts

Table 3 presents regression results for the effect of Reg-SHO on the likelihood of bad news management forecast disclosure (hypothesis 1). In column (1), based on the full sample, the estimated coefficient on $PILOT$ x $DURSHO$ is 0.11 (t = 2.31) and statistically significant at the five percent level, indicating that pilot firms are more likely to issue bad news earnings forecasts during the Reg-SHO period relative to control firms. In terms of economic significance, the increase in likelihood of bad news forecast disclosure by pilot firms relative to control firms is approximately 5 percent.

[22] Systematic differences between pilot and control firms are also observed in Li and Zhang (2015) and He and Tian (2014). In addition, the SEC randomly selected pilot firms based upon dollar trading volume one year immediately prior to the SHO announcement. If we restrict our pre Reg-SHO sample period to this one year period, the differences between pilot and control groups are smaller.

As expected, the coefficient on *PILOT* column (1) is not significantly different from zero, indicating that there is no reliable evidence of a difference between pilot and control firms' disclosure of bad news management forecasts in the pre Reg-SHO period. Nor is there significant evidence of a difference in the voluntary disclosure of bad news between pilot and control firms in the post Reg-SHO period --- the coefficient on *PILOT* x *POSTSHO* in column (1) is 0.10 (t = 1.49) and not significant at conventional levels. These results reinforce the inference that the difference in managerial disclosure behavior between pilot and control firms during Reg-SHO is driven by the increase in short selling pressure resulting from the relaxation of constraints on short-selling.

[Insert Table 3 about here]

We also investigated whether the increased disclosure of bad news forecasts by pilot firms during the Reg-SHO period differs for extreme versus moderate bad news. Extreme bad news is likely to be revealed even in the absence of short selling pressure due to the associated litigation costs and other factors discussed in section 2. In this case, it is less likely that the removal of short selling constraints under Reg-SHO will have an effect on firms' disclosure of bad news. However, for less extreme or moderate bad news the removal of short selling constraints is more likely to influence firms' disclosure decisions at the margin of the cost-benefit tradeoff for voluntary disclosure. Therefore, we expect that the increased disclosure of bad news concentrates in the moderate bad news cases, and has less or no effect for extreme bad news cases. To investigate this possibility, we divided the sample of all bad news management forecasts into two groups: 'moderate bad news' forecasts, which are those where the forecast less analyst consensus is above the median (*i.e.*, lower magnitude) for all bad news forecasts, and 'extreme bad news' forecasts where the forecast less analyst consensus is below the median (*i.e.*, greater magnitude). We then re-estimated equation (1) twice: first where the left-hand side (LHS) variable is redefined to be one if the forecast is 'moderate bad news' and zero if it is not bad (column (2) of Table 3), and then where the LHS is redefined to be one if the forecast is 'extreme bad news' and zero if it is not bad (column (3) of Table 3). In each case the LHS excludes bad news forecasts of the other type: column (2) excludes extreme bad news forecasts and column (3) excludes moderate bad news forecasts.

In column (2), based on moderate bad news forecasts, the coefficient on $PILOT$ x $DURSHO$ is 0.19 and significantly different from zero (t = 2.76). In contrast, when the LHS variable is based on extreme bad news forecasts (column (3)) the coefficient is 0.06 and not significantly different from zero at conventional levels (t = 0.63). The results suggest that Reg-SHO relaxes the threshold of bad news disclosure, and encourages moderate bad news to be released to the market.[23]

We also investigated whether the difference in the disclosure of bad news forecasts between pilot and control firms during the pilot period is greater for firms facing higher potential litigation risk/costs. As discussed in section 2, prior research has suggested that litigation costs represent an important cost factor influencing firms' voluntary disclosure decisions, particularly

[23] There are also differences between the likelihood of disclosure of moderate bad news between pilot and control firms in each of the pre Reg-SHO, transition, and post Reg-SHO periods apparent in column (2) of Table 3. In the pre Reg-SHO period pilot firms disclose moderate bad news less often than control firms (the coefficient on $PILOT$ is significantly negative), but this difference disappears and becomes positive in each of the transition, during Reg-SHO, and post Reg-SHO periods. This reinforces the importance of employing a difference-in-difference research design. Interestingly, the significant and positive coefficient on $PILOT$ x $POSTSHO$ in column (2) of Table 3 suggests that the difference in voluntary disclosure between pilot and control firms persists even after the pilot period ended and all firms faced the same short-sales constraints. Because our post Reg-SHO period is relatively short, this could be due to gradual transition by control firms to the new regulation. It could also be influenced by research design factors. Specifically, the post Reg-SHO period includes the global financial crisis (GFC) of late 2008/early 2009. To explore this further we divided the post-Reg SHO period into two sub-periods: August 2007 – September 2008, and October 2008 – July 2009. The second of these corresponds to the GFC. Only in the second sub-period is there evidence of a difference between the disclosure of bad news between pilot and control firms. This suggests that the significant coefficient on $PILOT$ x $POSTSHO$ in column (2) of Table 3 is driven by failed firms during the global financial crisis, rather than short selling related differences.

regarding bad news (e.g., Field et al. 2005; Kothari et al. 2009; Skinner 1994, 1997). This suggests that for firms facing a low litigation risk environment, any effects of a change in short selling constraints will be muted since there is little scope for already low potential litigation costs to be affected. Following prior research (e.g., Field et al. 2005; Skinner 1994, 1997) we investigate this possibility by dividing sample firms by industry --- firms with Standard Industrial Classification (SIC) codes within the ranges 2833-2836, 3570-3577, 3600-3674, 5200-5960, 7371-7379 or 8371-8734 are classified as having high litigation risk/costs, otherwise they are considered to face low litigation risk/cost. Columns (4) and (5) of Table 3 present regression results for these two groups. In column (4), for firms in high litigation risk industries, the coefficient on *PILOT* x *DURSHO* is 0.21 and statistically significant at the 5% level (t =2.01), while in column (5), where litigation risk is low, the coefficient is 0.05 and statistically insignificant (t = 0.59). Thus, only for high litigation risk firms do we obtain statistically reliable evidence consistent with Reg-SHO increasing the likelihood that firms disclose bad news management earnings forecasts.(Kim and Skinner 2012)[24]

