Short Selling Threats and Non-GAAP Reporting:

Evidence from a Natural Experiment

by

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ABSTRACT

This study examines how short selling threats affect firms' non-generally accepted accounting principles (non-GAAP) reporting quality. From 2005 to 2007, the SEC implemented a Pilot Program under Regulation SHO, in which one-third of the Russell 3000 index stocks were randomly chosen as pilot stocks and exempted from short-sale price tests. As a result, short selling threats increased considerably for pilot stocks. Using difference-in-differences tests, I find that pilot firms respond to the increased short selling threats by reducing the use of low-quality non-GAAP exclusions, resulting in an improvement in the quality of overall non-GAAP exclusions. Further tests show that this effect of short selling threats is more pronounced for smaller firms, firms with lower institutional ownership, firms with lower analyst coverage, and firms with lower ratios of fundamental value to market value. These findings suggest short sellers play an important monitoring role in disciplining managers, as evidenced by the non-GAAP reporting choices of managers.

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CHAPTER 1

INTRODUCTION

When announcing quarterly earnings, managers may decide to disclose non-GAAP earnings along with generally accepted accounting principles (GAAP) earnings to provide additional information to capital market participants. By definition, non-GAAP earnings are prepared without being in accordance with generally accepted accounting principles.[1] Generally, managers decide to disclose non-GAAP earnings when GAAP figures fail to accurately reflect the operations and/or performance of their firms. Non-GAAP earnings usually exclude one or more income statement items, referred to as "non-GAAP exclusions," which managers consider to be transitory or less representative of core operations. Prior studies evaluate the quality of non-GAAP exclusions, where exclusions are considered to be of high quality when they are not associated with future operating performance (e.g., Doyle, Lundholm, and Soliman, 2003; Gu and Chen, 2004; Kolev, Marquardt, and McVay, 2008; Black and Christensen, 2009). Thus, exclusions of transitory (recurring) items are of high (low) quality, and high-quality (low-quality) exclusions result in high-quality (low-quality) non-GAAP earnings.[2] Previous research also documents that managers may use their discretion in preparing non-GAAP earnings opportunistically. Specifically, managers may exclude recurring expenses from the calculation of non-GAAP earnings to portray more favorable firm performance to investors (e.g., Bhattacharya, Black, Christensen, and Larson, 2003; Doyle et al., 2003; Barth, Gow, and Taylor, 2012).

[1] Manager-reported non-GAAP earnings are also often referred to as "pro forma" or "core" earnings. Analysts also provide forecasted non-GAAP earnings, frequently referred to as "Street" earnings.

[2] More recent studies refer to the exclusions of recurring items as "low-quality" or "aggressive" (e.g., Brown, Christensen, Elliott, and Mergenthaler, 2012; Black, Black, Christensen, and Heninger, 2012; Curtis, McVay, and Whipple, 2014).

There is extensive evidence suggesting that short sellers play an important monitoring role in capital markets (e.g., Desai, Krishnamurthy, and Venkataraman, 2006; Karpoff and Lou, 2010). With respect to non-GAAP reporting, Christensen, Drake, and Thornock (2014) document that short sellers analyze managers' non-GAAP reporting behavior and target firms with low-quality non-GAAP exclusions. This is because less sophisticated investors fail to understand the implications of recurring exclusions for future performance and overvalue firms with low-quality non-GAAP exclusions. Thus, by targeting such firms, short sellers can profit from subsequent price declines. In this study, I investigate the monitoring effect that short sellers exert on managers when managers are preparing non-GAAP figures. More specifically, I examine whether the threats of short selling curb managers' behavior to make aggressive non-GAAP exclusions and therefore improve the quality of non-GAAP reporting. To study these issues, I utilize a regulatory experiment.

On June 23, 2004, the Securities and Exchange Commission (SEC) adopted Regulation SHO (Reg SHO) to govern the short selling activities in the U.S. stock markets. Rule 202T of Reg SHO created a Pilot Program, which temporarily suspended short-sale price tests (the uptick test for the NYSE and the bid price test for the NASDAQ) for a list of pilot stocks from May 2, 2005 to August 6, 2007.[3] To select pilot stocks, the SEC ranked Russell 3000 stocks by average daily trading volume and designated every third stock as a

[3] The Pilot Program was initially planned to become effective on January 3, 2005 (Securities Exchange Act Release No. 50104, July 28, 2004). However, the Pilot Program was postponed until May 2, 2005 in response to comments received by the SEC (Securities and Exchange Act Release No. 50747, November 29, 2004). On July 6, 2007, the SEC removed short-sale price tests for all stocks (Securities Exchange Act of 1934 Release No. 34-55970, July 3, 2007), effectively ending the Pilot Program.

pilot. The purpose of the Pilot Program was to evaluate the effectiveness and necessity of short-sale price tests.

In this study, I employ the Pilot Program to examine the effect of short selling threats on the quality of firms' non-GAAP reporting. The Pilot Program provides a unique and suitable setting to study this effect. First, due to the suspension of short-sale price tests, the short selling constraints on pilot firms decreased considerably relative to those on control firms (SEC, 2007; Diether, Lee, and Werner, 2009). Therefore, the Pilot Program creates variation in short selling threats between pilot and control firms. Second, since firms had no influence over the passage of Reg SHO or the selection of pilot stocks, the Pilot Program represents an exogenous shock to the short selling constraints of pilot firms and thus can provide insights on causal relations. Finally, there are specific announcement, beginning, and ending dates for the Pilot Program, facilitating an empirical implementation of difference-in-differences tests. For these tests, the pilot firms are assigned to the treatment group and the remaining firms in the Russell 3000 index are assigned to the control group.

Both practitioner and academic evidence indicates that managers are aware of, and sensitive to, the effect of removing short-sale price tests on the amount of short selling activities in their firms (Opinion Research Corporation, 2008). I predict that in response to the increased short selling threats, managers of pilot firms reduce the use of low-quality non-GAAP exclusions and therefore improve the quality of overall exclusions. This is because prior research documents that short selling can trigger rigorous external scrutiny, increase delisting risk, and help detect financial misconduct in firms (Desai, Ramesh, Thiagarajan, and Balachandran, 2002; Karpoff and Lou, 2010). These findings suggest that

the expected costs of low-quality non-GAAP exclusions increase with an increase in short selling threats.[4]

The paper closest to my study is Christensen et al. (2014) who examine whether short selling volume is associated with the quality of non-GAAP exclusions, during the sample period 2005-2006. There are at least two major differences between my study and Christensen et al. (2014). First, the research question is inherently different. Christensen et al. (2014) examine whether non-GAAP exclusions that have already been made attract the attention of short sellers (proxied by short selling volume) *ex post*, while my study investigates whether the *threats* of short selling *ex ante* curb managers' behavior to make low-quality non-GAAP exclusions. Second, the research design differs considerably. Short selling volume used in Christensen et al. (2014) is determined endogenously. Therefore, it is difficult to consider causality because the association between short selling volume and the quality of non-GAAP exclusions may be driven by correlated omitted variables. I utilize the exogenous shock provided by the Pilot Program and perform difference-in-differences analyses to make causal inferences about the relation between short selling threats and the quality of non-GAAP exclusions.

In line with related studies, I estimate the association between non-GAAP exclusions and firms' future performance (measured as future operating earnings and future operating cash flows) to assess the quality of exclusions (e.g., Doyle et al., 2003; Kolev et al., 2008; Bentley, Christensen, Gee, and Whipple, 2018). High-quality exclusions are

[4] It is worth noting that if pilot firms respond to the increased short selling threats by discontinuing non-GAAP earnings disclosures, my empirical analysis may suffer from self-selection bias. However, as discussed in greater detail in Section 4.1, I find no evidence that pilot firms halt non-GAAP reporting due to the Pilot Program.

transitory in nature and therefore have no predictive ability for firms' future performance. In other words, they are the appropriate items to exclude when calculating non-GAAP earnings. Low-quality exclusions are recurring and should be included for calculating non-GAAP earnings. Using a difference-in-differences regression model, I find that the quality of non-GAAP exclusions is significantly improved in pilot firms relative to control firms after the implementation of the Pilot Program. Specifically, prior to the Pilot Program, one dollar of expense exclusions in the current quarter is associated with 77 cents of expenses over the next four quarters for both pilot and control firms (indicating recurring expenses). During the Pilot Program, non-GAAP exclusions of pilot firms are no longer associated with future performance, while exclusions of control firms are still strongly negatively related to performance over the next four quarters. The results are consistent with my hypothesis and indicate that pilot firms reduce the use of low-quality non-GAAP exclusions relative to control firms as a result of the Pilot Program.

To provide further insights, I conduct cross-sectional tests. Short sellers act as monitoring agents and discourage managers from engaging in aggressive non-GAAP reporting. However, this monitoring effect of short sellers will have a smaller impact on firms that are already closely monitored by other parties. As a result, I expect that the effect of short selling threats is more pronounced in firms with weaker *ex ante* monitoring. Prior studies show that larger firms have stronger monitoring, and that institutional investors and financial analysts serve a monitoring role on managers' reporting behavior and disclosure decisions (e.g., Shleifer and Vishny, 1986; Bushee, 1998; Chen, Harford, and Li, 2007; Armstrong, Guay, and Weber, 2010; Cheng, Huang, Li, and Lobo, 2010). Therefore, I use firm size, institutional ownership, and analyst coverage to measure *ex ante* monitoring

strength. As expected, I find that the effect of short selling threats on the quality of non-GAAP exclusions is more pronounced for smaller firms, firms with lower institutional ownership, and firms with lower analyst coverage.

Dechow, Hutton, and Meulbroek (2001) provide evidence that short sellers are more likely to take positions in firms with low ratios of fundamental value (e.g., earnings, book value, and cash flows) to market value. Therefore, I expect the effect of increased short selling threats induced by the Pilot Program to be more pronounced in such firms. Using earnings-to-price ratio, book-to-market ratio, and cash flow-to-price ratio, I find that the effect of short selling threats on the quality of non-GAAP exclusions is driven by firms with lower ratios of fundamental value to market value, consistent with my expectations.

I conduct two supplementary analyses. First, I examine the type of items excluded from non-GAAP calculations. I find that the exclusion of recurring items, specifically stock compensation expense and amortization, does not change significantly for control firms with the implementation of the Pilot Program, but decreases significantly for pilot firms. I also find that the exclusion of transitory items does not change significantly for control firms, but increases significantly for pilot firms with the implementation of the Pilot Program. These findings suggest that, as a result of the Pilot Program, pilot firms reduce their use of non-GAAP exclusions of recurring items and increase their use of non-GAAP exclusions of transitory items, providing additional evidence on the effect of increased short selling threats on the quality of non-GAAP exclusions.

