Third-Party Consequences of Short-Selling Threats:

The Case of Auditor Behavior

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Abstract

Most prior research on short selling focuses on its impact on targeted firms. In contrast, this study examines how short-selling threats affect other market participants, in particular auditors. During 2005-2007, the SEC ordered a pilot program in which one-third of the Russell 3000 index firms were arbitrarily chosen as pilot stocks to be exempted from short-sale price tests. As a result, these pilot stocks faced significantly higher short-selling threats. We use this controlled experiment as our identification strategy and implement a difference-in-differences test to show that auditors react to the threats and charge higher audit fees to the pilot firms. Further, consistent with our predictions, we find that the impact only exists when auditors are concerned with the down-side risk caused by short sellers or when auditors have greater bargaining powers. The results are robust to numerous controls, alternative partition methods, firm fixed-effects estimation, and alternative model specifications. In additional analyses, we find that auditors are more likely to switch in the presence of short-selling threats (but only for clients with higher bankruptcy risk). This paper is among the first to document a third-party consequence of short-selling threats and to explore a specific cost of short-selling threats on shareholders (i.e., increased audit fees). In addition, we establish a causal impact of short-selling threats as a determinant of auditor behaviors.

Key Words: Short Selling, Audit Fees, Regulation SHO, Bankruptcy Risk, Earnings Management, Auditor Bargaining Power, Pilot Program (Rule 202T)

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1. Introduction

Short sellers are among the most sophisticated investors in capital markets. Because of their unique "business model" of profiting from price declines, they are often disliked by various market participants. Not surprisingly, there is considerable academic interest in the role of short sellers. However, extant research mainly focuses on short sellers' impact on the targeted firms and pays less attention to the possible influence on other market participants. This paper is intended to fill this gap in the literature.

We examine whether and how short-selling threats affect the behavior of auditors in terms of fees. Our focus on auditors is directly motivated by the potential litigation risk of auditors associated with short-selling activities. For example, after Sino Forest was shorted by Muddy Waters Research, a Hong Kong - based short seller, it filed for bankruptcy protection on March 30, 2012.[1] The auditor, Ernst & Young LLP, was accused of failing to meet industry standards through its audits with a sufficient level of professional skepticism and failing to perform sufficient work to verify the existence and ownership of Sino's most significant assets. Ernst & Young eventually agreed to pay a record $117 million to settle a shareholder class-action lawsuit related to the Sino Forest case.

Because short sellers benefit from declining stock prices, they have strong incentives to identify overpriced securities, such as those supported by inaccurate or fraudulent accounting numbers (e.g., Karpoff and Lou 2010). As short sales drive stock prices down to (arguably) their fundamental value, some investors lose money and often blame the loss on the auditors, resulting in higher litigation risk for auditors. To compensate for such increase in risk, the auditors in turn

[1] On June 2, 2011, shares in Sino Forest plummeted following the release of a negative research report by Carson Block of Muddy Waters Research, which made allegations that Sino Forest had been fraudulently inflating its assets and earnings, and that the company's shares were essentially worthless.

may ask for higher audit fees. Further, if the risk outweighs the compensation provided by the clients, we should also observe auditor switches.

Ex ante, however, it is not clear whether these predictions on audit fees (and switches) would be supported. First, if there is no opportunity for short sellers to exploit profits (due to regulatory constraints or high trading costs), short-selling *threats* will not be realized and will not matter either for auditors or their clients. Second, note that short selling exerts *ex ante* threats to managers. As a result, managers may take actions to reduce the probability of being targeted by short sellers. For example, Fang, Huang, and Karpoff (2015) show that short sellers' threats constrain managers' earnings-management incentives. In other words, the presence of short-selling threats could potentially reduce firms' propensity of misreporting and consequently decrease the litigation risk of auditors. Third, if the auditors do not have enough credibility and bargaining power to justify the higher fees, we would not observe increased audit fees. We explore these counter-arguments in cross-sectional analyses discussed in detail later.

It is inherently difficult to identify the effect of short selling on auditor behavior because of endogeneity concerns. For example, a positive association between short interest and audit fees could be alternatively interpreted as both short interest and audit fees being driven by correlated omitted variables, such as the risk of the clients. To avoid such endogeneity concerns, we employ an exogenous shock to short-selling threats. To facilitate research on the effects of short-sale price tests in financial markets, the SEC initiated a pilot program under Rule 202T of Regulation SHO in July 2004. The price tests are designed to limit short selling in a declining market, thus setting a substantial barrier for short sellers as they profit from declining prices.[2] Under the pilot

[2] Short-sale price tests include the tick test for exchange-listed stocks and the bid test for Nasdaq National Market Stocks. The tick test (Rule 10a-1) requires that a listed security be sold short (1) at a price above the price at which the immediately preceding sale was effected (plus tick), or (2) at the last sale price if it is higher than the last different price (zero-plus tick). The bid test (Rule 3350) requires that a listed security be sold short at a price one

program, every third stock in the Russell 3000 index ranked by trading volume in each exchange (i.e., NYSE, NASDAQ, and AMEX) was selected as a pilot stock. From May 2, 2005 to August 6, 2007, pilot stocks were exempted from short-sale price tests. Diether, Lee, and Werner (2009) show that pilot stocks listed on NYSE and NASDAQ experienced a significant increase in both short-sale trades and short sales-to-share volume ratio during the pilot program period.