[24] We also employed two alternative metrics for litigation risk. The first metric is predicted litigation risk from Kim and Skinner (2012)'s model based on firm- and industry-specific information such as prior stock returns, return volatility and firm performance. We modified the model to estimate the perceived *ex anti* litigation risk for our sample firms (Yuan and Zhang 2014): $Prob\ (lawsuit_{i,t+1}=1) = \alpha_{i,t} + \beta_1(Return_{i,}) + \beta_2(Turnover_{i,t}) + \beta_3\,(Return\ volatility_{i,t}) + \beta_4\,(Beta_{i,t}) + \beta_5(Skewness_{i,t}) + \beta_6\,(MVE_{i,t}) + \beta_7\,(ROA_{i,t}) + \beta_8\,(Biotech_{i,t}) + \beta_9\,(Electronics_{i,t}) + \beta_{10}\,(Computer_{i,t}) + \beta_{11}\,(Retail_{i,t}) + \varepsilon_{i,t}$. The inferences from columns (4) and (5) of Tables 3 and 4 still hold at conventional significance levels using this model to divide the sample into high and low litigation risk firm-quarters. But in Table 5 (relating to the timing of earnings announcements) hypothesis 3 is not supported using this litigation risk approach. One potential explanation is that given that short selling can directly affect firms' stock returns and return volatility, predicted litigation risk using the Kim and Skinner (2012) model is endogenous with short selling activities in the prior period. Thus, it is difficult to disentangle causal effects. The second metric we employed is the ratio of total lawsuits to the number of firms in an industry (based upon the 2-digit SIC code) 5yrs immediately before Reg-SHO (Yuan and Zhang, 2014). The inferences from columns (4) and (5) in Tables 3 and 5 are unaffected by this approach. However, the litigation risk results in Table 4 no longer hold (regarding the timing of bad news forecasts).

The effect of Reg-SHO on the timing of bad news earnings forecasts

Table 4 presents results for the effect of Reg-SHO on the timing of bad news management forecasts (hypothesis 2), based on the subsample of firm-quarters where a bad news management forecast was released. The LHS variable is measured as the number of days prior to the end of the forecasted fiscal quarter that the management forecast was released. In column (2), for the full sample, the coefficient estimate on *PILOT* x *DURSHO* is 1.85 and significant at the 10% level (t = 1.86). This suggests that managers in the pilot firms issued bad news forecasts earlier than those issued by control firms during Reg-SHO, although the result is statistically weak. Regarding economic significance, the increase in the horizon difference between pilot and control firms of 1.85 days is approximately 5.5% of the mean horizon of bad news forecasts for the control firms before Reg-SHO.

[Insert Table 4 about here]

Similar to the analysis of the likelihood of bad news forecasts in the previous subsection, we separated bad news management forecasts into extreme bad news and moderate bad news forecasts. For the subsample based on moderate bad news forecasts (column (2) of Table 4) the coefficient on *PILOT* x *DURSHO* is 2.02 (t = 1.70, significant at the 10% level). In column (3), where the subsample is based on extreme bad news forecasts, the coefficient is 1.81 and not significant (t= 1.08), suggesting that the difference between pilot and control firms' forecast horizons is more pronounced for moderate bad news forecasts.

Columns (4) and (5) of Table 4 report results for the high and low litigation risk subsamples employed in the previous section. The coefficient on *PILOT* x *DURSHO* is 3.30 (t = 1.96, significant at the 5 % level) for high litigation risk companies, while for low litigation risk companies the coefficient is 0.99 and not significant (t = 0.88). Thus, consistent with the results from Table 3, the difference between pilot and control firms appears to be driven by companies that face higher litigation risks/costs.

The effect of Reg-SHO on the timing of earnings announcements

In Table 5, we report results regarding the effect of Reg-SHO on the timing of earnings announcements (hypothesis 3). Based on the full sample (column (1)), the coefficient on $PILOT$ x $DURSHO$ is -0.22 (t = -2.04, significant at the 5% level), suggesting that during the Reg-SHO pilot period managers in pilot firms move their earnings announcements forward (compared with control firms).[25] In contrast, in the post Reg-SHO period the coefficient on $PILOT$ x $POSTSHO$ is not significant (coeff = -0.08, t = -0.89), suggesting that the difference between pilot and control firms in the timing of earnings announcement disappears in the post Reg-SHO period.[26]

[Insert Table 5 about here]

The discussion surrounding hypothesis 3 in section 2 suggests that the potential effect of short selling on firms reporting of (mandatory) earnings news is likely to be concentrated in situations where the firm reports bad earnings news. To investigate this we partitioned the

[25] The coefficient on $PILOT$ x $DURSHO$ appears economically small, indicating a change in the difference between pilot and control firms of only 0.22 days, on average. As discussed in Brown et al. (2012), this result may be attributable to the substantial clustering of announcements around the expected date (*i.e.*, around $ER\_ANN\_DELAY = 0$). To address this issue, following Brown et al. (2012), we reran our test after eliminating observations with an absolute ER_ANN_DELAY of less than three days (i.e., earnings announcements that are less than three days late or less than three days early). The results in the reduced sample exhibit a larger effect for Reg-SHO – the coefficient on $PILOT$ x $DURSHO$ is -0.65 and significant at the 5% level.

[26] As in Table 4, the coefficient on $PILOT$ in column (1) of Table 5 is statistically significant (coeff = 0.17, t = 2.24). The positive coefficient indicates that, on average, prior to the Reg-SHO period pilot firms delayed their earnings results compared with control firms, consistent with their delay of management earnings forecasts relative to control firms in the pre Reg-SHO period in Table 4.

sample based on earnings news: reported earnings less than (greater than or equal to) the analyst consensus forecast at the at the beginning of the quarter is classified as bad (neutral or good) news. We expect the effect of Reg-SHO on the timing of earnings announcement to be more pronounced in the subsample where the earnings news is bad. Columns (2) and (3) of Table 5 report results weakly consistent with this expectation. For bad earnings news, the coefficient on *PILOT* x *DURSHO* is -0.31 (t = -1.76, significant at the 10% level). The coefficient is -0.14 and insignificant (t = -0.96) for good or neutral news.