Second, Fang, Huang, and Karpoff (2016) provide evidence that pilot firms reduce their earnings management of GAAP earnings during the Pilot Program. Hence, my results of an increase in non-GAAP reporting quality for pilot firms during the Pilot Program may

be due (in part) to an increase in the quality of the underlying GAAP earnings from less earnings management. To test this alternative explanation, I include total accruals and abnormal accruals, commonly used to measure earnings management (e.g., Jones, 1991; Dechow, Sloan, and Sweeney, 1995; Klein, 2002; Cheng and Warfield, 2005; Francis, Michas, and Seavey, 2013; Fang et al., 2016), as additional control variables in regression analyses. The inclusion of these additional control variables does not affect my results, indicating that my main findings of an improvement in the quality of non-GAAP exclusions for pilot firms as a result of increased short selling threats are not driven by an improvement in the quality of the underlying GAAP earnings for these firms.

This study makes the following contributions to the literature. First, the study shows that an increase in short selling threats can improve firms' non-GAAP reporting quality. This result extends the emerging literature on the real effects of secondary financial markets by revealing one channel through which firms' voluntary disclosure decisions can be affected.[5] Second, the findings of this study identify short selling threats as a factor that has a positive influence over the quality of non-GAAP exclusions and thus the quality of non-GAAP earnings, adding to the literature on the determinants of non-GAAP reporting quality. Third, the evidence in this study promotes the understanding on the benefits and drawbacks of short selling. Prior research shows that short sellers identify and target firms manipulating earnings, leading to accelerated discovery of financial misconduct (Dechow, Sloan, and Sweeney, 1996; Christophe, Ferri, and Angel, 2004; Desai et al., 2006; Karpoff

[5] See Bond, Edmans, and Goldstein (2012) for a review of research on the potential real effects of secondary financial markets. For example, Karpoff and Rice (1989) investigate the effect of stock liquidity on firm performance. Fang, Huang, and Karpoff (2016) examine the effect of short selling prospects on earnings management. De Angelis, Grullon, and Michenaud (2017) examine the effect of short selling pressure on incentive contract design.

and Lou, 2010).[6] My results demonstrate that short selling threats provide additional benefits to shareholders and potential investors by reducing the use of low-quality exclusions and thus improving the quality of non-GAAP reporting. Finally, the study complements and extends findings on the effect of Reg SHO. Recent research investigates the effects of Reg SHO on short selling activities and market quality (Diether et al., 2009), corporate equity issuance and investment decisions (Grullon, Michenaud, and Weston, 2015), management forecast precision (Li and Zhang, 2015), earnings management (Fang et al., 2016), the design of executive incentive contracts (De Angelis, Grullon, and Michenaud, 2017), and audit fees (Hope, Hu, and Zhao, 2017). This study contributes to this line of literature by examining the effect of Reg SHO on non-GAAP reporting quality.

The rest of the study proceeds as follows. Section 2 provides background information on Reg SHO and lays out the hypothesis development. Section 3 discusses sample selection, descriptive statistics, and variable measurement. Section 4 describes the research design and empirical results. Section 5 discusses the cross-sectional tests. Section 6 provides robustness checks and additional analyses. Section 7 concludes.

[6] Other studies document the potential detriment of short selling, as manipulative short selling may reduce price efficiency (e.g., Gerard and Nanda, 1993; Henry and Koski, 2010).

CHAPTER 2

BACKGROUND AND HYPOTHESIS DEVELOPMENT

2.1 *Short-Sale Price Tests and Reg SHO Pilot Program*

The SEC first adopted short-sale price tests in the 1930s during the Great Depression in order to prevent bear raids by short sellers. The NYSE used an uptick rule, which only allowed short sales if the price was above the most recently traded price (i.e., plus tick) or if the price equaled the most recently traded price when that price was higher than the last different price (i.e., zero-plus tick).[7] NASDAQ used a bid price test under Rule 3350 to decide whether a short sale was allowed. If the current inside bid price was at or below the previous inside bid price, Rule 3350 required the short sale price to be at least one penny above the current inside bid price.

In July 2004, the SEC established a Pilot Program under Rule 202T of Reg SHO in order to study the effectiveness and necessity of short-sale price tests. The SEC ranked stocks in the Russell 3000 index by average daily trading volume and designated every third stock as a pilot. These stocks were selected to represent a wide cross-section of the stock market. On May 2, 2005, the pilot stocks started to trade without short-sale price tests. The Pilot Program was originally scheduled to end on August 6, 2007. However, the SEC removed short-sale price tests for all exchange-listed stocks one month early on July 6, 2007.

Various market participants criticized the SEC's decision to eliminate all short-sale price tests. A study conducted by Opinion Research Corporation (2008) for NYSE Euronext showed that 85% of the surveyed managers preferred re-establishing the short-

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

sale price tests as soon as possible. This survey finding indicates that managers understood and feared the effect of eliminating short-sale price tests on future short selling activities in their firms' shares. Muriel Siebert, the former state banking superintendent of New York, believed that the removal of the short-sale price tests may exacerbate the downdrafts of the financial market (Morgenson, 2007). The *Wall Street Journal* argued that the SEC's decision was premature and subjective (Pozen and Bar-Yam, 2008). In response to all the critiques, the SEC adopted a new short-sale price test (commonly referred to as the "alternative uptick rule") in February 2010. The alternative uptick rule is triggered if the price of a stock drops more than 10% in one day, compared to the closing price on the preceding day.

2.2 *Literature Review and Hypothesis Development*

The voluntary disclosure of manager-adjusted non-GAAP earnings has increased greatly over the past two decades (Black, Christensen, Ciesielski, and Whipple, 2018). Standard setters raise concerns about the usefulness of performance metrics that are not in compliance with GAAP. Abarbanell and Lehavy (2007) provide evidence that it may be premature to draw the conclusion that analysts' Street earnings are more informative than GAAP earnings and that the market generally fixates on inflated earnings. Bhattacharya, Black, Christensen, and Mergenthaler (2007) show that if pro forma earnings are used opportunistically, then less sophisticated individual investors are more likely to be misled. Baik, Farber, and Petroni (2009) find that analysts are more likely to exclude expense items when providing Street earnings for glamour stocks than for value stocks, which is consistent with their incentives to promote stocks exhibiting glamour characteristics. By examining S&P core earnings measure, Albring, Caban-Garcia, and Reck (2010) show that

explicitly defined non-GAAP earnings are more strongly associated with stock price and returns than GAAP operating earnings, implying that such explicitly defined non-GAAP measure is more value relevant. Chen (2010) finds that both analysts and investors underestimate the persistence of non-GAAP exclusions. Christensen, Merkley, Tucker, and Venkataraman (2011) examine how earnings guidance provided by managers influences analysts' Street earnings. They find that analysts' decisions to exclude certain items are actively influenced by managers' earnings guidance. Frankel, McVay, and Soliman (2011) find that firms with less independent boards are more likely to report non-GAAP earnings opportunistically and often exclude recurring items from non-GAAP earnings numbers. Hsu (2011) provides evidence that the market tends to overprice special items that are included in pro forma earnings, suggesting that such special items are less persistent than the market believes. Seetharaman, Wang, and Zhang (2014) demonstrate that accounting experts are associated with high-quality non-GAAP disclosure by providing evidence that non-GAAP earnings exclusions decline after accounting experts are appointed to audit committees. Huang and Skantz (2016) find that both pro forma earnings issued by managers and Street earnings provided by analysts improve price discovery. Black, Christensen, Joo, and Schmardebeck (2017) examine the relation between real earnings management, accruals management, and non-GAAP reporting. They find that when real and accruals management are used to meet expectations, non-GAAP earnings metric is less likely to be reported. When expectations are missed after GAAP earnings management, managers are more likely to report non-GAAP earnings. Black, Christensen, Kiosse, and Steffen (2017) show that although regulations, such as the Sarbanes-Oxley Act of 2002 (SOX) and Regulation G, have generally curbed opportunistic non-GAAP reporting, some

firms choose to continue disclosing non-GAAP earnings that could be misleading to investors in the post-SOX period. Bond, Czernkowski, Lee, and Loyeung (2017) find that Regulation G and Compliance and Disclosure Interpretations (C&DIs) in 2010 are both associated with an increase in non-GAAP reporting quality. Regulation G increases the earnings response coefficients (ERCs), while C&DIs decrease the ERCs. Leung and Veenman (2018) provide evidence that non-GAAP earnings disclosed by loss firms are highly predictive of future firm performance and therefore can offset the low informativeness of GAAP losses. Kyung, Lee, and Marquardt (2019) examine how voluntary adoption of clawback provisions influence non-GAAP disclosure. They find that after clawback adoption, non-GAAP reporting frequency increases and non-GAAP exclusion quality decreases.

Non-GAAP earnings numbers are generally higher than their GAAP counterparts because managers typically exclude expenses when calculating the adjusted earnings metrics (Black and Christensen, 2009). Managers claim that the purpose of non-GAAP disclosures is to better inform investors about the continuing operations of firms. Previous research shows that on average, investors find non-GAAP disclosures to be informative and pay more attention to non-GAAP earnings than to GAAP earnings (Bradshaw and Sloan, 2002; Bhattacharya et al., 2003; Lougee and Marquardt, 2004; Bradshaw, Christensen, Gee, and Whipple, 2018). However, there is also evidence that managers sometimes employ non-GAAP reporting opportunistically. For example, Doyle et al. (2003) find that expenses excluded from non-GAAP earnings have predictive power for future cash flows, suggesting that these expenses are recurring. They also find that investors fail to fully understand the implications of excluded items for future performance. Bowen,

Davis, and Matsumoto (2005) show that managers strategically emphasize non-GAAP earnings in earnings press releases to portray more favorable operation results. McVay (2006) provides evidence that managers shift recurring expenses (e.g., normal severance fees) into special items (e.g., restructuring charges) to increase non-GAAP earnings numbers. Using hand-collected data, Black and Christensen (2009) find that managers often exclude recurring expenses such as depreciation, R&D, and stock-based compensation to meet earnings targets. Barth et al. (2012) also provide evidence that managers exclude recurring items to increase and smooth earnings. Doyle, Jennings, and Soliman (2013) demonstrate that managers use non-GAAP earnings metrics to meet or beat analyst forecasts when it is costly to use accrual earnings management.

Even though opportunistic non-GAAP disclosures provide benefits to firms, managers cannot manipulate non-GAAP earnings with impunity. Christensen et al. (2014) show that short sellers are highly active in shorting stocks of firms that exclude recurring items when calculating non-GAAP earnings as if these adjustments are indicative of poor future performance. This finding suggests that for a given level of recurring non-GAAP exclusions, managers' costs increase with a decrease in short selling constraints. Desai et al. (2002) show that short selling can trigger rigorous external scrutiny and increase firm delisting risk. Desai et al. (2006) find that short sellers monitor firms' reporting behavior and uncover aggressive earnings management. Karpoff and Lou (2010) provide evidence that short selling can help detecting financial misconduct in firms. Thus, the costs of making low-quality non-GAAP exclusions increase with an increase in short selling threats.