We use this controlled experiment to examine whether and how short-selling threats affect auditor behaviors. We adopt a difference-in-differences design, assigning the pilot firms as our treatment sample and non-pilot firms as the control sample. Our empirical analyses show that, on average, pilot firms have larger increase in audit fees during the pilot program compared to non-pilot firms, indicating that short-selling threats increase audit fees. In terms of economic significance, the exemption of short-sale price tests leads to overall $216 million extra cost of auditing for the 986 pilot firms from 2005 to 2007.

Motivated by the tension discussed above, we then explore cross-sectional variations in the impact of short-selling threats on audit fees. Specifically, we first investigate whether the effect is greater when auditors care more about the down-side risk caused by short sellers, as indicated by the client's bankruptcy risk or their upward earnings-management behavior. Next, we consider the role of the auditor's bargaining power vis-à-vis client firms, as measured by whether the auditor is the industry leader at the city-level. Consistent with our predictions, we find that the impact of short-selling threats is highly significant for client firms with higher downside risk or firms with auditors with stronger bargaining power. In contrast, the effect is not significant for firms with lower downside risk or with auditors with weaker bargaining power. These subsample analyses thus provide strong evidence supporting the impact of short-selling threats on audit fees.

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penny above the bid price if the bid is a downtick from the previous bid.

We conduct several robustness checks to ensure that the inferences are not sensitive to our research-design choices. First, we use alternative variables to partition the full sample in the sub-sample analyses. Second, we use firm fixed-effects estimation to control for time-invariant omitted variables. Third, we show that short interest cannot explain away our results. Finally, we scrutinize SEC comment letters and conclude that our results are unlikely to be driven by the possibility that the SEC increased scrutiny of pilot firms during the pilot program.

In addition, we conduct several sets of supplemental analyses. First, we show that short-selling threats increase the probability of auditor switch when the clients have higher bankruptcy risk. Also, we find no impact of short-selling threats on tax-service fees, which are arguably irrelevant to litigation risk caused by short sellers.[3] Second, we combine the cross-sectional analyses together and show that the impact of short-selling threats on audit fees is more prominent for clients with *both* high down-side risk and powerful auditors. Third, we identify risk premium and increased audit effort as two channels through which litigation risk affects audit fees, and our exploratory analyses suggest that risk premium is likely the main channel.

This paper contributes to the literature by being among the first to examine third-party consequence of short-selling threats. Numerous studies have examined the impact of short selling and its regulation on the targeted firms. Unlike prior studies, our research shows that the impact extends to other related market participants, such as auditors. In addition, this paper highlights a considerable cost to shareholders imposed by short-selling threats – the increase of audit fees and likelihood of auditor switches. In this way, our findings also have implications for regulators' evaluations of costs and benefits of future regulations on short selling. Finally, this

[3] This analysis serves as a placebo analysis in that we do not find any effect in a setting in which we do not expect to observe any impact.

article establishes the causal impact of short-selling threats on auditor behaviors. Using the pilot program as a controlled experiment, we are able to draw the causal inference that short-selling threats are a determinant of auditor behaviors.

The next section reviews the related literature and develops the hypotheses. Section 3 describes the research design and the sample. Section 4 reports the results for the primary analyses as well as three sets of cross-sectional analyses. Section 5 presents results of robustness checks. Section 6 includes several sets of supplemental analyses. Finally, Section 7 concludes.

2. Literature and Hypotheses

2.1 Literature on the Impact of Short-Selling Threats in the Capital Markets

Short sellers are arguably the most sophisticated players in the capital markets. For example, prior research indicates that they are more informed than financial analysts (Christophe, Ferri, and Hsieh 2010; Drake, Rees, and Swanson 2011) and can front-run insider trading (Khan and Lu 2013).

As short sellers can act as an information intermediary in the capital markets (Pownall and Simko 2005), many studies have examined the impact of short selling on asset pricing and market efficiency. For example, Aitken, Frino, McCorry, and Swan (1998) use intraday data and show that short sales are instantaneously bad news. Such sales can help impound adverse information into stock prices within fifteen minutes. Dechow, Hutton, Meulbroek, and Sloan (2001) confirm that short sellers are sophisticated traders in that they use fundamental analysis to exploit the lower expected future return of firms with lower ratio of fundamentals to market values. Jones and Lamont (2002) use data from 1926 to 1933 and find that stocks that are expensive to short have high valuation and low subsequent returns, suggesting that stocks can be

overpriced when short-sale constraints bind. Chang, Cheng, and Yu (2007) reach a similar conclusion using Hong Kong stock-market data. Diether, Lee, and Werner (2009) use the SEC SHO project and conclude that price tests distort order flow created by the price tests themselves.