Columns (4) and (5) of Table 5 present the results for the high and low litigation subsamples used in the previous sections. The coefficient on *PILOT* x *DURSHO* is -0.50 (t = -2.20), significant at the 5% level for high litigation risk firms, and -0.13 (t= -1.08), insignificant for low litigation risk firms. Thus, consistent with the results from Tables 3 and 4, the difference between pilot and control firms appears to be attributable to companies that face higher litigation risk.

Additional analyses

Firms' prior forecasting behavior

Previous research (e.g., Billings et al. 2015; Field et al. 2005; Rogers and Van Buskirk 2013) indicates that firms' prior forecasting behavior is an important factor in explaining their subsequent voluntary forecasting decisions. To investigate the role of prior forecasting behavior on our results, we divided our sample into three groups: firms which did not forecast earnings in the period prior to the announcement of the Regulation SHO pilot program; firms which issued one to three earnings forecasts in the pre-Regulation SHO period; and firms which issued greater than three earnings forecasts in the pre-Regulation SHO period. Table 6 reports the estimated coefficients and *t*-statistics associated with *PILOT* x *DURSHO* in equations (1), (2), and (3) (corresponding to the analyses in Tables 3, 4 and 5 respectively). Column (1) repeats the results from Tables 3-5 for comparison purposes, while columns (2)-(4) provide results for the three subsamples based on firms' prior forecasting behavior. Only firms that already forecasted frequently prior to Reg-SHO exhibit significant results (column (4)). This is consistent with Regulation SHO only having an impact on firms whose prior forecasting behavior suggests were on the margin in terms of the costs and benefits associated with voluntary forecasting.

[Insert Table 6 about here]

Association with stock price response to the initial announcement of Reg-SHO

Grullon et al. (2015) document a negative short-term cumulative abnormal return around the Reg-SHO announcement date for pilot firms. If Reg-SHO affects managerial disclosure behavior, then we expect the effect will be more pronounced for those firms which are most affected by the regulation, as indicated by larger stock price declines around the Reg-SHO announcement date. To investigate this possibility, we partitioned the full sample into two groups based on the 12-day cumulative abnormal return around the Reg-SHO announcement. Specifically, Reg-SHO was announced on June 28, 2004; the cumulative abnormal return, CAR[-10,+1], for each firm was computed between 10 days before and 1 day after the Reg-SHO announcement. We partitioned the sample into firms with a CAR below the median (Low CAR firms) and with a CAR above the median (High CAR firms). Low CAR firms are those with the greatest magnitude price decline and so are those expected to be most affected by the Reg-SHO provisions.[27] We repeated our analyses from Tables 3-5 for each subsample. Untabulated results

[27] The average CAR is 5.02 percent and -6.56 percent for High and Low CAR firms respectively. The average CAR for pilot firms is -1.16 percent and -0.6 percent for control firms.

indicate that in all cases only firms who experienced larger magnitude negative stock price declines around the announcement of Reg-SHO exhibit significant changes in voluntary disclosure behavior by pilot versus control firms during the Reg-SHO period.

Good news earnings forecasts

To rule out the possibility that our findings are due to a change in managerial disclosure in general (i.e., increases in the likelihood of both good news and bad news forecasts), we also investigated the effect of short selling on good news forecasts. We re-estimated equation (1) with *GOODNEWS* as the LHS variable (rather than *BADNEWS*), and equation (2) with *MF_HORIZON_GOOD* as the LHS variable (rather than *MF_HORIZON_BAD*). Untabulated results indicate there is weak evidence of a *decrease* in the likelihood of good news management forecasts for pilot firms during the Reg-SHO period. This contrasts with the increase in likelihood of bad news disclosure reported in Table 3. The results also indicate no significant effect of Reg-SHO on the timing of good news forecasts by pilot versus control firms, contrary to the results for the timing of bad news forecasts in Table 4. Thus, it is unlikely that the results reported in Tables 3 and 4 reflect a general change in the voluntary disclosure behavior of pilot firms. Instead, they relate specifically to bad news disclosure and are consistent with being driven by the changes in short selling constraints introduced by Reg-SHO.

Bundled management forecasts

Rogers and Van Buskirk (2013) show that the traditional measure of management forecast news contains error due to the bundling of current quarter earnings announcements with management forecasts for the next quarter. The majority of management forecasts in our sample are bundled with earnings announcements, leading to a potential measurement error in the traditional calculation of forecast news. Following Rogers and Van Buskirk (2013), we correct management forecast news by substituting the conditional analyst consensus forecast (to earnings announcements) for the unconditional consensus forecasts used in the traditional calculation of management forecast news. Untabulated results indicate that all of our results are robust to this redefinition of forecast news.

Correlation of management forecasts horizon and earnings announcement delay

Because the majority of management forecasts are bundled with earnings announcements, our finding of a decrease in bad news forecast horizons might be due to a reduction in delay of earnings announcements. To mitigate this possibility, we included an indicator variable in our regressions to control for this bundling effect. Following Rogers and Van Buskirk (2013), we defined the indicator variable as equal to 1 if management forecasts fall within two days of the earnings announcement date and 0 otherwise. Untabulated results indicate that all of our results are robust to this check.

Pseudo pilot firms and pseudo Reg-SHO pilot program period

Following He and Tian (2014), we ran simulations to create pseudo pilot firms and a pseudo pilot program period to address the possibility that our results may simply be due to chance. Specifically, we randomized one third of firms into a pseudo pilot group and the rest were assigned to the control group in the quarter immediately before the Reg-SHO announcement (July 2004). Using this bootstrapped sample, we estimated equations (1)-(3) and repeated the simulation 5000 times. The original coefficients on the likelihood (0.11 in Table 3) and horizon of bad news forecasts (1.85 in Table 4) as well as that of the delay of earnings announcements (-0.22 in Table 5) fall in the tails (90%, 95% and 5%, respectively) of the simulated distribution of the coefficients on $PILOT$ x $DURSHO$, which is inconsistent with our findings in Tables 3-5 being generated by chance.