The Pilot Program of Reg SHO eliminated short-sale price tests for pilot firms, resulting in an exogenous decrease in short selling constraints. Therefore, short selling

threats in pilot firms increased, leading to an increase in the expected costs of using low-quality non-GAAP exclusions. Thus, I conjecture that managers of pilot firms, in response to the increased expected costs of using low-quality exclusions, reduce the level of their recurring non-GAAP exclusions and thus increase the quality of their overall exclusions during the Pilot Program. The above discussion leads to my hypothesis, stated in the alternative form:

Hypothesis: The quality of non-GAAP exclusions increases in pilot firms relative to control firms during the Pilot Program of Reg SHO.

This study is related to the growing literature in finance and accounting that utilizes changes in short selling regulations to investigate the economic implications of short selling. Several papers examine the effects of a 2008 short-sales ban on the U.S. stock market (Autore, Billingsley, and Kovacs, 2011; Frino, Lecce, and Lepone, 2011; Boehmer, Jones, and Zhang, 2013), as well as short-selling bans around the world (Beber and Pagano, 2013). These studies provide evidence that short selling restrictions generally decrease market quality.

Using the Pilot Program of Reg SHO, Alexander and Peterson (2008) find that the elimination of short-sale price tests benefits traders by allowing them to place orders that receive faster execution. Diether et al. (2009) find that short selling activities and short sales as a fraction of share volume increase for both NYSE and NASDAQ listed pilot stocks relative to control stocks. Kecskés, Mansi, Zhang (2013) investigate whether short sellers in the equity market provide information to investors in the bond market and exploit the Pilot Program to establish causality. Grullon et al. (2015) examine the impact of the Pilot Program on pilot firms' stock prices, equity issuance and investment decisions. They find that the increase in short selling activities causes stock prices to decline, and that small

pilot firms react to lower prices by reducing equity issuance and investment. Li and Zhang (2015) provide evidence that managers respond to the increased short selling pressure and subsequent price sensitivity to bad news by reducing the precision of bad news forecasts. Fang et al. (2016) study earnings management and find that discretionary accruals and the likelihood of meeting or beating analyst consensus forecast decrease for pilot firms during the period of the Pilot Program. De Angelis et al. (2017) show that the reduction of short selling constraints causes pilot firms to grant relatively more stock options to their managers. Hope et al. (2017) find that auditors react to the increased short selling threats by charging higher audit fees to pilot firms.

In my empirical analysis, I use the Pilot Program experiment to study the effect of short selling threats on the quality of firms' non-GAAP exclusions. This experiment facilitates a difference-in-differences comparison of pilot and control firms' non-GAAP reporting before and after the implementation of the Pilot Program. The difference-in-differences tests allow me to control for possible common time trends for both pilot and control firms. Moreover, the exogenous change in short selling threats facilitates important causal inferences.

CHAPTER 3

DATA AND VARIABLE MEASUREMENT

3.1 *Sample Construction*

I obtain the list of pilot and control firms from Li and Zhang (2015). Following Diether et al. (2009), Li and Zhang (2015) use the 2004 and 2005 versions of the Russell 3000 index to construct the initial sample. Specifically, only firms that were in the Russell 3000 index in both 2004 and 2005 are included. This list is then merged with the list of pilot stocks issued by the SEC, resulting in 876 pilot firms and 1,757 control firms. Among those firms, 864 pilot firms and 1,740 control firms have financial data available in Compustat North America. I obtain non-GAAP earnings data from Bentley et al. (2018), analyst forecasts data from I/B/E/S, and institutional holdings data from Thomson's CDA/Spectrum database.

The sample period in the study comprises of twelve calendar quarters: 2003 Q1 to 2004 Q2 as the pre period and 2005 Q2 to 2006 Q3 as the post period. I omit the transition period from 2004 Q3 to 2005 Q1 since the SEC announced the list of pilot stocks on July 28, 2004. The sample period starts from 2003 Q1 because the non-GAAP earnings data are available from the first quarter of 2003. I set the pre and post periods to the same duration of six calendar quarters each to have a balanced sample for the difference-in-differences test. After including observations with non-GAAP earnings data and removing observations with missing data for control variables, the final sample consists of 1,759 firm-quarter observations for 295 pilot firms and 3,515 firm-quarter observations for 601 control firms. Table 1 describes the process of constructing the final sample.

3.2 *Variable Measurement*

Consistent with prior research (e.g., Bentley et al., 2018), I measure firms' future performance (*Future Operating Performance*) as operating earnings (*Future Operating Earnings*) or operating cash flows (*Future Operating Cash Flows*) summed over quarters q+1 through q+4, divided by total assets in quarter q. Operating earnings and operating cash flows are chosen because they are the performance measures that best approximate the concept of core earnings.

Non-GAAP exclusions (*Exclusions*) are computed as non-GAAP earnings per share reported by managers less bottom-line diluted GAAP earnings per share, multiplied by the number of diluted shares outstanding and divided by total assets in quarter q. When non-GAAP earnings are higher than GAAP earnings, *Exclusions* are positive, suggesting that the exclusions of expenses outweigh the exclusions of revenues. *Non-GAAP Earnings* are non-GAAP earnings per share reported by managers multiplied by the number of diluted shares outstanding, divided by total assets in quarter q.

I create an indicator variable *Pilot*, which equals one if a firm's stock is selected as a pilot stock and zero otherwise. The indicator variable *Post* is constructed to denote the period of the Pilot Program. Specifically, *Post* equals one if the observation falls between 2005 Q2 and 2006 Q3 (inclusive) and zero otherwise.

Following the literature on non-GAAP reporting quality, I include a number of firm characteristics as control variables. These control variables are *Sales Growth* (sales in quarter q less sales in quarter q–4, divided by total assets in quarter q), *Size* (natural log of total assets), *Earnings Volatility* (standard deviation of ROA over the preceding eight quarters), *Loss* (an indicator variable which equals one if bottom-line diluted GAAP

earnings per share is negative; zero otherwise), and *BM* (book-to-market ratio). Prior research finds that the costs of opportunistic financial reporting behavior increase with firm size, as shareholders are more likely to sue larger firms (e.g., Francis, Philbrick, and Schipper, 1994). Therefore, *Size* is included in the model. A subset of firms, such as loss firms, firms with high growth, or firms with high earnings volatility, may have less persistent earnings, which may affect their non-GAAP exclusions (e.g., Dichev and Tang, 2008). Thus, I include *Loss*, *Sales Growth*, *BM*, and *Earnings Volatility* in the model.[8] Appendix A provides detailed variable definitions.

3.3 *Descriptive Statistics*

Table 2 presents descriptive statistics for the analysis variables measured before (Panel A) and after (Panel B) the implementation of the Pilot Program. As discussed, the pilot firms were randomly selected by the SEC from the Russell 3000 index. Therefore, I expect no significant differences in firm characteristics between the pilot and control firms. However, after deleting observations with missing data for non-GAAP exclusions and other variables, there seem to be several differences between the pilot and control firms. For instance, in Panel A, pilot firms have slightly higher future operating earnings and future operating cash flows than control firms prior to the Pilot Program. There are also minor differences in earnings volatility, whether firms report a loss, and book-to-market

[8] As an alternative specification, I also include *Firm Age* as an additional control variable in my tests to control for firm age. *Firm Age* is defined as natural log of the number of years since a firm first appeared in Compustat. The results (not tabulated for parsimony) are essentially indistinguishable from the tabulated results. For example, in Table 5, with this alternative specification, the estimated coefficients on my variable of interest *Pilot* × *Post* × *Exclusions* is 0.698 (standard error of 0.301) and 0.767 (standard error of 0.425) for *Future Operating Earnings* and *Future Operating Cash Flows*, respectively.

ratio.[9] Besides these minor differences, pilot firms and control firms are generally similar in firm characteristics. Table 2 also shows that the average non-GAAP earnings are higher than the average GAAP earnings for both pilot and control firms, consistent with the notion that managers typically exclude expenses when calculating non-GAAP earnings.

3.4 *Correlation Coefficients*

Table 3 presents pairwise correlation coefficients among the variables used in the main analysis. The numbers above and below the diagonal represent Spearman and Pearson correlations, respectively. Panel A presents correlation coefficients using the full sample. *Future Operating Earnings* and *Future Operating Cash Flows* show a strong positive correlation (both the Pearson and Spearman correlation coefficients are 0.76). *Non-GAAP Earnings* is positively correlated with both *Future Operating Earnings* and *Future Operating Cash Flows* (the Pearson correlation coefficients are 0.76 and 0.69 for *Future Operating Earnings* and *Future Operating Cash Flows*, respectively; the Spearman correlation coefficients are 0.76 and 0.68 for *Future Operating Earnings* and *Future Operating Cash Flows*, respectively). *GAAP Earnings* is also positively associated with *Future Operating Earnings* and *Future Operating Cash Flows*. However, the association is lower than that of *Non-GAAP Earnings*. For instance, the Pearson correlation coefficient between *GAAP Earnings* and *Future Operating Earnings* is 0.62, while the Pearson correlation coefficient between *Non-GAAP Earnings* and *Future Operating Earnings* is 0.76. *Exclusions* is negatively correlated with *Future Operating Earnings* and *Future Operating Cash Flows* (the Pearson correlation coefficients are -0.16 and -0.06 for *Future*

[9] I compute descriptive statistics for the full sample of pilot and control firms, regardless of whether they report non-GAAP earnings. I find that the statistical differences in firm characteristics between the pilot and control firms are consistent between the full sample and the sample used in regression analysis.

Operating Earnings and *Future Operating Cash Flows*, respectively; the Spearman correlation coefficients are -0.06 and -0.04 for *Future Operating Earnings* and *Future Operating Cash Flows*, respectively), consistent with the notion that non-GAAP exclusions are on average expenses with implications for firms' future performance.

Table 3 Panel B presents correlation coefficients calculated for pilot firms in the pre-program period. *Non-GAAP Earnings* is again more positively correlated with *Future Operating Earnings* and *Future Operating Cash Flows* than *GAAP Earnings*. *Exclusions* is negatively associated with both dependent variables. The Pearson correlation coefficients are -0.18 and -0.08 for *Future Operating Earnings* and *Future Operating Cash Flows*, respectively. The Spearman correlation coefficients are -0.11 and -0.04 for *Future Operating Earnings* and *Future Operating Cash Flows*, respectively. Table 3 Panel C shows correlation coefficients computed for control firms in the pre-program period. *Exclusions* continues to be negatively correlated with both *Future Operating Earnings* and *Future Operating Cash Flows* (the Pearson correlation coefficients are -0.21 and -0.07, and the Spearman correlation coefficients are -0.11 and -0.03 for *Future Operating Earnings* and *Future Operating Cash Flows*, respectively).