Several recent studies use the SEC's Regulation SHO pilot program for identification strategy as we do in this paper. Two papers show that short-selling threats can affect corporate real decisions. Grullon, Michenaud, and Weston (2015) conclude that an increase in short-selling activities causes small firms to reduce equity issues and investment. Similarly, He and Tian (2014) find that short-selling pressure can affect corporate innovation efficiency.

Short-selling threats can also affect corporate accounting decisions. Fang et al. (2015) find that the reduction in short-selling costs emanating from SHO disciplines managerial opportunistic reporting behavior and reduces earnings management. Li and Zhang (2015) document that managers respond to the decrease of short-selling costs by reducing the precision of bad-news forecasts and the reliability of bad-news annual reports. The explanation they provide is that managers want to maintain the current level of stock price and therefore disclose strategically.

Another two papers show that short selling can affect corporate governance and the contracting process inside the firm. Chang, Lin, and Ma (2014) show that short selling can discipline managerial empire building, for example, by reducing abnormal capital investment. De Angelis, Grullon, and Michenaud (2015) show that firms with increased short-selling pressure are more likely to reduce managerial exposure to downside risk by granting relatively more stock options to top executives and adopting new anti-takeover provisions.

Overall, these interesting short-selling studies focus on the impact of short sellers on the targeted firms. Also, the majority of prior research emphasizes the "bright sides" of short sellers,

such as improving pricing efficiency and disciplining managers. Our paper extends the extant literature by examining the impact of short-selling threats on other market players (i.e., auditors) and by exploring one specific cost of short-selling threats on shareholders (i.e., increased audit fees).

2.2 Litigation Risk as a Determinant of Audit Fees

DeFond and Zhang (2014) and Hay, Knechel, and Wong (2006) provide comprehensive reviews of audit-fee research. These articles explain how audit fees are the outcome of both supply and demand factors. In order to control for known determinants of audit fees, we build on Francis, Reichelt, and Wang (2005) and Gul and Goodwin (2010) as well as the review studies and include numerous client and auditor characteristics in our regression models (see Section 3.2 for details).

According to DeFond and Zhang (2014, 297), "Litigation damage claims against auditors can be large enough to threaten the viability of even the largest audit firms." They summarize four strategies auditors use to counter litigation threats: (1) reduce risk by increasing effort (e.g., Simunic 1980); (2) bear risk by charging a risk premium (e.g., Bell, Doogar, and Solomon 2008); (3) avoid risk through client retention and acceptance (e.g., Bedard and Johnstone 2004); and (4) attenuate risk through lobbying for reduced legal liability (Geiger and Raghunandan 2001).

Note that the first two strategies lead to higher audit fees and they are inherently difficult to distinguish in archival research as we usually do not have data on audit effort (e.g., work hours). Nevertheless, prior research concludes that higher litigation risk is associated with higher fees. These studies usually compare the audit fees for clients in different environments with high versus low litigation risk, such as public vs. private firms (Simunic and Stein 1996), IPO years

versus non-IPO years (Venkataraman, Weber, and Willenborg 2008), and U.K. firms cross-listed in the U.S. vs. listed in the U.K. (Seetharaman, Gul, and Lynn 2002), non-U.S. firms cross-listed in countries with stronger legal regimes vs. cross-listed in countries with weaker legal regimes (Choi, Kim, Liu, and Simunic 2009), and public-equity firms vs. private-equity firms with public debt (Badertscher, Jørgensen, Katz, and Kinney 2014).

One common challenge for this type of research is that firms self-select to an environment with high (or low) litigation risk (e.g., Minutti-Meza 2014). Although various methods are employed in the literature to address this issue, the endogeneity concern caused by self-selection cannot be fully eliminated. Our study thus also complements extant research on litigation risk, as the pilot firms are arbitrarily chosen by the SEC and therefore the consequent variation in litigation risks is exogenous and does not suffer from the endogeneity problem.

2.3 Relation between Auditing and Short Selling

To the best of our knowledge, only two prior papers examine the relation between short selling and auditing. Blau, Brough, Smith, and Stephens (2013) examine whether and how short sellers profit from auditor changes. They find that short sellers can generate significant returns by shorting firms with "bad news" auditor changes. Their paper is different from ours as they focus on how short sellers react to auditing events while we focus on how auditors react to short-selling regulations.