To address the concern that our findings are driven by an unrecognised bias in our research design we also conducted a placebo test by taking the pilot firms identified by SEC to a "pseudo Reg-SHO" period, July 2003, that is one year before the official Reg-SHO period. Untabulated findings indicate that the coefficients on $PILOT$ x $DURSHO$ in all three models are insignificant at conventional levels, which suggests our inferences in Tables 3-5 are not driven by unobservable confounding factors within our research design.

Impact of external financing on the effect of short selling

We also investigated the potential influence of firms' external financing needs on the effect of short selling. Korajczyk, Lucas, and McDonald (1991) show that firms' earnings news contains more good news before seasoned equity offerings (SEOs) than after SEOs, which is consistent with the findings of Asquith and Mullins (1986) that there is on average a stock price run-up preceding equity issuance, and suggests that firms may withhold bad news disclosure prior to issuing equity. Grullon et al. (2015) document that pilot firms significantly reduce equity issuance during the Reg-SHO period and hence it is possible that the reduced withholding of bad news we document simply reflects the changes in the difference between pilot and control firms' equity issuance. We investigated this possibility by partitioning sample firms based on whether firms issue equity in the year subsequent to disclosure/non-disclosure. Firms were classified as equity issuers if they issued equity and non-equity issuers if they did not, as recorded in the Thomson Reuters' SDC Platinum database.

Untabulated results show that our main findings—more likely and timelier disclosure of bad news by pilot firms — hold in the subsample comprised of non-equity issuers, and suggests our inferences in Tables 3-5 are not driven by the previously documented changing trend in equity issuance.

CONCLUSION

In this study, we investigate how short selling pressure affects firms' voluntary disclosure of bad news. As informed traders, short sellers enhance the informativeness of stock prices, potentially decreasing the benefits and increasing the costs to managers from withholding and/or delaying the release of bad news. We employ a quasi-natural experiment — the relaxation of short selling constraints for a pilot group of firms under Reg-SHO — to investigate this possibility. Relative to control firms, we observe a significant increase in the likelihood of bad news management forecasts among pilot firms. In addition, pilot firms also provide these forecasts in a more timely fashion. We also find that pilot firms accelerate the release of quarterly earnings news when the news is bad. We further find that Reg-SHO lifts the threshold of bad news disclosure by encouraging more timely disclosure of moderate bad news, and that the effect of Reg-SHO on the disclosure of bad news is more significant when litigation risk is higher.

Overall, our results are consistent with results from related research (e.g., Chang et al. 2014; Fang et al. 2016; He and Tian 2014; Hope et al. 2017; Karpoff and Lou 2010; Ke et al. 2018; Massa et al. 2015b), showing that short selling has real effects on corporate decisions, and are of potential relevance to regulators in considering the likely effects of regulated short-sales constraints.

However, our results are inconsistent to some extent with some of the results reported in Li and Zhang (2015) who use a similar research design and sample. Specifically, they report that pilot firms decrease the precision of range bad news earnings forecasts and decrease the readability of bad news annual reports (compared with control firms) during the Reg-SHO period. That is, their results suggest that Reg-SHO discouraged the transparent disclosure of bad news earnings forecasts, whereas our results indicate it increased the likelihood of bad news forecast disclosure and accelerated its timing. This suggests a need for further future research to better understand this apparent inconsistency.

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Appendix 1: Variable Definitions

BADNEWS = an indicator variable equal to 1 if there is at least one management forecast less than the consensus of analyst forecasts at the time of announcement for each fiscal period and 0 otherwise;

MF_HORIZON_BAD = the number of days between the forecast date and the end date of a fiscal period being forecasted (conditional on *BADNEWS*=1);

ER_ANN_DELAY = the firm's deviation from its expected announcement date, assumed to be the same as the reporting lag in days for the corresponding quarter of year t-1;

CRET = market (value-weighted) adjusted daily returns over the previous quarter, computed at the beginning of a fiscal quarter;

GUIDANCE_D = an indicator variable equal to 1 for those firm-quarters with at least one outstanding management earnings forecast as of the earnings announcement date based on the I/B/E/S guidance feed database and 0 otherwise.

ROA = earnings before extraordinary items scaled by lagged total assets in current quarter;

RETVOLATILITY = standard deviation of market (value-weighted) adjusted daily returns over past quarter, computed at the beginning of a fiscal quarter;

RD = research and development expenditures scaled by total asset, computed at the beginning of a fiscal quarter;

INSTITUTION = percentage of institutional ownership over outstanding shares over past quarter, computed at the beginning of a fiscal quarter. If missing institution data, 0 will be assigned;

LNANALYST = logarithm of one plus the number of analyst following, computed at the beginning of a fiscal quarter;

LOSS_D = an indicator variable equal to 1 if actual EPS is negative and 0 otherwise;

MF_D = an indicator variable equal to 1 if a management forecast was issued in the previous quarter and 0 otherwise;

MTB = ratio of market value of equity to book value at the beginning of a fiscal quarter;

NEG_SURPRISE_D = an indicator variable equal to 1 if actual EPS minus analyst consensus of forecasts is negative and 0 otherwise;

SIZE = logarithm of the market value of equity at the beginning of a fiscal quarter;

SPECIAL_D = an indicator variable equal to 1 if the firm reports non-zero special items in quarter q and 0 otherwise;

STDROA = standard deviation of returns on assets over at least three of the previous eight quarters, computed at the beginning of the quarter;

SURPRISE = analyst forecast error, computed as the difference between actual EPS and analyst consensus of forecasts at the time of earnings announcement;

UE_D = an indicator variable equal to 1 if analyst consensus at the beginning of the quarter t is greater than actual earnings at the end of quarter t, and zero otherwise;.