Correlation coefficients calculated for pilot firms in the post-program period are presented in Table 3 Panel D. *Exclusions* is no longer negatively correlated with *Future Operating Earnings* and *Future Operating Cash Flows*. The Pearson correlation coefficients are not statistically significant for either *Future Operating Earnings* or *Future Operating Cash Flows*. The Spearman correlation coefficients are not statistically significant for *Future Operating Earnings*, but significantly positive for *Future Operating Cash Flows*. The results are consistent with the prediction that the quality of non-GAAP

exclusions improves for pilot firms following the Pilot Program. Lastly, Table 3 Panel E shows correlation coefficients computed for control firms in the post-program period. *Exclusions* continues to be negatively correlated with both dependent variables (the Pearson correlation coefficients are -0.16 and -0.06 for *Future Operating Earnings* and *Future Operating Cash Flows*, respectively; the Spearman correlation coefficients are -0.10 and -0.03 for *Future Operating Earnings* and *Future Operating Cash Flows*, respectively). The results are consistent with the expectation that control firms do not improve their quality of non-GAAP exclusions following the program.

CHAPTER 4

RESEARCH DESIGN AND EMPIRICAL ANALYSIS

4.1 *The Pilot Program and Non-GAAP Reporting Likelihood*

Before testing my hypothesis, I first examine whether the Pilot Program affects the likelihood of non-GAAP reporting. It is possible that managers of pilot firms respond to the increased short selling threats by stopping non-GAAP reporting instead of increasing the quality of exclusions. If this is the case for a large number of firms, my main test may suffer from self-selection bias, since I am only able to observe the quality of exclusions that are disclosed. In this section, I check whether the self-selection concern is an actual problem by investigating the effect of the Pilot Program on non-GAAP reporting likelihood. Specifically, I use the non-GAAP reporting incidences of all pilot and control firms over the sample period and estimate the following difference-in-differences logistic regression:[10]

$$\begin{aligned} Report_q = {} & \beta_1 Pilot + \beta_2 Post + \beta_3 Pilot \times Post + \beta_4 Sale\ Growth_q + \beta_5 Size_q \\ & + \beta_6 Earnings\ Volatility_q + \beta_7 Loss_q + \beta_8 BM_q + \beta_9 Intangibles_q \\ & + \beta_{10} Leverage_q + \beta_{11} Special\ Items_q + \beta_{12} Bad\ News_q + \beta_{13} Q4_q \\ & + IndustryFEs + QuarterFEs + \varepsilon_q, \end{aligned} \quad (1)$$

where *Report* equals one if a firm-quarter has non-GAAP earnings and zero otherwise; *Intangibles* represents intangible assets divided by total assets; *Leverage* is defined as long-term debt divided by shareholder's equity. *Special Items*, *Bad News*, and *Q4* are all indicator variables. *Special Items* equals one if the firm has special items in the current quarter, zero otherwise. *Bad News* equals one if the firm's earnings are lower than the latest consensus analyst forecasts prior to earnings announcement, zero otherwise. *Q4* equals one

[10] I also estimate Equation (1) using an OLS method. The conclusion remains unchanged.

if the current quarter is the fourth fiscal quarter, zero otherwise. All other variables are as defined in Appendix A. The coefficient on the interaction term *Pilot* × *Post* (β_3) measures the difference in changes of the likelihood of reporting non-GAAP earnings around the Pilot Program between pilot and control firms. I include industry fixed effects and quarter fixed effects, where industries are defined using the Fama-French 48-industry classification (Fama and French, 1997). All continuous variables are winsorized at the 1st and 99th percentiles to reduce the influence of outliers. Standard errors are clustered at the firm level.

Table 4 presents the results from estimating Equation (1). The coefficient on *Pilot* is not significantly different from zero, suggesting that there is no significant difference in the likelihood of reporting non-GAAP earnings between pilot and control firms prior to the Pilot Program. The coefficient on *Post* is positive and significant, consistent with prior evidence that there is an increasing trend in the likelihood of non-GAAP reporting.[11] The coefficient on the interaction term *Pilot* × *Post* is not statistically significant, suggesting that there is no significant difference in the changes of the likelihood of non-GAAP reporting around the Pilot Program between pilot and control firms. It appears that the decision over whether to report non-GAAP earnings is not influenced by the Pilot Program, which alleviates the concern over self-selection bias.

[11] The inclusion of quarter fixed effects can make the coefficient on *Post* less useful in detecting the absolute trend of non-GAAP reporting over time for control firms. Because the time trend is not of interest in this study, I include quarter fixed effects in the model. In untabulated robustness tests, I find that my conclusions are unchanged if I omit quarter fixed effects.

4.2 *The Pilot Program and the Quality of Non-GAAP Exclusions*

To study the impact of the Pilot Program on the quality of non-GAAP exclusions, I estimate the following difference-in-differences regression:

$$\begin{aligned} \textit{Future Operating Performance}_{q+1,\,q+4} = {} & \beta_1 \textit{Pilot} + \beta_2 \textit{Non-GAAP Earnings}_q \\ & + \beta_3 \textit{Pilot} \times \textit{Non-GAAP Earnings}_q + \beta_4 \textit{Exclusions}_q \\ & + \beta_5 \textit{Pilot} \times \textit{Exclusions}_q + \beta_6 \textit{Post} + \beta_7 \textit{Pilot} \times \textit{Post} \\ & + \beta_8 \textit{Post} \times \textit{Exclusions}_q + \beta_9 \textit{Pilot} \times \textit{Post} \times \textit{Exclusions}_q \\ & + \beta_{10} \textit{Sales Growth}_q + \beta_{11} \textit{Pilot} \times \textit{Sales Growth}_q + \beta_{12} \textit{Size}_q \\ & + \beta_{13} \textit{Pilot} \times \textit{Size}_q + \beta_{14} \textit{Earnings Volatility}_q \\ & + \beta_{15} \textit{Pilot} \times \textit{Earnings Volatility}_q + \beta_{16} \textit{Loss}_q + \beta_{17} \textit{Pilot} \times \textit{Loss}_q \\ & + \beta_{18} \textit{BM}_q + \beta_{19} \textit{Pilot} \times \textit{BM}_q + \textit{IndustryFEs} + \textit{QuarterFEs} + \varepsilon_q. \end{aligned} \quad (2)$$

Following prior literature, I examine the association between non-GAAP exclusions (*Exclusions*) and firms' future performance (*Future Operating Performance*) to measure the quality of exclusions (Doyle et al., 2003; Kolev et al., 2008; Bentley et al., 2018). This analysis is motivated by managers' assertion that non-GAAP earnings are more informative because they exclude transitory items unrelated to firms' core earnings and better reflect the performance of continuing operations. Therefore, the extant literature regards the quality of non-GAAP exclusions as higher if the association between exclusions and firms' future performance is weaker.

The dependent variable, *Future Operating Performance*, is alternatively: *Future Operating Earnings*, measured as operating earnings summed over quarters q+1 to q+4, divided by total assets in quarter q, or *Future Operating Cash Flows*, measured as operating cash flows summed over quarters q+1 to q+4, divided by total assets in quarter q. All variables in Equation (2) are defined in Appendix A. As in Equation (1), standard errors are clustered by firms. I include industry fixed effects (the Fama-French 48-industry

classification) and quarter fixed effects in the regression model. Since *Future Operating Performance* is based on four quarters while *Non-GAAP Earnings* and *Exclusions* are quarterly, perfectly permanent earnings and exclusions would have a coefficient of four while perfectly transitory earnings and exclusions would have a coefficient of zero. Consistent with prior research, high-quality exclusions are defined as those that have the least predictive power for future performance (e.g., Kolev et al., 2008). The coefficient on *Exclusions* (β_4) represents the association between exclusions and future performance for control firms in the period prior to the Pilot Program. The coefficient on the interaction term *Pilot* × *Exclusions* (β_5) represents the difference in the association between exclusions and future performance for pilot firms compared to control firms in the pre-program period.

Column (1) of Table 5 presents the results using *Future Operating Earnings* as the dependent variable. The coefficient on *Non-GAAP Earnings* (β_2) is 3.383, suggesting that one dollar of non-GAAP earnings in the current quarter is associated with $3.38 of operating earnings over the next four quarters for control firms. The coefficient on the interaction term *Pilot* × *Non-GAAP Earnings* (β_3) is insignificant, indicating that there is no significant difference in the association between current quarter non-GAAP earnings and future performance for pilot firms compared to control firms. The coefficient on *Exclusions* (β_4) is -0.771, indicating that one dollar of exclusions in the current quarter is associated with 77 cents of expenses over the next four quarters for control firms prior to the Pilot Program. This result is consistent with previous evidence that the excluded items are not perfectly transitory, but on average are more transitory than non-GAAP earnings (Doyle et al., 2003; Kolev et al., 2008). The coefficient on *Pilot* × *Exclusions* (β_5) is insignificant, implying that the association between exclusions and future performance

does not differ between pilot firms and control firms prior to the Pilot Program (i.e., exclusions are of similar quality in these firms prior to the beginning of the program). The coefficient on *Post* × *Exclusions* (β_8) is insignificant, indicating that the association between exclusions and future performance during the program is not significantly different from that in the pre-program period for control firms. In other words, the quality of exclusions does not change for control firms after the implementation of the Pilot Program.

If the Pilot Program improved the quality of non-GAAP exclusions, then the relation between future performance and exclusions should be less negative, or even insignificant, during the period of the program (i.e., the exclusions become more transitory). My variable of interest is the interaction term *Pilot* × *Post* × *Exclusions*. The coefficient on this interaction term (β_9) is 0.699 and statistically significant, suggesting an improvement in the quality of exclusions for pilot firms relative to control firms following the beginning of the program. Results from *F*-tests show that the sum of coefficients on *Exclusions*, *Pilot* × *Exclusions*, *Post* × *Exclusions*, and *Pilot* × *Post* × *Exclusions* ($\beta_4 + \beta_5 + \beta_8 + \beta_9$) is not significantly different from zero (*p*-value = 0.532). In other words, during the program, non-GAAP exclusions of pilot firms become transitory and have no predictive power for future performance. Similar inferences are found in Column (2), where *Future Operating Cash Flows* is used as the dependent variable. Overall, these results indicate that the quality of non-GAAP exclusions improves for pilot firms during the program relative to control firms.

CHAPTER 5

CROSS-SECTIONAL ANALYSES

The results so far suggest that short selling threats improve the quality of non-GAAP exclusions. However, this effect is unlikely to be the same across all types of firms. In this section, I conduct cross-sectional analyses to further shed light on the effect of short selling threats on the quality of non-GAAP exclusions.