Cassell, Drake, and Rasmussen (2011) argue that short interest is a signal of audit risk and find a positive association between short interest and audit fees. Our paper complements theirs but is different for at least two major reasons. First, the research question is inherently different. In Cassell et al. (2011), short sellers are *a source of risk-related information* to auditors, while

this study examines whether short sellers are *a source of risk per se* to auditors. In other words, their focus is on short sellers' role of collecting risk information associated with the firm, but our focus is the short sellers' treatment effect in increasing auditors' litigation risk. Second, the research design is different. Cassell et al. (2011) rely on the association between short interest and audit fees, so "it is difficult to establish a causal link" (Willekens 2011, 1302).[4] We use the SEC SHO pilot program that arbitrarily removes short-selling restrictions of one third of Russell 3000 index firms to establish causality.[5]

2.4 Hypotheses Development

Prior research and anecdotal evidence show that auditors can be sued by investors or regulators because of their mistakes in the auditing process. Assume the auditors' probability of committing a mistake that would inflate earnings is θ and the litigation risk is $LR(\theta)$. Clearly $LR(\theta)$ is an increasing function of θ. Now consider the role of short sellers. Short sellers are among the most sophisticated players in the capital markets and profit from price declines. As their short-selling activities drive down the stock price of the targeted firm by incorporating bad news more quickly, investors who suffer from the price decline are likely to sue the firm's auditor if any audit-related errors are found to associate with the price decline. Therefore, the existence of short-selling threats will amplify the auditors' litigation risk if auditors make mistakes. For example, the auditors' mistakes might not have been identified if there were no

[4] It is important to note that the causal inference in Cassel et al. (2011) is different from that in our paper. They are interested in establishing that auditors use short interest as a risk signal, but they need to rule out the possibility that both auditors and short sellers use a common underlying information set about risk. Also, as Cassel et al. (2011) acknowledge, changes analyses (i.e., the association between changes in short interest and changes in audit fees) are also only suggestive, because it is possible that both the change of short interest and the change of audit fees are driven by correlated omitted variables related to the changes of risk.

[5] We control for short interest in one of our robustness tests.

short sellers.[6] Indeed, Fang et al. (2015) show that short sellers increase the scrutiny of the pilot firms' reporting behavior. More specifically, they find that the short interest goes up in months in which firms are found to have engaged in financial misrepresentation. In other words, given θ, we have LR(No Threat | θ) < LR(Threat | θ), where *No Threat* indicates that short sellers are constrained and *Threat* indicates short sellers are not constrained (or less constrained). From the auditors' perspective, they would react to the increased litigation risk by charging higher fees, which reflect either increased audit effort or a risk premium, or both (Simunic and Stein 1996). Therefore, our first hypothesis (stated in the alternative form) is as follows:

Hypothesis 1 (Audit Fees): Audit fees increase in response to the increase in short-selling threats.

Next, we consider three cross-sectional hypotheses. All three hypotheses are related to the null hypothesis of H1 regarding reasons why it is possible that short-selling threats would *not* lead to higher audit fees. The first two relate to the degree of down-side risk caused by short sellers and the third cross-sectional hypothesis explores auditors' relative bargaining power.

First, for financially healthy firms, the auditors' (moderate) mistakes are unlikely material enough to cause a sufficiently large price decline that can cover the (often significant) short-selling costs. In other words, the threat of short sellers would not be realized as taking a short position is not an optimal decision. As short sellers are not likely to take action, their threat

[6] For example, on May 15, 2002, the hedge-fund manager David Einhorn alleged that Allied Capital inflated the value of its assets. Allied capital's stock price dropped almost 20% when the market opened on May 16, 2002. Six days later, the law firm Kirby McInerney & Squire LLP commenced a class-action lawsuit against the management of Allied Capital and its auditor (Arthur Andersen) using the same allegations as David Einhorn (see http://globenewswire.com/news-release/2002/05/22/287485/27761/en/Kirby-McInerney-Squire-LLP-Commences-Class-Action-Lawsuit-on-Behalf-of-Allied-Capital-Corp-Investors-ALD.html).

would not matter for the auditors' litigation risk. In this case, we will have LR(No Threat | θ) = LR(Threat | θ). On the other hand, for those firms with higher bankruptcy risk, short sellers are likely to take action as they have higher possibility to profit. Also, as investors' losses related to firms' bankruptcies are usually significant, investors often turn to auditors, whose "deeper pockets" could be worth their effort in initiating litigation fights.[7] In other words, when the bankruptcy risk is high, LR(No Threat | θ) < LR(Threat | θ). Therefore, hypothesis 2.1 (stated in the alternative form) is as follows:

Hypothesis 2.1 (Bankruptcy Risk): The impact of short selling on audit fees is more pronounced for firms with higher bankruptcy risk.

Fang et al. (2015) show that managers reduce their upward earnings management when faced by greater threats from short sellers. Anticipating such reactions, *ex ante* the short sellers may be less likely to profit from firms whose managers are disciplined. If they take no short positions on these firms, other investors may be less likely to lose money and sue auditors. As a result, the auditors' litigation risk would also not increase accordingly. In other words, we have LR(No Threat | θ) = LR(Threat | θ). In contrast, for those firms whose managers are not disciplined (i.e., who keep inflating earnings upward), we will have LR(No Threat | θ) < LR(Threat | θ) as short sellers are more likely to take short positions in those firms.[8] Therefore, hypothesis 2.2 (stated in the alternative form) is as follows:

[7] Another way of thinking about this is that short sellers may discipline auditors (just like they discipline managers as per Fang et al. 2015) for catching material and marginally material mistakes, and this is especially true for firms that are at the edge of bankruptcy.