Table 1

Panel A: Sample selection

Restrictions		Number of firms
Firms included in the Russel 3000 index in 2004		2998
Less:		
Firms not listed on NYSE, AMEX, or Nasdaq, or firms with IPOs after April 30, 2004	38	
Firms without required financial and stock price	137	
Firms with stock price less than 1 or book value of equity less than 0	16	
Final sample		2807
Pilot firms		919
Control firms		1888
Final firm-quarter observations for Hypotheses H1		55127
Final firm-quarter observations for Hypotheses H2		9687
Final firm-quarter observations for Hypotheses H3		53787

Note: The number of observations for hypothesis 1 decreases to 55104 when the logit model is estimated with quarter and industry fixed effects

Panel B: Industry distribution

Industry description	# of firms	Percentage (%)	# pilot firms	# control firms
Business Services	291	10.4	85	206
Depository Institutions	270	9.6	76	194
Chemicals and Allied Products	214	7.6	76	138
Electronic & Other Electrical Equipment	209	7.4	68	141
Holding and Other Investment Offices	152	5.4	60	92
Measuring, Photographic & Medical	145	5.2	50	95
Industrial and Commercial Machinery	134	4.8	40	94
Insurance Carriers	101	3.6	27	74
Electric, Gas and Sanitary Services	99	3.5	38	61
Communications	89	3.2	20	69
Engineering, Accounting and Research	76	2.7	25	51
Oil and Gas Extraction	72	2.6	21	51
Health Services	56	2.0	12	44
Wholesale Trade - Durable Goods	53	1.9	24	29
Miscellaneous Retail	50	1.8	18	32
Transportation Equipment	48	1.7	15	33
Food and Kindred Products	45	1.6	17	28
Security & Commodity Brokers and Dealers	37	1.3	14	23
Apparel and Accessory Stores	36	1.3	13	23
Primary Metal Industries	36	1.3	16	20
Printing, Publishing and Allied Industry	36	1.3	13	23
Eating and Drinking Places	34	1.2	11	23
Wholesale Trade - Nondurable Goods	34	1.2	13	21
Fabricated Metal Products	28	1.0	11	17
Others	462	16.5	156	306
Total	**2807**		**919**	**1888**

Table 2

Descriptive statistics

Panel A: Summary statistics for sample firm-quarters before the Reg-SHO announcement

	pilot group				*control* group				Test for difference (pilot-control)	
	# obs.	Mean	std. dev.	Median	# obs.	Mean	std. dev.	Median	t-stat	Wilcoxon z-stat
BADNEWS	4806	0.18	0.38	0.00	9898	0.18	0.38	0.00	0.10	0.10
MF_HORIZON_BAD	841	32.32	20.22	41.00	1729	33.49	19.77	42.00	-1.40	-1.76
ER_ANN_DELAY	4817	0.01	4.09	-1.00	9887	-0.15	4.24	-1.00	2.19	1.47
SIZE	4806	6.98	1.47	6.75	9898	6.93	1.48	6.70	1.89	2.41
LNANALYST	4806	1.55	0.66	1.39	9898	1.55	0.65	1.61	-0.31	-0.79
MTB	4806	3.05	3.27	2.10	9898	2.95	3.38	2.10	1.69	1.86
ROA	4806	0.01	0.04	0.01	9898	0.01	0.04	0.01	2.35	4.01
CRET	4806	0.01	0.11	0.01	9898	0.01	0.11	0.00	0.24	0.51
RETVOLATILITY	4806	0.02	0.01	0.02	9898	0.02	0.01	0.02	-2.38	-1.33
RD	4806	0.01	0.02	0.00	9898	0.01	0.02	0.00	-0.17	-0.82
INSTITUTION	4806	0.53	0.32	0.59	9898	0.52	0.33	0.59	0.89	0.19
UE_D	4806	0.41	0.49	0.00	9898	0.40	0.49	0.00	0.68	0.68
LOSS_D	4806	0.14	0.35	0.00	9898	0.15	0.36	0.00	-1.50	-1.50
MF_D	4806	0.24	0.42	0.00	9898	0.23	0.42	0.00	0.70	0.70
FE	4690	0.06	0.72	0.05	9600	0.07	0.79	0.05	-1.13	-1.49
NEG_FE_D	4817	0.28	0.45	0.00	9887	0.28	0.45	0.00	0.48	0.48
STDROA	4817	0.02	0.04	0.01	9887	0.02	0.06	0.01	-3.50	-1.83
SPECIAL_D	4817	0.99	0.08	1.00	9887	0.99	0.10	1.00	2.36	2.36
GUIDANCE_D	4817	0.29	0.45	0.00	9887	0.28	0.45	0.00	0.81	0.82

Panel B: Summary statistics for sample firm-quarters during the Reg-SHO implementation

	pilot group				*control* group				Test for difference (pilot-control)	
	# obs.	Mean	std. dev.	Median	# obs.	Mean	std. dev.	Median	t-stat	Wilcoxon z-stat
BADNEWS	6260	0.18	0.39	0.00	12746	0.17	0.38	0.00	1.74	1.74
MF_HORIZON_BAD	1147	37.15	16.15	42.00	2216	36.09	17.59	43.00	1.71	0.60
ER_ANN_DELAY	6327	0.83	4.10	0.00	12899	0.85	3.99	0.00	-0.32	0.40
SIZE	6260	7.41	1.43	7.24	12746	7.31	1.47	7.12	4.52	4.90
LNANALYST	6260	1.71	0.64	1.61	12746	1.69	0.65	1.61	1.49	1.39
MTB	6260	3.18	3.09	2.37	12746	3.28	3.44	2.39	-1.93	-0.56
ROA	6260	0.01	0.04	0.01	12746	0.01	0.04	0.01	2.77	4.01
CRET	6260	0.00	0.08	0.00	12746	0.00	0.08	0.00	-0.57	-0.80
RETVOLATILITY	6260	0.02	0.01	0.01	12746	0.02	0.01	0.01	-2.77	-2.99
RD	6260	0.01	0.02	0.00	12746	0.01	0.02	0.00	-3.28	-2.74
INSTITUTION	6260	0.62	0.34	0.71	12746	0.62	0.35	0.71	0.94	0.34
UE_D	6260	0.46	0.50	0.00	12746	0.46	0.50	0.00	0.60	0.60
LOSS_D	6260	0.12	0.32	0.00	12746	0.13	0.33	0.00	-1.52	-1.52
MF_D	6260	0.27	0.44	0.00	12746	0.26	0.44	0.00	0.59	0.59
FE	6199	0.03	0.74	0.04	12588	0.03	0.80	0.04	-0.20	-0.75
NEG_FE_D	6327	0.33	0.47	0.00	12899	0.33	0.47	0.00	0.37	0.37
STDROA	6327	0.01	0.02	0.01	12899	0.01	0.03	0.01	-2.94	-0.15
SPECIAL_D	6327	0.99	0.10	1.00	12899	0.99	0.10	1.00	0.68	0.68
GUIDANCE_D	6327	0.28	0.45	0.00	12899	0.27	0.45	0.00	0.75	0.75