5.1 *Ex Ante Monitoring*

In the main discussion, I argue that the effect of short selling threats on the quality of non-GAAP exclusions exists because managers are less motivated to use low-quality non-GAAP exclusions. In other words, short sellers act as monitoring agents and discourage managers from engaging in aggressive non-GAAP reporting. If a firm has already been closely monitored, then the monitoring effect of short sellers will not have a strong impact on managerial reporting choices. In contrast, for firms with little *ex ante* monitoring, short sellers will have a bigger impact on discouraging managers' opportunistic behavior.

Prior studies show that larger firms have more visibility, and that institutional investors and financial analysts can serve a monitoring role on managers' reporting behavior and disclosure decisions (e.g., Shleifer and Vishny, 1986; Bushee, 1998; Chen, Harford, and Li, 2007; Armstrong, Guay, and Weber, 2010; Cheng, Huang, Li, and Lobo, 2010). Thus, I use firm size (*Size*), institutional ownership (*InstOwn*), and analyst coverage (*Coverage*) to measure *ex ante* monitoring strength. Specifically, I divide the firms in my sample into two groups based on the median of firm size, institutional ownership, and analyst coverage, respectively. To this end, I first calculate the average value of each cross-

sectional variable for every firm over the pre-program window. Then, I assign firms into two groups based on the median value of the pre-program period averages. Firms in the subsample with higher (lower) values for each cross-sectional variable have stronger (weaker) *ex ante* monitoring. Therefore, I expect the effect of short selling threats on the quality of non-GAAP exclusions to be less (more) pronounced for firms in the groups with higher (lower) values of firm size, institutional ownership, and analyst coverage.

I estimate the difference-in-differences regression of Equation (2) for each subsample of all three variables measuring *ex ante* monitoring strength. Results are presented in Table 6 Panel A. As predicted, the effect of short selling threats on the quality of non-GAAP exclusions is driven by smaller firms, firms with lower institutional ownership, and firms with lower analyst coverage. The results are consistent using either *Future Operating Earnings* or *Future Operating Cash Flows* as the dependent variables. These results indicate that the effect of short selling threats has a stronger impact on firms with weaker *ex ante* monitoring.

5.2 *Ratio of Fundamental Value to Market Value*

Dechow et al. (2001) provide evidence that short sellers are more likely to position themselves in firms with low ratios of fundamental value (e.g., earnings, book value, and cash flows) to market value. Therefore, I expect the effect of increased short selling threats induced by the Pilot Program to be more pronounced in such firms.

To test the above conjecture, I use three measures to capture the ratio of fundamental value to market value: earnings-to-price ratio (*EP Ratio*), book-to-market ratio (*BM*), and cash flow-to-price ratio (*CFP Ratio*). I first compute these ratios for each firm in every quarter. Similar to Section 5.1, I calculate the average value of each ratio for

every firm over the pre-program window. I then rank these average values for all firms in the sample and assign firms into two groups based on the sample median. Firms in the group with lower (higher) ratios of fundamental value to market value are more (less) likely to be shorted. Thus, I predict the effect of short selling threats on the quality of exclusions to be more (less) pronounced for firms in the groups with lower (higher) earnings-to-price ratio, book-to-market ratio, and cash flow-to-price ratio.

I estimate the difference-in-differences regression of Equation (2) for each subsample with different levels of ratios. Results are presented in Table 6 Panel B. As expected, the effect of short selling threats on the quality of non-GAAP exclusions is more pronounced in firms with lower earnings-to-price ratio, book-to-market ratio, and cash flow-to-price ratio, regardless of which dependent variable is used.

CHAPTER 6

ROBUSTNESS CHECKS AND ADDITIONAL ANALYSES

6.1 *Robustness Checks*

One may argue that the time trend in the quality of non-GAAP exclusions differs between pilot firms and control firms for some unknown reasons. However, such a trend is unlikely to exist because pilot firms are selected by the SEC using a random sampling process. Nonetheless, I conduct placebo tests to further provide evidence on the validity of my results. Specifically, I choose July 1, 2013 as my placebo event date and create variables for the pilot and control firms for the 12-quarter period centered around the placebo event date. Table 7 presents the results of the placebo test. The coefficients on *Pilot* × *Post* × *Exclusions* are insignificant in the analyses using either *Future Operating Earnings* or *Future Operating Cash Flows* as dependent variables, indicating that the association between exclusions and future performance does not differ between pilot and control firms in the post-placebo event period. Overall, I find no evidence that pilot firms improve the quality of their non-GAAP exclusions relative to control firms around the placebo event date.

Grullon et al. (2015) show that pilot firms reduce the level of investment and equity financing relative to control firms following the Pilot Program. To ensure that my results are not driven by changes in investment and equity financing level, I reestimate Equation (2) by including R&D and capital expenditures, equity issuance, and debt issuance as control variables (untabulated). The conclusions remain unchanged. De Angelis et al. (2017) provide evidence that the Pilot Program increases the use of stock options in pilot firms relative to control firms. To rule out the possibility that the results from the main test

are driven by changes in managerial compensation structures, I reestimate Equation (2) with additional control variables capturing the proportion of stock options in management compensation packages. Again, the conclusions are unchanged.

6.2 *Type of Non-GAAP Exclusions*

I conduct analyses to examine the type of non-GAAP exclusions for pilot and control firms. Bentley et al. (2018) provide evidence that the most common recurring items excluded are stock compensation expense and amortization. Therefore, I empirically examine how well stock compensation expense and amortization explain the non-GAAP exclusions in pilot and control firms by estimating the following regression:

$$Exclusions_q = \beta_0 + \beta_1 Stock\ Compensation_q + \beta_2 Amortization_q + \varepsilon_q, \quad (3)$$

where *Stock Compensation* represents quarterly stock compensation expense divided by total assets. *Amortization* is calculated as annual amortization expense divided by four and by total assets. *Exclusions* is as defined before.[12] I estimate regression (3) for subsamples of pilot and control firms in the periods before and after the implementation of the Pilot Program.

Table 8 Panel A presents the results. I denote differences in the explanatory power across subsamples with superscripts.[13] In the periods prior to the program, the explanatory

[12] For this regression, I truncate continuous variables at the 1st and 99th percentiles to mitigate the effect of outliers. I truncate the variables, rather than winsorizing, to preserve the identity that *Exclusions* equals the net sum, or partial sum, of all exclusion types (i.e., the dependent variable is the sum or partial sum of the independent variables). Winsorizing violates the identity by altering the data.

[13] I use a bootstrap procedure to compare explanatory power across subsamples. For example, to test whether stock compensation expense and amortization have more explanatory power for non-GAAP exclusions in pilot firms compared to control firms before the implementation of the program, I pool the observations from both subsamples (Pre-Control and Pre-Pilot) together and randomly assign observations to pseudo subsamples of the same size as Pre-Control and Pre-Pilot subsamples. I then estimate Equation (3) using the pseudo subsamples and record the difference in explanatory power. Repeating this procedure 500 times provides an empirical distribution for the difference in explanatory power under the null hypothesis that there is no significant difference in explanatory power. I then compare the observed difference from the original

power of stock compensation expense and amortization for non-GAAP exclusions is not significantly different between pilot and control firms (Columns 1 and 3). After the implementation of the program, the explanatory power of stock compensation expense and amortization does not change significantly for control firms (Columns 1 and 2). However, the explanatory power decreases significantly for pilot firms following the program (Columns 3 and 4). These findings are consistent with the results from the main test on changes of the quality of non-GAAP exclusions by suggesting that pilot firms reduce their use of non-GAAP exclusions of recurring items after the implementation of the Pilot Program.

I also estimate the following regression to examine how well transitory items explain the non-GAAP exclusions in pilot and control firms:

$$Exclusions_q = \beta_0 + \beta_1 \, Transitory \, Value_q + \varepsilon_q, \quad (4)$$

where *Transitory Value* equals the sum of all Compustat-provided transitory items. *Exclusions* is as defined before.[14] Table 8 Panel B presents the results. Before the implementation of the program, the explanatory power of transitory items for non-GAAP exclusions does not differ significantly between pilot and control firms (Columns 5 and 7). After the program, the explanatory power of transitory items does not change significantly for control firms (Columns 5 and 6). In contrast, the explanatory power increases significantly for pilot firms following the program (Columns 7 and 8). These findings are, again, consistent with the results from the main test on changes of non-GAAP exclusions

subsamples to this distribution. If the observed difference falls outside of the 95% confidence interval, then the observed difference is statistically significant.

[14] Similar to regression (3), continuous variables are truncated at the 1st and 99th percentiles to reduce the influence of outliers.

quality and suggest that pilot firms increase their use of non-GAAP exclusions of transitory items after the implementation of the Pilot Program.

6.3 *Quality of Exclusions Considering GAAP Earnings Management*

Fang et al. (2016) find that earnings management decreases for pilot firms during the Pilot Program relative to control firms. Thus, an alternative explanation for the results of my main difference-in-differences test may be that the results capture the reduction of GAAP earnings management rather than the improvement in the quality of non-GAAP exclusions. To test this alternative explanation, I include total accruals and abnormal accruals as additional control variables in the regression model of Equation (2). Both total accruals and abnormal accruals are commonly used to measure GAAP earnings management (e.g., Jones, 1991; Dechow et al., 1995; Klein, 2002; Cheng and Warfield, 2005; Francis et al., 2013; Fang et al., 2016; among others). *Total Accruals* is defined as the firm's net income before extraordinary items less operating cash flows, scaled by total assets in the previous quarter. The calculation of *Abnormal Accruals* is based on modified Jones model that controls for firm performance (Dechow et al., 1995; Jones, 1991; Kothari, Leone, and Wasley, 2005; Francis et al., 2013). *Abnormal Accruals* is computed as the firm-specific residuals in the following model:

$$\begin{aligned} Total\ Accruals_{i,q} = {} & \beta_1 (1/Assets_{i,q-1}) + \beta_2 (\Delta Sales_{i,q} - \Delta AR_{i,q}) / Assets_{i,q-1} \\ & + \beta_3 PPE_{i,q} / Assets_{i,q-1} + \beta_4 ROA_{i,q} + IndustryFEs \\ & + QuarterFEs + \varepsilon_{i,q}, \end{aligned} \quad (5)$$

where *Assets* is total assets; *Sales* is net sales; *AR* is accounts receivable; *PPE* is gross property, plant and equipment; *ROA* is return on assets. Industry and quarter fixed effects are also included. *Total Accruals* and *Abnormal Accruals* are signed measures.