[8] Another way of thinking about this is that the accruals are a joint decision by managers and the auditor. As a result, auditors are more likely to be accused of "inflating the earnings to mislead investors" when discretionary accruals are positive than when they are negative.

Hypothesis 2.2 (Disciplined Managers): The impact of short selling on audit fees is more pronounced for firms whose managers are not disciplined by the short sellers.

Finally, we examine the role of the auditor's relative bargaining power as proxied by their city-level industry-market share. As DeFond and Zhang (2014, 297) point out, charging higher fees to counter litigation risk "requires the client's willingness to pay for those fees." This is particularly important in our setting, because the threats from short sellers are unobservable *ex ante* and even unverifiable *ex post* (i.e., it is possible that short sales do not increase *ex post* even though short selling becomes much easier). As a result, whether auditors can charge higher fees in the presence of short-selling threats depends on how much they can persuade their clients, either by credibility or power, or both.

Industry-leader auditors have both higher credibility (or reputation backed by their industry expertise) and superior bargaining power (Numan and Willekens 2012). This suggests they can use the presence of short-selling threats as evidence to ask for a higher price. As a result of such bargaining power, the audit fees charged by industry-leader auditors should increase in the presence of short-selling threats. Our final hypothesis, H2.3 (stated in the alternative), is as follows:

Hypothesis 2.3 (Auditors' Bargaining Power): The impact of short selling on audit fees is more pronounced for auditors with greater bargaining power.

However, we note that there is also support for the null of this hypothesis. That is, to the extent that industry-leader auditors are more experienced, they are less likely to make material mistakes that can attract short sellers. If this effect dominates, the audit fees charged by industry-leader auditors would not increase in the presence of short-selling threats.

The tension in the theory is complicated by the endogeneity in the empirical tests for all hypotheses. The key challenge is that both auditor behaviors and short-selling activities are associated with the clients/targeted firms' risks. In the next section, we illustrate our research design, which is dedicated to capturing the causal effect.

3. Research Design

3.1 Identification Strategy

To capture the impact of short-selling threats on auditors, we need to observe variations in short-selling threats but hold firm risk (that is not caused by short selling) constant. The SEC SHO pilot program provides a controlled experiment that we employ as our identification strategy.

To facilitate research on the effects of short-sale price tests on financial markets, the SEC initiated a pilot program under Rule 202T of Regulation SHO in July 2004. Under the pilot program, Russell 3000 index stocks (as of June 25, 2004) were sorted into three groups – AMEX, NASDAQ, and NYSE – and ranked within each group from highest to lowest by the average daily dollar volume over the last 12 months prior to July 28, 2004.[9] In each group, every third stock was selected as a pilot stock. From May 2, 2005 to August 6, 2007, pilot stocks were

[9] Those stocks not listed on AMEX, NASDAQ, or NYSE were excluded as short sales in these securities were not subject to a price test. Also, the SEC excluded issuers whose initial public offerings commenced after April 30, 2004. More details can be found at SEC's "Order Suspending the Operation of Short Sale Price Provisions for Designated Securities and Time Periods" (Release No. 3450104; July 28, 2004).

exempted from short-sale price tests. Subsequent to the pilot program, on July 6, 2007, the SEC eliminated short-sale price tests for all exchange-listed stocks. This controlled experiment provides an ideal setting to examine the impact of short-selling threats on auditor behavior as both relevance and exogeneity requirements are clearly satisfied. According to the SEC (2007) and Diether et al. (2009), the exemption reduced the short-selling costs and increased the short-selling prospects for pilot firms significantly. Equally important, the pilot program represents a truly exogenous shock to the cost of selling short in the affected firms. As Fang et al. (2015) explain, there is no evidence that the firms themselves lobbied for the pilot program, or that any individual firm could foresee being in the pilot group until the program was announced.