The sample comprises firm-quarters before the announcement of Reg-SHO (12/01/2002 - 07/28/2004) in Panel A and during the implementation of Reg-SHO (05/02/2005-07/06/2007) in Panel B. The *pilot group* comprises Russell 3000 components that are on the pilot list of Reg-SHO in June 2004. The *control group* comprises all other firms in the Russell 3000. Mean differences from zero in variables between pilot and control groups (assumed independent samples) are tested using a two-sample t-test (unpaired) and Wilcoxon Rank sum test. Variables definitions are provided in the appendix.

Table 3

The effect of Reg-SHO on the likelihood of bad news forecasts

	Predicted sign	Full sample	Subsamples			
			Moderate bad news	Extreme bad news	High litigation risk	Low litigation risk
		(1)	(2)	(3)	(4)	(5)
Intercept		-4.08***	-4.84***	-4.22***	-4.12***	-3.89***
		(-7.04)	(-7.31)	(-6.97)	(-12.26)	(-6.89)
PILOT		-0.08	-0.11**	-0.07	-0.00	-0.10*
		(-1.61)	(-2.34)	(-0.79)	(-0.02)	(-1.68)
TRANSITION		-0.27***	-0.32***	-0.24**	-0.29***	-0.24**
		(-2.74)	(-3.02)	(-2.49)	(-3.21)	(-2.16)
DURSHO		-0.38***	-0.43***	-0.33***	-0.25***	-0.43***
		(-3.88)	(-3.93)	(-3.49)	(-2.63)	(-4.05)
POSTSHO		-0.41***	-0.37***	-0.44***	-0.42***	-0.37***
		(-3.63)	(-3.12)	(-3.66)	(-3.89)	(-3.03)
PILOT×TRANSITION		0.04	0.19***	-0.01	0.28***	-0.13***
		(0.87)	(3.81)	(-0.12)	(3.49)	(-2.79)
PILOT×DURSHO	**+ (H1)**	**0.11****	**0.19*****	**0.06**	**0.21****	**0.05**
		(2.31)	**(2.76)**	**(0.63)**	**(2.01)**	**(0.59)**
PILOT×POSTSHO		0.10	0.22***	0.02	0.28**	-0.01
		(1.49)	(2.61)	(0.22)	(2.25)	(-0.12)
SIZE		0.05**	0.15***	-0.12***	0.06*	0.04
		(2.23)	(6.02)	(-4.09)	(1.85)	(1.36)
LNANALYST		0.19***	0.15***	0.27***	0.13**	0.24***
		(6.02)	(3.23)	(6.25)	(2.13)	(5.39)
MTB		-0.01**	0.01	-0.04***	-0.02	-0.01
		(-2.02)	(0.95)	(-4.42)	(-1.41)	(-1.33)
ROA		-0.02	0.56	-0.63	1.24	-0.84
		(-0.04)	(0.64)	(-0.86)	(1.28)	(-1.36)
CRET		-0.06	0.00	-0.27	-0.30	0.14
		(-0.31)	(0.02)	(-1.31)	(-1.16)	(0.60)
RETVOLATILITY		-9.48***	-10.31***	-9.39***	-1.29	-14.43***
		(-5.43)	(-4.15)	(-4.77)	(-0.52)	(-6.76)
RD		-0.23	-0.52	1.63	1.05	-2.32
		(-0.18)	(-0.27)	(1.03)	(0.64)	(-1.16)
INSTITUTION		0.11	0.06	0.14*	0.23**	0.05
		(1.58)	(0.70)	(1.82)	(2.16)	(0.61)
LOSS_D		-0.17**	-0.94***	-0.05	-0.04	-0.26***
		(-2.48)	(-7.72)	(-0.62)	(-0.50)	(-2.94)
UE_D		1.23***	0.59***	2.16***	1.32***	1.17***
		(36.46)	(13.35)	(38.87)	(18.62)	(31.01)
MF_D		2.75***	2.74***	2.60***	2.65***	2.79***
		(21.16)	(21.18)	(18.94)	(19.29)	(20.31)
N		55104	50747	51045	14290	40814

R^2	0.34	0.33	0.35	0.31	0.34

The sample period spans the pre Reg-SHO period (*PRESHO*), the transition period (*TRANSITION*), the during Reg-SHO period (*DURSHO*), and the post Reg-SHO period (*POSTSHO*). The LHS variable is *BADNEWS,* defined as one if there is at least one management forecast less than the consensus of analyst forecasts at the time of announcement for each fiscal period and zero otherwise. The coefficients are estimated using logit with industry fixed effects and fiscal quarter fixed effects. In Columns (2) and (3) we divided the sample of all bad news management forecasts into two groups: 'moderate bad news' forecasts, which are those where the forecast less analyst consensus is above the median (*i.e.*, lower magnitude) for all bad news forecasts, and 'extreme bad news' forecasts where the forecast less analyst consensus is below the median (*i.e.*, greater magnitude). We then re-estimated the regression twice: first where the LHS variable is redefined to be one if the forecast is 'moderate bad news' and zero if it is not bad (column (2)), and then where the LHS is redefined to be one if the forecast is 'extreme bad news' and zero if it is not bad (column (3)). In each case the LHS excludes bad news forecasts of the opposite type: column (2) excludes extreme bad news forecasts from the estimation sample and column (3) excludes moderate bad news forecasts. In Columns (4) and (5) the regression is estimated separately for subsamples of firms with high litigation risk or low litigation risk. A firm is considered to have high litigation risk if its Standard Industrial Classification (SIC) codes falls within 2833-2836, 3570-3577, 3600-3674, 5200-5960, 7371-7379 or 8371-8734. Otherwise, it is considered to have low litigation risk. ***, ** and * indicate significance of coefficient at 1%, 5% and 10% levels, respectively, based on firm and year-quarter clustered standard errors. All variables are defined in the Appendix.