Panel A of Table 9 presents regression results when *Total Accruals*, *Pilot* × *Total Accruals*, *Post* × *Total Accruals*, and *Pilot* × *Post* × *Total Accruals* are included in Equation (2). The coefficients on *Pilot* × *Post* × *Exclusions* are significantly positive in both Column (1) and Column (2), indicating that pilot firms improve the quality of their non-GAAP exclusions relative to control firms following the Pilot Program, no matter the level of their GAAP earnings management measured by total accruals. Panel B of Table 9 shows the results when *Abnormal Accruals*, *Pilot* × *Abnormal Accruals*, *Post* × *Abnormal Accruals*, and *Pilot* × *Post* × *Abnormal Accruals* are included in the regression. In Column (1) and Column (2), the coefficients on *Pilot* × *Post* × *Exclusions* are again significantly positive. Overall, the results from Table 9 are consistent with the results from the main difference-in-differences test. In summary, the improvement in the quality of pilot firms' non-GAAP exclusions is not driven by the change of GAAP earnings management in pilot firms.

CHAPTER 7

CONCLUSION

In this study, I exploit the regulatory experiment introduced by the Pilot Program under Regulation SHO to study the causal effect of short selling threats on firms' non-GAAP reporting quality. From 2005 to 2007, one-third of the stocks from the Russell 3000 index were exempted from short-sale price tests under the Pilot Program introduced by Reg SHO. To implement this Pilot Program, the SEC ranked Russell 3000 stocks by average daily trading volume, designated every third one as a pilot stock, and temporarily suspended the short-sale price tests for these stocks. During this time, short selling constraints decreased and short selling threats increased considerably for pilot firms. Short selling threats for control firms stayed unchanged until July 2007, when the SEC removed short-sale price tests for all exchange-listed stocks. Since firms had no influence over the selection of pilot stocks, the program represents an exogenous shock to the short selling constraints of pilot firms, and thus can provide insights on causal relations.

Using difference-in-differences tests, I provide empirical evidence that the increased short selling threats improve the quality of non-GAAP exclusions. Specifically, prior to the Pilot Program, the exclusions of both pilot and control firms are strongly associated with future firm performance. After the implementation of the program, exclusions of pilot firms are no longer related to future performance, while exclusions of control firms are still highly negatively associated with future performance over the next four quarters. A placebo test shows that this difference in the association between exclusions and future performance cannot be explained by the time trend in non-GAAP reporting quality between pilot and control firms. Therefore, the results show that managers

respond to the increased short selling threats by reducing the use of low-quality exclusions and therefore improving the quality of their non-GAAP reporting. Moreover, I find that the effect of short selling threats is more pronounced in smaller firms, firms with lower institutional ownership, and firms with lower analyst coverage. This result is driven by weaker *ex ante* monitoring at such firms. I also find that the effect of short selling threats is stronger in firms with lower ratios of fundamental value to market value. This result is driven by the fact that short sellers target firms with low ratios of fundamental value to market value.

Although short selling remains a controversial activity, my results reveal important benefits from such activities to investors. In particular, I document that an increase in short selling threats curbs managers' behavior to use aggressive non-GAAP exclusions. Therefore, I contribute to the literature by documenting the benefits of short sellers that inarguably spills over to shareholders and potential investors. Overall, the findings of this study are consistent with the notion that short sellers play an important monitoring role in disciplining managers' non-GAAP reporting.

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APPENDIX A

VARIABLE DEFINITIONS

Variable	Definition
Abnormal Accruals	Firm-specific residuals calculated in the following model: $Total\ Accruals_{i,q} = \beta_1 (1/Assets_{i,q-1}) + \beta_2 (\Delta Sales_{i,q} - \Delta AR_{i,q}) / Assets_{i,q-1} + \beta_3 PPE_{i,q}/Assets_{i,q-1} + \beta_4 ROA_{i,q} + IndustryFEs + QuarterFEs + \varepsilon_{i,q}$
Amortization	Amortization expense, calculated as annual amortization expense divided by 4, and divided by total assets in quarter *q*
AR	Accounts receivable
Assets	Total assets
Bad News	Indicator variable equals to 1 if the firm's earnings are lower than the latest consensus analyst forecast prior to the earnings announcement, 0 otherwise
BM	Book-to-market ratio, calculated as shareholder's equity divided by market value of equity
CFP Ratio	Cash flow-to-price ratio, calculated as cash flows from operations divided by market value of equity
Coverage	Analyst coverage, defined as the number of analysts following the firm
Earnings Volatility	Standard deviation of ROA over the preceding eight quarters
EP Ratio	Earnings-to-price ratio, calculated as operating earnings divided by market value of equity
Exclusions	Non-GAAP exclusions, calculated as non-GAAP EPS reported by managers less bottom-line diluted GAAP EPS, multiplied by the number of diluted shares outstanding, divided by total assets in quarter *q*
Future Operating Cash Flows	Operating cash flows summed over quarters *q*+1 to *q*+4, divided by total assets in quarter *q*
Future Operating Earnings	Operating earnings summed over quarters *q*+1 to *q*+4, divided by total assets in quarter *q*
Future Operating Performance	Either *Future Operating Cash Flows* or *Future Operating Earnings*

GAAP Earnings	Bottom-line diluted GAAP EPS multiplied by the number of diluted shares outstanding, divided by total assets in quarter *q*
InstOwn	Institutional ownership, calculated as the percentage of stocks held by institutional investors
Intangibles	Intangible assets, as reported in Compustat, divided by total assets in quarter *q*
Leverage	Long-term debt divided by shareholder's equity
Loss	Indicator variable equals to 1 if bottom-line diluted GAAP EPS is less than zero, 0 otherwise
MVE	Market value of equity
Non-GAAP Earnings	Non-GAAP EPS reported by managers multiplied by the number of diluted shares outstanding, divided by total assets in quarter *q*
Pilot	Indicator variable equals to 1 for pilot firms and 0 for control firms
Post	Indicator variable equals to 1 if the observation is between 2005 Q2 and 2006 Q3, and 0 if the observation is between 2003 Q1 and 2004 Q2
PPE	Gross property, plant and equipment
Q4	Indicator variable equals to 1 if the current quarter is the fourth fiscal quarter, 0 otherwise
Report	Indicator variable equals to 1 if a firm quarter has non-GAAP earnings, 0 otherwise
ROA	Return on assets
Sales	Net sales
Sales Growth	Sales in quarter *q* less sales in quarter *q*–4, divided by total assets in quarter *q*
Size	Natural log of total assets
Special Items	Indicator variable equals to 1 if the firm has special items in the current quarter, 0 otherwise
Stock Compensation	Stock compensation expense, calculated as stock compensation expense divided by total assets in quarter *q*

Total Accruals	Net income before extraordinary items less operating cash flows, divided by total assets in quarter *q*-1
Transitory Value	The sum of all special item components. This item is the sum of the following Compustat variables: aqdq (acquisitions), gldq (investment gain/loss), gdwlidq (goodwill impairment), setdq (settlement), rcdq (restructuring), wddq (write-downs), dtedq (debt extinguishment), rdipdq (R&D and intellectual property), spidq (other). This variable is set to zero if all of the above components are missing. The per-share values are multiplied by the number of diluted shares outstanding and divided by total assets in quarter *q*

Table 1
Sample Construction

Description	Pilot Firms	Pilot Firm Obs.	Control Firms	Control Firm Obs.
Pilot and control firms that were in Russell 3000 index in both 2004 and 2005	876	--	1,757	--
All observations for pilot and control firms in Compustat North America, from 2003 Q1 to 2006 Q3	864	12,746	1,740	25,667
Remove observations in the period from 2004 Q3 to 2005 Q1	864	10,064	1,740	20,322
Keep observations with non-GAAP earnings data	316	1,866	642	3,718
Keep observations with non-missing control data	295	1,759	601	3,515

Table 2
Descriptive Statistics

Panel A: Pre Period

Variable	Pilot Firms (1)			Control Firms (2)			Difference in Mean	Difference in Median
	N	Mean	Median	N	Mean	Median	(1) - (2)	(1) - (2)
Future Operating Earnings	785	0.052	0.050	1603	0.044	0.041	0.008*	0.009*
Future Operating Cash Flows	769	0.107	0.101	1554	0.095	0.088	0.012**	0.013**
GAAP Earnings	785	0.005	0.008	1603	0.004	0.006	0.001	0.002
Non-GAAP Earnings	785	0.012	0.011	1603	0.010	0.009	0.002	0.002
Exclusions	785	0.007	0.002	1603	0.006	0.002	0.001	0.000
Sales Growth	785	0.023	0.014	1603	0.023	0.013	0.000	0.001
Size	785	7.434	7.161	1603	7.418	7.326	0.016	-0.165
Earnings Volatility	785	0.018	0.009	1603	0.025	0.010	-0.007*	-0.001
Loss	785	0.217	0.000	1603	0.256	0.000	-0.039*	0.000*
BM	785	0.482	0.448	1603	0.513	0.462	-0.031*	-0.014
MVE	785	5570.8	1178.3	1603	5246.1	1241.0	324.7	-62.8

Table 2 (cont'd)

Panel B: Post Period

Variable	Pilot Firms (1)			Control Firms (2)			Difference in Mean	Difference in Median
	N	Mean	Median	N	Mean	Median	(1) - (2)	(1) - (2)
Future Operating Earnings	974	0.059	0.052	1912	0.052	0.052	0.007*	0.000
Future Operating Cash Flows	977	0.105	0.094	1907	0.099	0.092	0.006*	0.002
GAAP Earnings	974	0.011	0.012	1912	0.010	0.010	0.001	0.002
Non-GAAP Earnings	974	0.017	0.014	1912	0.016	0.014	0.001	0.000
Exclusions	974	0.006	0.003	1912	0.006	0.003	0.000	0.000
Sales Growth	974	0.026	0.019	1912	0.025	0.017	0.001	0.002
Size	974	7.592	7.264	1912	7.540	7.439	0.052	-0.175
Earnings Volatility	974	0.013	0.007	1912	0.014	0.007	-0.001	0.000
Loss	974	0.144	0.000	1912	0.172	0.000	-0.028*	0.000*
BM	974	0.442	0.416	1912	0.455	0.419	-0.013	-0.003
MVE	974	6781.7	1670.9	1912	6651.9	1637.9	129.8	32.9

This table presents descriptive statistics for the variables used in the main analyses. All variables are defined in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 3
Correlation Matrices

Panel A: Full Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) *Future Operating Earnings*		0.76***	0.69***	0.76***	-0.06***	0.39***	-0.07***	0.06***	-0.38***	-0.49***
(2) *Future Operating Cash Flows*	0.76***		0.57***	0.68***	-0.04***	0.36***	-0.1***	0.14***	-0.28***	-0.48***
(3) *GAAP Earnings*	0.62***	0.51***		0.80***	-0.40***	0.35***	0.01	-0.04***	-0.68***	-0.38***
(4) *Non-GAAP Earnings*	0.76***	0.69***	0.72***		0.08***	0.42***	-0.12***	0.09***	-0.48***	-0.51***
(5) *Exclusions*	-0.16***	-0.06***	-0.70***	-0.07***		0.06***	-0.31***	0.34***	0.49***	-0.16***
(6) *Sales Growth*	0.28***	0.32***	0.23***	0.31***	-0.03**		-0.22***	0.14***	-0.16***	-0.31***
(7) *Size*	0.02	-0.09***	0.09***	-0.04**	-0.17***	-0.19***		-0.44***	-0.21***	0.16***
(8) *Earnings Volatility*	-0.14***	-0.05***	-0.19***	-0.12***	0.17***	0.02	-0.28***		0.34***	-0.25***
(9) *Loss*	-0.40***	-0.30***	-0.64***	-0.48***	0.47***	-0.13***	-0.21***	0.30***		0.10***
(10) *BM*	-0.32***	-0.37***	-0.19***	-0.36***	-0.06***	-0.24***	0.16***	-0.12***	0.10***	