3.2 Models and Variables

Employing a difference-in-differences design, we estimate the following model for H1.

$$\begin{aligned} LnAF_{i,t} = {} & \alpha_0 + \alpha_1 Pilot_{i,t} + \alpha_2 During_{i,t} + \alpha_3 Pilot_{i,t} \times During_{i,t} + \alpha_4 Post_{i,t} + \alpha_5 Pilot_{i,t} \times Post_{i,t} \\ & + \alpha_6 Size_{i,t} + \alpha_7 Lev_{i,t} + \alpha_8 BTM_{i,t} + \alpha_9 ROA_{i,t} + \alpha_{10} Loss_{i,t} + \alpha_{11} AbsAccr_{i,t} \\ & + \alpha_{12} CA/TA_{i,t} + \alpha_{13} Quick_{i,t} + \alpha_{14} INVREC_{i,t} + \alpha_{15} SizeGr_{i,t} + \alpha_{16} LIT_{i,t} \\ & + \alpha_{17} FYEnd_{i,t} + \alpha_{18} NBusSeg_{i,t} + \alpha_{19} GCO_{i,t} + \alpha_{20} BIG4_{i,t} + \alpha_{21} MNC_{i,t} \\ & + \alpha_{21} Switch_{i,t} + \alpha_{21} Andersen_{i,t} + IND_i + \varepsilon_{i,t} \end{aligned} \tag{1}$$

We use OLS to estimate the impact of short-selling threats on audit fees (H1). *LnAF* is defined as the log of audit fees in dollars. Our sample period is from 2000 to 2012.[10] *Pilot* is an indicator variable equal to one for all observations of firms that were arbitrarily picked by SEC as pilot firms (zero otherwise). *During* is one for observations from year 2005 to 2007 and zero

[10] Audit fee data are available starting in 2000. If we instead end the sample period in 2010 (as in Fang et al. 2015), no inferences are affected.

otherwise; *Post* is one for observations from year 2008 to 2012 and zero otherwise. As the pilot firm list was announced on July 28, 2004 but the price tests were not removed for pilot firms until May 2, 2005, it is unclear ex ante whether auditors reacted in year 2004. Therefore, we follow Fang et al. (2015) and exclude all observations in year 2004.[11] Note the benchmark is the period before the pilot program (i.e., years before 2004). H1 is supported if α_3 is significantly positive and α_5 is insignificant, indicating that benchmarking on non-pilot firms, audit fees increase more for pilot firms from the Pre-pilot to the During-pilot period, but not more from the Pre-pilot to the Post-pilot period.

Following prior literature on auditor behavior (e.g., Francis et al. 2005; Gul and Goodwin 2010), we include a number of control variables: firm size (*Size*), Leverage (*Lev*), Book-to-Market ratio (*BTM*), profitability (*ROA*), indicator for loss (*Loss*), total accruals (*AbsAccr*), the ratio of current asset to total assets (*CA/TA*), quick ratio (*Quick*), the ratio of inventory and accounts receivable to total assets (*INVREC*), growth of total assets (*SizeGr*), indicator for litigation-intensive industry (*Litigation*), indicator for fiscal year not ending in December (*FYEnd*), number of business segments (*NBusSeg*), indicator for going-concern opinion (*GCO*), whether there is an auditor switch during the year (*Switch*), indicator for being audited by Big 4 auditors (*BIG4*), indicator for being a multinational company (*MNC*), and whether the firm is audited by Arthur Andersen (*Andersen*). In addition, we control for general industry and time-period effects through the inclusion of SIC 2-digit industry and year fixed effects.[12] All variables are defined in the Appendix.

[11] Conclusions are unaltered if we include 2004 in our treatment sample (or in our control sample).
[12] As *Litigation* is defined at the 4-digit SIC-code level we can include both *Litigation* and industry fixed effects.

3.3 Sample Selection

Table 1 illustrates our sample-selection procedures. We start from the 2004 Russell 3000 index firms and identify those firms arbitrarily selected (i.e., every third stock as ranked by trading volume) by SEC as pilot firms. Following prior studies using this setting, we require that firms also be included in the 2005 Russell 3000 index and listed on NYSE, NASDAQ, or AMEX. Also, we exclude firms in financial and utilities industries. After merging with all regression variables, we have 13,323 firm-year observations for 1,485 individual firms, including 494 pilot firms and 991 non-pilot firms. In the full sample, 726 firms (48.89%) are listed on NYSE, 744 firms (50.10%) on NASDAQ, and 15 firms (1.01%) on AMEX.

All accounting variables are from Compustat and audit-related variables are from Audit Analytics. All continuous variables are winsorized at the 1st and 99th percentiles. The reported t-statistics are based on standard errors clustered by firm and year (Petersen 2009).

4. The Impact of Short Selling on Audit fees

4.1 Summary Statistics

Table 2 summarizes the variables used in the main test. Panel A provides statistics for the full sample and these are generally consistent with what prior research reports. Sample firms pay their auditors 2.123 million dollars on average each year in audit fees. Panel B compares pilot firms with non-pilot firms before the announcement of the pilot program (i.e., year 2003). Pilot firms and non-pilot firms are generally indistinguishable in terms of firm-level characteristics. The *only* difference that is significant at the 10% level is audit fees: non-pilot firms have larger audit fees than pilot firms before the reform. Note that such a difference in 2003 (and also in

previous years, see Table 4 and Figure 1) is exogenous to the treatment effect in our setting as the SEC arbitrarily chose pilot firms in 2004.[13]

Table 3 provides a Pearson correlation matrix for all variables used in the main analyses. Note that the negative correlation between *LnAF* and *Pilot* does not necessarily contradict our prediction, as it is merely a reflection of the difference in audit fees as indicated in Table 2. For the control variables, we can find that audit fees are positively correlated with *SIZE, LEV, ROA, InvRec, NBusSeg, BIG4,* and *MNC* and negatively correlated with *Loss, AbsAcc, CA/TA, Quick, SizeGr, Litigation, GCO, Switch,* and *Andersen*. These correlations are generally consistent with prior literature.