Table 4
The effect of Reg-SHO on the horizon of bad news forecasts

	Predicted sign	Full sample	Subsamples			
			Moderate bad news	*Extreme bad news*	*High Litigation Risk*	*Low Litigation Risk*
		(1)	(2)	(3)	(4)	(5)
Intercept		26.22***	21.14***	25.02**	28.97***	27.33***
		(11.13)	(10.63)	(2.47)	(8.31)	(8.95)
PILOT		-1.70**	-1.76	-1.69	-1.73	-1.93*
		(-2.00)	(-1.54)	(-1.51)	(-1.39)	(-1.91)
TRANSITION		-4.62	-2.27	-5.20*	-5.88**	-4.05
		(-1.64)	(-1.03)	(-1.71)	(-2.37)	(-1.27)
DURSHO		-1.95	0.43	-2.26	-2.61*	-1.80
		(-1.21)	(0.32)	(-1.15)	(-1.67)	(-0.99)
POSTSHO		-0.03	1.11	1.00	-2.78	1.32
		(-0.02)	(0.82)	(0.51)	(-1.46)	(0.83)
PILOT×TRANSITION		1.34	0.78	2.31	2.63	0.50
		(0.97)	(0.43)	(1.38)	(1.41)	(0.35)
PILOT×DURSHO	**+ (H2)**	**1.85***	**2.02***	**1.81**	**3.30****	**0.99**
		(1.86)	**(1.70)**	**(1.08)**	**(1.96)**	**(0.88)**
PILOT×POSTSHO		0.78	2.02	-0.65	3.08**	-0.27
		(0.71)	(1.52)	(-0.47)	(2.09)	(-0.22)
SIZE		0.88***	1.01***	0.40	1.28***	0.54
		(3.05)	(3.32)	(0.93)	(3.53)	(1.55)
LNANALYST		0.34	-0.86	1.17	0.25	0.55
		(0.63)	(-1.64)	(1.39)	(0.32)	(0.84)
MTB		0.28***	0.24***	0.22*	0.05	0.32***
		(3.82)	(2.86)	(1.85)	(0.30)	(3.74)
ROA		-9.86*	5.88	-11.33*	-27.24***	4.34
		(-1.80)	(0.41)	(-1.76)	(-2.65)	(0.47)
CRET		-0.80	0.88	-1.24	1.89	-2.56
		(-0.38)	(0.28)	(-0.45)	(0.70)	(-0.91)
RETVOLATILITY		-48.29	-114.37**	-14.68	-24.53	-83.92**
		(-1.45)	(-2.50)	(-0.44)	(-0.61)	(-2.20)
RD		73.42***	38.19**	74.52***	38.32*	93.03***
		(4.96)	(2.17)	(3.03)	(1.82)	(3.90)
INSTITUTION		-0.68	0.75	-1.53	-2.00*	0.39
		(-0.86)	(0.84)	(-1.57)	(-1.67)	(0.39)
LOSS_D		-5.66***	-1.97	-5.54***	-4.83***	-6.61***
		(-7.31)	(-1.37)	(-6.84)	(-4.03)	(-6.49)
UE_D		-4.91***	-3.45***	-8.18***	-4.15***	-5.21***
		(-8.26)	(-7.78)	(-8.74)	(-4.82)	(-8.90)
MF_D		6.41***	1.50*	6.66***	5.54***	6.90***
		(5.98)	(1.73)	(6.12)	(4.87)	(5.67)
N		9687	4984	5201	3424	6263
R^2		0.15	0.12	0.15	0.11	0.18

The sample period spans the pre Reg-SHO period (*PRESHO*), the transition period (*TRANSITION*), the during Reg-SHO period (*DURSHO*), and the post Reg-SHO period (*POSTSHO*). The LHS variable is *MF_HORIZON_BAD,* defined as the number of days between the forecast date and the fiscal quarter being forecasted, conditional on there being a bad news forecast. The coefficients are estimated using OLS with industry fixed effects and fiscal quarter fixed effects. In Columns (2) and (3) we divided the sample of all bad news management forecasts into two groups: 'moderate bad news' forecasts, which are those where the forecast less analyst consensus is above the median (*i.e.*, lower magnitude) for all bad news forecasts, and 'extreme bad news' forecasts where the forecast less analyst consensus is below the median (*i.e.*, greater magnitude). We then re-estimated the regression twice: first where the LHS variable is redefined to be one if the forecast is 'moderate bad news' and zero if it is not bad (column (2)), and then where the LHS is redefined to be one if the forecast is 'extreme bad news' and zero if it is not bad (column (3)). In each case the LHS excludes bad news forecasts of the opposite type: column (2) excludes extreme bad news forecasts from the estimation sample and column (3) excludes moderate bad news forecasts. In Columns (4) and (5) the regression is estimated separately for subsamples of firms with high litigation risk or low litigation risk. A firm is considered to have high litigation risk if its Standard Industrial Classification (SIC) codes falls within 2833-2836, 3570-3577, 3600-3674, 5200-5960, 7371-7379 or 8371-8734. Otherwise, it is considered to have low litigation risk. *t*-statistics are in parentheses. ***, ** and * indicate significance of coefficient at 1%, 5% and 10% levels, respectively, based on firm and year-quarter clustered standard errors. All variables are defined in the Appendix.