Table 3 (cont'd)

Panel B: Pre-Period Pilot Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) Future Operating Earnings		0.75***	0.72***	0.78***	-0.11***	0.47***	-0.12***	-0.00	-0.42***	-0.51***
(2) Future Operating Cash Flows	0.81***		0.55***	0.66***	-0.04*	0.36***	-0.05**	0.04	-0.29***	-0.43***
(3) GAAP earnings	0.73***	0.61***		0.80***	-0.45***	0.40***	-0.00	-0.12***	-0.70***	-0.36***
(4) Non-GAAP earnings	0.80***	0.71***	0.76***		0.02	0.47***	-0.12***	-0.00	-0.50***	-0.49***
(5) Exclusions	-0.18***	-0.08***	-0.71***	-0.13***		0.01	-0.28***	0.31***	0.52***	-0.15***
(6) Sales growth	0.34***	0.31***	0.25***	0.30***	-0.07*		-0.20***	0.08***	-0.22***	-0.31***
(7) Size	-0.01	-0.03	0.08**	-0.02	-0.15***	-0.21***		-0.36***	-0.18***	0.14***
(8) ROA	-0.13***	-0.11***	-0.17***	-0.11***	0.13***	0.06	-0.30***		0.35***	-0.16***
(9) Loss	-0.43***	-0.33***	-0.66***	-0.50***	0.50***	-0.16***	-0.18***	0.27***		0.11***
(10) BM	-0.29***	-0.27***	-0.14***	-0.31***	-0.09**	-0.25***	0.12***	-0.11***	0.11***	

Panel C: Pre-Period Control Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) Future Operating Earnings		0.81***	0.68***	0.77***	-0.11***	0.43***	-0.09***	0.04	-0.41***	-0.46***
(2) Future Operating Cash Flows	0.80***		0.57***	0.69***	-0.03*	0.38***	-0.09***	0.10***	-0.33***	-0.43***
(3) GAAP earnings	0.56***	0.48***		0.80***	-0.48***	0.38***	0.03*	-0.09***	-0.73***	-0.35***
(4) Non-GAAP earnings	0.76***	0.72***	0.70***		-0.03**	0.45***	-0.07***	0.03	-0.54***	-0.45***
(5) Exclusions	-0.21***	-0.07***	-0.73***	-0.09***		-0.02**	-0.26***	0.35***	0.54***	-0.08***
(6) Sales growth	0.35***	0.40***	0.26***	0.37***	-0.07***		-0.22***	0.16***	-0.18***	-0.34***
(7) Size	-0.02	-0.10***	0.12***	0.01	-0.16***	-0.23***		-0.49***	-0.24***	0.21***
(8) ROA	-0.14***	-0.08***	-0.23***	-0.16***	0.19***	0.04*	-0.30***		0.37***	-0.27***
(9) Loss	-0.40***	-0.33***	-0.65***	-0.52***	0.47***	-0.15***	-0.24***	0.33***		0.11***
(10) BM	-0.31***	-0.34***	-0.18***	-0.31***	-0.01	-0.28***	0.19***	-0.14***	0.11***	

Table 3 (cont'd)

Panel D: Post-Period Pilot Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) Future Operating Earnings		0.73***	0.71***	0.73***	0.00	0.27***	-0.00	0.12***	-0.33***	-0.44***
(2) Future Operating Cash Flows	0.70***		0.62***	0.69***	0.01*	0.32***	-0.10***	0.21***	-0.26***	-0.47***
(3) GAAP earnings	0.64***	0.52***		0.81***	-0.31***	0.34***	-0.00	0.07**	-0.60***	-0.38***
(4) Non-GAAP earnings	0.73***	0.67***	0.71***		0.16***	0.36***	-0.12***	0.23***	-0.41***	-0.50***
(5) Exclusions	-0.04	-0.01	-0.67***	0.00		0.08***	-0.30***	0.32***	0.43***	-0.22***
(6) Sales growth	0.22***	0.24***	0.25***	0.25***	-0.10***		-0.21***	0.10***	-0.17***	-0.20***
(7) Size	0.07**	-0.08**	0.07**	-0.04	-0.15***	-0.15***		-0.34***	-0.17***	0.05*
(8) ROA	-0.02	0.07**	-0.10***	0.04	0.19***	-0.02	-0.17***		0.31***	-0.30***
(9) Loss	-0.38***	-0.26***	-0.63***	-0.44***	0.46***	-0.18***	-0.16***	0.26***		0.03
(10) BM	-0.35***	-0.42***	-0.24***	-0.40***	-0.07**	-0.09***	0.05	-0.26***	0.05	

Panel E: Post-Period Control Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) Future Operating Earnings		0.74***	0.68***	0.75***	-0.10***	0.36***	-0.07**	0.09***	-0.35***	-0.52***
(2) Future Operating Cash Flows	0.73***		0.57***	0.70***	-0.03*	0.36***	-0.13***	0.19***	-0.23***	-0.54***
(3) GAAP earnings	0.61***	0.50***		0.78***	-0.37***	0.23***	-0.00	0.01	-0.64***	-0.39***
(4) Non-GAAP earnings	0.75***	0.69***	0.71***		0.14***	0.39***	-0.16***	0.17***	-0.42***	-0.54***
(5) Exclusions	-0.16***	-0.06***	-0.70***	-0.06**		0.13***	-0.37***	0.36***	0.47***	-0.20***
(6) Sales growth	0.22***	0.28***	0.16***	0.28***	0.07***		-0.23***	0.17***	-0.08***	-0.33***
(7) Size	0.04*	-0.12***	0.07***	-0.10***	-0.21***	-0.18***		-0.47***	-0.22***	0.20***
(8) ROA	-0.18***	-0.02	-0.15***	-0.05**	0.18***	-0.01	-0.31***		0.29***	-0.28***
(9) Loss	-0.39***	-0.25***	-0.61***	-0.44***	0.49***	-0.06***	-0.22***	0.26***		0.08***
(10) BM	-0.32***	-0.42***	-0.19***	-0.40***	-0.10***	-0.25***	0.21***	-0.12***	0.08***	

This table presents Pearson (below the diagonal) and Spearman (above the diagonal) correlation coefficients for the variables in the main analysis for the full sample (Panel A), for pilot and control firms in the pre-program period (Panels B and C), and for pilot and control firms in the post-program period (Panels D and E). All variables are defined in Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 4
Changes in Likelihood of Reporting Non-GAAP Earnings

Dependent Variable	(1) *Probability of Reporting Non-GAAP Earnings*	
	Coefficient Estimate	Standard Error
Pilot	0.000	0.027
Post	0.117***	0.021
Pilot × Post	**0.011**	**0.027**
Sales Growth	0.000*	0.000
Size	0.034***	0.007
Earnings Volatility	0.461	0.314
Loss	-0.042*	0.023
BM	0.034	0.042
Intangibles	0.308***	0.055
Leverage	0.008	0.008
Special Items	0.220***	0.015
Bad News	0.086***	0.012
Q4	-0.001	0.013
Industry Fixed Effects	Included	
Quarter Fixed Effects	Included	
Observations	8,176	
Adjusted R^2	0.505	

This table presents results on changes in likelihood of reporting non-GAAP earnings around the Pilot Program. Standard errors are clustered by firm. ∗, ∗∗, and ∗∗∗ indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively. All variables are defined in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 5
Changes in the Quality of Non-GAAP Exclusions during the Pilot Program

Dependent Variable	(1) *Future Operating Earnings*		(2) *Future Operating Cash Flows*	
	Coefficient Estimate	Standard Error	Coefficient Estimate	Standard Error
Pilot	-0.002	0.016	0.004	0.021
Non-GAAP Earnings	3.383***	0.198	3.636***	0.186
Pilot × Non-GAAP Earnings	0.019	0.315	-0.251	0.321
Exclusions	-0.771***	0.145	-0.851***	0.159
Pilot × Exclusions	-0.091	0.260	0.022	0.316
Post	-0.001	0.004	0.000	0.005
Pilot × Post	-0.002	0.004	-0.007	0.005
Post × Exclusions	-0.266	0.180	-0.169	0.225
Pilot × Post × Exclusions	**0.699****	**0.301**	**0.766***	**0.426**
Sales Growth	0.079*	0.042	0.187**	0.080
Pilot × Sales Growth	0.021	0.059	-0.025	0.106
Size	0.002*	0.001	-0.002	0.001
Pilot × Size	0.000	0.001	0.000	0.002
Earnings Volatility	-0.153**	0.066	-0.078	0.063
Pilot × Earnings Volatility	0.045	0.110	-0.093	0.164
Loss	0.007*	0.004	0.007	0.005
Pilot × Loss	0.008	0.008	0.015*	0.009
BM	-0.033***	0.007	-0.054***	0.008
Pilot × BM	-0.003	0.012	0.003	0.013
Industry Fixed Effects	Included		Included	
Quarter Fixed Effects	Included		Included	
Observations	5,274		5,207	
Adjusted R^2	0.724		0.788	
p-value of F-tests:				
Post period pilot firms' exclusions are transitory				
$\beta_4 + \beta_5 + \beta_8 + \beta_9 = 0$	*0.532*		*0.889*	
Post period control firms' exclusions are transitory				
$\beta_4 + \beta_8 = 0$	*0.000*		*0.003*	

This table presents results on changes in the quality of non-GAAP exclusions during the Pilot Program. Standard errors are clustered by firm. ∗, ∗∗, and ∗∗∗ indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively. All variables are defined in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 6
Cross-Sectional Tests on Changes in the Quality of Non-GAAP Exclusions during the Pilot Program

Panel A: *Ex Ante* Monitoring

	Coefficient Estimate (Standard Error)					
	(1)		(2)		(3)	
Cross-Sectional Variable	*Size*		*InstOwn*		*Coverage*	
	low	high	low	high	low	high
DV: *Future Operating Earnings*						
Pilot × *Post* × *Exclusions*	0.796**	0.426	0.887**	0.407	0.848**	0.156
	(0.396)	(0.302)	(0.420)	(0.366)	(0.379)	(0.369)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,631	2,643	2,634	2,617	2,559	2,615
Adjusted R^2	0.699	0.832	0.679	0.788	0.663	0.812
DV: *Future Operating Cash Flows*						
Pilot × *Post* × *Exclusions*	0.828**	0.430	0.876**	0.446	0.822**	0.232
	(0.399)	(0.301)	(0.419)	(0.372)	(0.379)	(0.376)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,560	2,624	2,593	2,568	2,515	2,570
Adjusted R^2	0.698	0.832	0.685	0.785	0.663	0.813