4.2 Univariate Analyses

Table 4 presents the year-by-year comparison of audit fees between pilot firms and non-pilot firms. Consistent with Panel B of Table 2, the average audit fees are higher in the non-pilot sample than in the pilot sample every year. Importantly, the differences are insignificant almost *only* during the pilot program (i.e., during 2005-2007). Also, the differences during 2005 to 2007 are the smallest in magnitude. Figure 1 illustrates the difference in *LnAF* between non-pilot firms and pilot firms and the p-value of t-tests by year. We see that the bars (i.e., the difference magnitude) are lowest during 2005 to 2007, but the p-values are highest during those three years. Table 4 and Figure 1 suggest that audit fees of pilot firms increase after the removal of short-selling restrictions such that the fee difference between pilot firms and non-pilot firms

[13] Consistent with other papers using the same setting (e.g., Grullon et al. 2015; De Angelis et al. 2015; He and Tian 2014), the non-pilot firms in our final sample are on average 6% larger than pilot firms (although the difference is insignificant). This could partially explain why the non-pilot firms have larger audit fees than pilot firms in year 2003.

narrows down. This inference is consistent with H1 from the difference-in-differences perspective.

4.3 Full Sample Regression Analyses (H1)

Panel A of Table 5 presents the regression results for testing H1. Column 1 shows the results using Equation 1. The adjusted R^2 is around 82% and most control variables have the same signs as in previous research. The coefficient on *Pilot* is significantly negative, confirming the previous observation that non-pilot firms have higher audit fees than pilot firms. The coefficients for both *During* and *Post* are significantly positive, indicating that the average audit fees in the *During* and *Post* periods are higher (in fact more than 140% higher) than the fees in the pre-pilot period. As prior literature discusses, a primary explanation for this substantial increase in audit fees is SOX (e.g., Ghosh and Pawlewicz 2009). These observations also illustrate the importance of using a differences-in-differences approach to control for trend-related factors.

More importantly, the coefficients of primary interest are α_3 *(Pilot* × *During*) and α_5 (*Pilot* × *Post*). We find that α_3 is significantly positive at the 0.01 level (using two-sided tests) but α_5 is insignificant, indicating that benchmarking on non-pilot firms in the pre-pilot period, pilot firms have a larger increase in audit fees during the pilot program, but do not have larger increase in the post-pilot period.

It is worth noting that the impact of short-selling threats on audit fees (α_3=0.0405) is not only statistically significant, but also economically meaningful. Benchmarking on non-pilot firms, the larger increase in audit fees suggests that pilot firms could have saved a total of 216

million dollars in audit fees had the price test been not exempted.[14] These are wealth transfers from investors to auditors because of the short-selling threats.

In Column 2, we include year fixed effects from 2001-2003 and from 2005-2012, but exclude *During*, *Post*, and the fixed effect for 2001 (the benchmark year) to avoid multicollinearity. With this specification, α_3 *(Pilot* × *During*) is significantly positive at the 0.05 level and α_5 (*Pilot* × *Post*) is insignificant. Also, the magnitudes of all coefficients are similar to their counterparts in Column 1. These results show that the increase in short-selling threats for pilot firms during 2005 to 2007 leads to higher audit fees for these firms, providing support for H1.

4.4. Cross-Sectional Variations

In this section we test H2.1-H2.3 by exploring whether there are cross-sectional variations in the impact of short selling on audit fees.

4.4.1 Bankruptcy Risk (H2.1)

To test H2.1, we examine whether the impact of short-selling threats on audit fees varies with the client firms' bankruptcy risk. H2.1 is supported if the impact of short-selling threats on audit fees is larger for firms with higher bankruptcy risk. We use the Altman (1968) model to model bankruptcy risk. Specifically, we construct a bankruptcy-risk measure (*AltmanZ*) based on the following equation:

[14] 4% × average audit fees of 1.822 million for pilot firms in 2004 × 986 pilot firms × 3 years in pilot program. Note that when the coefficient is small (e.g., here 0.0405), we can interpret the coefficient of a log-linear model as the percentage of change in the dependent variable.

$$AltmanZ = 1.2X_1 + 1.4X_2 + 3.3X_3 + 0.6X_4 + 1.0X_5 \quad (2)$$

where X_1 is the ratio of working capital to total assets, X_2 is the ratio of retained earnings to total assets, X_3 is the ratio of earnings before interest and taxes to total assets, X_4 is the ratio of market value of equity to the book value of total debt, and X_5 is the ratio of sales to total assets.