Table 5

The effect of Reg-SHO on the timing of earnings announcements

		Full Sample	Subsample			
			Bad news	Good or neutral news	High Litigation risk	Low Litigation risk
	Predicted sign	(1)	(2)	(3)	(4)	(5)
Intercept		-17.62	89.03	-101.80	-294.12	158.76
		(-0.07)	(0.30)	(-0.37)	(-0.85)	(0.59)
PILOT		0.17**	0.24	0.11	0.32*	0.12
		(2.24)	(1.63)	(1.23)	(1.66)	(1.63)
TRANSITION		0.46*	0.58	0.35*	0.58**	0.43
		(1.69)	(1.50)	(1.73)	(2.44)	(1.38)
DURSHO		0.91***	1.20***	0.62**	1.18***	0.82***
		(3.69)	(3.84)	(2.56)	(4.38)	(2.89)
POSTSHO		-0.27	0.00	-0.60	-0.59	-0.20
		(-0.55)	(0.01)	(-1.23)	(-1.09)	(-0.35)
PILOT×TRANSITION		-0.03	-0.13	0.05	-0.21	0.02
		(-0.24)	(-0.73)	(0.29)	(-0.84)	(0.17)
PILOT×DURSHO	**-(H3)**	**-0.22****	**-0.31***	**-0.14**	**-0.50****	**-0.13**
		(-2.04)	**(-1.76)**	**(-0.96)**	**(-2.20)**	**(-1.08)**
PILOT×POSTSHO		-0.08	-0.19	0.02	-0.09	-0.08
		(-0.89)	(-1.12)	(0.17)	(-0.31)	(-0.63)
SIZE		0.01	-0.06***	0.08***	-0.01	0.01
		(0.25)	(-2.72)	(3.06)	(-0.44)	(0.46)
SURPRISE		-0.11***	-0.13***	0.04	-0.16**	-0.09***
		(-3.70)	(-2.98)	(0.86)	(-2.21)	(-3.34)
NEG_SURPRISE_D		0.27***	0.07	0.07	0.18*	0.29***
		(5.82)	(1.20)	(0.92)	(1.67)	(6.59)
LOSS_D		0.18**	0.06	0.14	0.17	0.19**
		(2.15)	(0.58)	(0.99)	(1.18)	(2.01)
STDROA		-2.86***	-3.18***	-2.67***	-2.16**	-3.21***
		(-5.11)	(-2.96)	(-5.18)	(-2.34)	(-4.75)
SPECIAL_D		0.28	0.15	0.43	0.25	0.29
		(1.37)	(0.69)	(1.63)	(0.63)	(1.30)
MTB		-0.03***	-0.04***	-0.02**	-0.05***	-0.02***
		(-4.12)	(-3.32)	(-2.50)	(-2.63)	(-3.53)
ROA		-2.91***	-2.79***	-2.22	-0.34	-4.43***
		(-3.32)	(-2.94)	(-1.53)	(-0.25)	(-5.00)
INSTITUTION		-0.02	-0.04	-0.00	-0.04	-0.02
		(-0.22)	(-0.32)	(-0.02)	(-0.24)	(-0.18)
GUIDANCE_D		0.11**	0.16*	0.05	0.04	0.13**
		(2.03)	(1.66)	(1.21)	(0.46)	(2.10)
N		53787	24229	29558	12674	41113
R^2		0.02	0.03	0.02	0.03	0.02

The sample period spans the pre Reg-SHO period (*PRESHO*), the transition period (*TRANSITION*), the during Reg-SHO period (*DURSHO*), and the post Reg-SHO period (*POSTSHO*). The LHS variable is *ER_ANN_DELAY*, the firm's deviation from its expected earnings announcement lag (assumed to be the same as the reporting lag in trading days for the corresponding quarter of year t-1). The regression is estimated using OLS with industry fixed effects and fiscal quarter fixed effected included. In columns 2 and 3 the sample is partitioned into bad and non-bad earnings news, the difference between actual EPS and the analyst consensus at the beginning of the quarter. In columns 4 and 5 the regression is estimated separately for subsamples of firms with high litigation risk or low litigation risk. A firm is considered to have high litigation risk if its Standard Industrial Classification (SIC) codes falls within 2833-2836, 3570-3577, 3600-3674, 5200-5960, 7371-7379 or 8371-8734. Otherwise it is considered to have low litigation risk. *t*-statistics are in parentheses. ***, ** and * indicate significance of coefficient at 1%, 5% and 10% levels, respectively, based on firm and year-quarter clustered standard errors. All variables are defined in the Appendix.

Table 6

The impact of firms' voluntary disclosure behaviour on the effect of Reg-SHO on likelihood of bad news forecasts, horizon of bad news forecasts, and timing of earnings announcements

	Full sample	No management forecast in the Pre-SHO period	1-3 management forecast in the Pre-SHO period	4-7 management forecast in the Pre-SHO period
	(1)	(2)	(3)	(4)
Likelihood of bad news forecast regression	0.11 (2.28)	-0.13 (-0.75)	-0.09 (-0.59)	**0.25** **(2.11)**
Forecast horizon regression	1.85 (1.86)	2.02 (1.45)	-0.22 (-0.17)	**2.92** **(2.20)**
Timing of earnings announcement regression	-0.22 (-2.04)	-0.16 (-0.93)	0.04 (0.18)	**-0.60** **(-2.47)**

The sample period spans the pre Reg-SHO period (*PRESHO*), the transition period (*TRANSITION*), the during Reg-SHO period (*DURSHO*), and the post Reg-SHO period (*POSTSHO*). We divided our sample into three groups: firms which did not forecast earnings in the period prior to the announcement of the Regulation SHO pilot program; firms which issued less than or equal to three earnings forecasts in the pre-Regulation SHO period; and firms which issued greater than three earnings forecasts in the pre-Regulation SHO period. The table reports estimated coefficients and *t*-statistics associated with *PILOT* x *DURSHO* in equations (1), (2), and (3) (corresponding to the analyses in tables 3, 4 and 5 respectively). Column (1) repeats the results from tables 3-5 for comparison purposes, while columns (2)-(4) provide results for the three subsamples based on firms' prior forecasting behavior. Control variables are included (but not reported) as in the equations (1), (2) and (3), respectively. Industry fixed effect and fiscal quarter fixed effects are included in all models. *T*-statistics are in parentheses and are based on firm and year-quarter clustered standard errors. All variables are defined in the Appendix.