Table 6 (cont'd)

Panel B: Ratio of Fundamental Value to Market Value

Cross-Sectional Variable	Coefficient Estimate (Standard Error)					
	(4) *EP Ratio*		(5) *BM*		(6) *CFP Ratio*	
	low	high	low	high	low	high
DV: *Future Operating Earnings*						
Pilot × *Post* × *Exclusions*	0.811**	0.094	1.249***	-0.226	1.085***	-0.176
	(0.385)	(0.267)	(0.417)	(0.356)	(0.388)	(0.373)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,676	2,578	2,688	2,586	2,639	2,599
Adjusted R^2	0.694	0.847	0.785	0.560	0.705	0.808
DV: *Future Operating Cash Flows*						
Pilot × *Post* × *Exclusions*	0.887**	0.030	1.216***	-0.161	1.101***	-0.079
	(0.391)	(0.273)	(0.420)	(0.362)	(0.392)	(0.395)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,619	2,548	2,653	2,531	2,603	2,569
Adjusted R^2	0.693	0.849	0.786	0.559	0.704	0.808

This table presents results on cross-sectional tests on changes in the quality of non-GAAP exclusions during the Pilot Program. Standard errors are clustered by firm and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively. All variables are defined in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 7
Placebo Test on Changes in the Quality of Non-GAAP Exclusions

Dependent Variable	(1) *Future Operating Earnings*		(2) *Future Operating Cash Flows*	
	Coefficient Estimate	Standard Error	Coefficient Estimate	Standard Error
Pilot	-0.006	0.017	0.015	0.017
Non-GAAP Earnings	2.688***	0.147	2.731***	0.204
Pilot × Non-GAAP Earnings	-0.027	0.282	0.053	0.310
Exclusions	-0.334***	0.082	-0.209**	0.100
Pilot × Exclusions	0.040	0.171	0.048	0.170
Post	-0.016***	0.003	-0.010***	0.003
Pilot × Post	0.001	0.003	0.002	0.003
Post × Exclusions	-0.133	0.118	-0.002	0.135
Pilot × Post × Exclusions	**0.125**	**0.201**	**0.063**	**0.204**
Sales Growth	0.111***	0.037	0.127***	0.046
Pilot × Sales Growth	-0.057	0.054	-0.016	0.074
Size	0.002**	0.001	0.000	0.001
Pilot × Size	0.001	0.002	-0.002	0.002
Earnings Volatility	-0.428***	0.105	-0.182	0.146
Pilot × Earnings Volatility	-0.211	0.201	-0.379*	0.219
Loss	0.000	0.003	0.010**	0.004
Pilot × Loss	-0.001	0.006	0.006	0.007
BM	-0.024***	0.005	-0.032***	0.006
Pilot × BM	0.003	0.011	0.006	0.011
Industry Fixed Effects	Included		Included	
Quarter Fixed Effects	Included		Included	
Observations	6,787		6,815	
Adjusted R^2	0.734		0.807	
p-value of F-tests:				
Post period pilot firms' exclusions are transitory				
$\beta_4 + \beta_5 + \beta_8 + \beta_9 = 0$	*0.012*		*0.071*	
Post period control firms' exclusions are transitory				
$\beta_4 + \beta_8 = 0$	*0.000*		*0.037*	

This table presents results of placebo test on changes in the quality of non-GAAP exclusions for period not overlapped with the Pilot Program. Standard errors are clustered by firm. ∗, ∗∗, and ∗∗∗ indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively. All variables are defined in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 8
Type of Non-GAAP Exclusions

Panel A: Recurring Items

	Coefficient Estimate (Standard Error)			
	(1)	(2)	(3)	(4)
Sample	Control Firms		Pilot Firms	
	Pre	Post	Pre	Post
DV: *Exclusions*				
Intercept	0.003***	0.001***	0.001**	0.005***
	(0.001)	(0.000)	(0.000)	(0.001)
Stock Compensation	1.070***	0.840***	0.893***	1.113***
	(0.241)	(0.125)	(0.222)	(0.373)
Amortization	0.992***	0.956***	1.075***	0.480
	(0.304)	(0.124)	(0.246)	(0.314)
Observations	1,472	1,858	736	978
Adjusted R^2	0.089[a]	0.092[a]	0.079[a]	0.027[b]

Panel B: Transitory Items

	Coefficient Estimate (Standard Error)			
	(5)	(6)	(7)	(8)
Sample	Control Firms		Pilot Firms	
	Pre	Post	Pre	Post
DV: *Exclusions*				
Intercept	0.002***	0.002***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Transitory Value	0.757***	0.768***	0.766***	1.083***
	(0.105)	(0.097)	(0.144)	(0.129)
Observations	1,086	1,349	517	704
Adjusted R^2	0.058[c]	0.057[c]	0.048[c]	0.110[d]

This table presents results from regressions of non-GAAP exclusions on possible exclusion types. Standard errors are clustered by firm and presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively. All variables are defined in Appendix A. All continuous variables are truncated at the 1st and 99th percentiles. The variables are truncated rather than winsorized to preserve the identity that *Exclusions* equals the sum or partial sum of all exclusion types. Winsorizing violates this identity by altering the data. Adjusted R^2 labeled with different superscripts are statistically different from each other at the 5% level. For example, the adjusted R^2 in Column (1) is statistically different from that in Column (4), but not from those in Columns (2) and (3), while the adjusted R^2 in Column (4) is statistically different from those in Columns (1), (2), and (3).

Table 9
Changes in the Quality of Non-GAAP Exclusions during the Pilot Program, Considering GAAP Earnings Management

Panel A: GAAP Earnings Management Measured by Total Accruals

Dependent Variable	(1) *Future Operating Earnings*		(2) *Future Operating Cash Flows*	
	Coefficient Estimate	Standard Error	Coefficient Estimate	Standard Error
Pilot	-0.003	0.016	0.001	0.019
Non-GAAP Earnings	3.240***	0.189	3.314***	0.167
Pilot × Non-GAAP Earnings	0.040	0.303	-0.209	0.306
Exclusions	-0.800***	0.278	-0.898***	0.328
Pilot × Exclusions	-0.242	0.156	-0.329	0.266
Post	-0.002	0.004	0.000	0.005
Pilot × Post	-0.002	0.005	-0.008	0.006
Post × Exclusions	-0.434	0.395	-0.120	0.225
Pilot × Post × Exclusions	**0.734****	**0.339**	**0.794***	**0.442**
Sales Growth	0.074*	0.042	0.180**	0.070
Pilot × Sales Growth	0.035	0.060	0.001	0.096
Size	0.002*	0.001	-0.002*	0.001
Pilot × Size	0.001	0.001	0.001	0.002
Earnings Volatility	-0.133**	0.065	-0.033	0.058
Pilot × Earnings Volatility	0.065	0.113	-0.044	0.144
Loss	0.007*	0.004	0.007	0.005
Pilot × Loss	0.008	0.008	0.015*	0.009
BM	-0.030***	0.007	-0.048***	0.007
Pilot × BM	-0.004	0.012	0.002	0.012
Total Accruals	-0.152***	0.042	-0.368***	0.054
Pilot × Total Accruals	0.000	0.068	0.013	0.080
Post × Total Accruals	-0.006	0.056	0.064	0.066
Pilot × Post × Total Accruals	0.031	0.100	0.018	0.097
Industry Fixed Effects	Included		Included	
Quarter Fixed Effects	Included		Included	
Observations	5,174		5,177	
Adjusted R^2	0.731		0.802	
p-value of F-tests:				
Post period pilot firms' exclusions are transitory				
$\beta_4 + \beta_5 + \beta_8 + \beta_9 = 0$	*0.421*		*0.265*	
Post period control firms' exclusions are transitory				
$\beta_4 + \beta_8 = 0$	*0.000*		*0.000*	

Table 9 (cont'd)

Panel B: GAAP Earnings Management Measured by Abnormal Accruals

Dependent Variable	(1) *Future Operating Earnings*		(2) *Future Operating Cash Flows*	
	Coefficient Estimate	Standard Error	Coefficient Estimate	Standard Error
Pilot	-0.006	0.017	-0.001	0.021
Non-GAAP Earnings	3.121***	0.196	3.209***	0.173
Pilot × Non-GAAP Earnings	0.134	0.316	-0.145	0.322
Exclusions	-0.786***	0.277	-0.879***	0.332
Pilot × Exclusions	-0.227	0.153	-0.266	0.163
Post	0.000	0.004	0.001	0.005
Pilot × Post	-0.004	0.005	-0.010	0.006
Post × Exclusions	-0.387	0.292	-0.098	0.224
Pilot × Post × Exclusions	**0.679****	**0.337**	**0.765***	**0.445**
Sales Growth	0.074*	0.043	0.171**	0.072
Pilot × Sales Growth	0.022	0.058	-0.007	0.098
Size	0.002*	0.001	-0.002*	0.001
Pilot × Size	0.001	0.001	0.001	0.002
Earnings Volatility	-0.140**	0.066	-0.046	0.059
Pilot × Earnings Volatility	0.058	0.116	-0.040	0.151
Loss	0.007*	0.004	0.007	0.005
Pilot × Loss	0.008	0.008	0.014	0.009
BM	-0.035***	0.007	-0.050***	0.007
Pilot × BM	0.000	0.012	0.002	0.013
Abnormal Accruals	-0.167***	0.043	-0.366***	0.053
Pilot × Abnormal Accruals	0.019	0.073	0.036	0.083
Post × Abnormal Accruals	0.021	0.058	0.090	0.068
Pilot × Post × Abnormal Accruals	-0.004	0.103	-0.007	0.102
Industry Fixed Effects	Included		Included	
Quarter Fixed Effects	Included		Included	
Observations	4,756		4,759	
Adjusted R^2	0.731		0.802	
p-value of F-tests:				
Post period pilot firms' exclusions are transitory				
$\beta_4 + \beta_5 + \beta_8 + \beta_9 = 0$	*0.637*		*0.132*	
Post period control firms' exclusions are transitory				
$\beta_4 + \beta_8 = 0$	*0.000*		*0.002*	

This table presents results on changes in the quality of non-GAAP exclusions during the Pilot Program, considering GAAP earnings management measured by total accruals (Panel A) and abnormal accruals (Panel B). Standard errors are clustered by firm. ∗, ∗∗, and ∗∗∗ indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively. All variables are defined in Appendix A. All continuous variables are winsorized at the 1st and 99th percentiles.