We partition the whole sample based on the sample median of *AltmanZ* into the High Bankruptcy-Risk subsample and Low Bankruptcy-Risk subsample. H2.1 predicts that the impact of short-selling threats on audit fees only exists (and would be larger) in the High Bankruptcy-Risk subsample.

Columns 1 and 2 in Panel B of Table 5 present the results. We find that the coefficient of *Pilot* × *During* is significant at the 0.01 level (Coeff. = 0.0912 and t = 3.55) for the High Bankruptcy-Risk subsample but not significant (Coeff. = -0.0040 and t = -0.18) for the Low Bankruptcy-Risk subsample. In addition, a Chi-square test shows that the difference between these two coefficients is significant (p-value = 0.006).[15] By contrast, the coefficient on *Pilot* × *Post* is not significant in either subsample. These results support H2.1 that the impact of short-selling threats on audit fees only exists for clients with considerable bankruptcy risk.

4.4.2 Disciplined Managers (H2.2)

H2.2 predicts that the impact of short-selling threats on audit fees is weaker if managers of the client firms take serious action to address the threat. As discussed, Fang et al. (2015) show that pilot firms reduced upward earnings management during the pilot program. They interpret this finding to imply that the pilot program reduced the cost of short selling sufficiently so that it

[15] Alternatively, we add a three-way interaction to the regression to test the differences between coefficients from subsamples. Our inferences remain.

increases potential short-sellers' monitoring activities among the pilot firms. In turn, such increased monitoring by short sellers led to a decrease in the pilot firms' earnings management. Following Fang et al.'s (2015) logic, we would expect that the impact would only exist and be larger for firms that are exposed to higher litigation risks with upward earnings management (i.e., among firms that are not disciplined by short sellers).

We follow McNichols (2002) to model normal accruals. Specifically, we consider the variables in both Jones (1991) and Dechow and Dichev (2002) in determining normal accruals.

$$TA_{j,t} = \phi_{0,j} + \phi_{1,j} CFO_{j,t-1} + \phi_{2,j} CFO_{j,t} + \phi_{3,j} CFO_{j,t+1} + \phi_{4,j} \Delta Rev_{j,t} + \phi_{5,j} PPE_{j,t} + \nu_{j,t} \qquad (3)$$

where *TA* is total accruals measured as income before extraordinary items minus *CFO*, *CFO* is net cash flow from operations excluding extraordinary items, ΔRev is the change in revenues, and *PPE* is the gross value of PPE. All variables are scaled by lagged total assets. We estimate this cross-sectional model within each Fama-French 48 industry-year intersection with at least ten observations. The residual is labeled as discretionary accruals (DA).

We partition the full sample based on whether DA is positive (i.e., DA > 0) or negative (i.e., DA < 0). H2.2 predicts that the impact of short-selling threats on audit fees only exists and would be larger for the subsample with DA > 0.

Columns 3 and 4 in Panel B of Table 5 present the results. We find that the coefficient of *Pilot* × *During* is significant at the 0.01 level (Coeff. = 0.0990 and t = 3.36) for the DA > 0 subsample but not significant (Coeff. = -0.0068 and t = -0.37) for the DA < 0 subsample. The Chi-square test shows that the difference between these two coefficients is also highly significant (p-value =0.002). Again, the coefficient of *Pilot* × *Post* is not significant in either subsample.

These results support H2.2 that the impact of short-selling threats on audit fees only exists for clients whose managers did not respond to the prospect of increased scrutiny from short sellers by decreasing their earnings management.

4.4.3 Auditors' Bargaining Power (H2.3)

H2.3 refers to the potential moderating role of auditors' bargaining power on the impact of short-selling threats on audit fees. As auditors are typically organized by city-level offices, we follow prior literature and focus on auditors' city-level industry-market share (e.g., Francis et al. 2005). Specifically, we rank each auditor's market share (in terms of audit fees) in a given SIC 2-digit industry in a given city and label the auditor with the largest market share as the city-level industry leader. These are the auditors with the greatest bargaining power vis-à-vis their clients (Numan and Willekens 2012). All other auditors are labeled as non-industry leaders (and thus with lower bargaining power). Again following Francis et al. (2005), we require each city-industry intersection to have more than two firms.

Columns 5 and 6 in Panel B of Table 5 present the results. We find that the coefficient on *Pilot* × *During* is significant at the 0.05 level (Coeff. = 0.0760 and t = 2.22) for auditors with stronger bargaining power. This is consistent with the bargaining-power argument that industry-leader auditors use the presence of short sellers to ask for a higher risk premium. In contrast, the coefficient is negative and significant at the 0.10 level for auditors without industry leadership. The Chi-square test shows that the difference between these two coefficients is highly significant (p-value = 0.006). Again, the coefficient of *Pilot* × *Post* is not significant in either subsample. These results support the argument that the influence of short-selling threats on audit

fees is larger for clients whose auditors are industry leaders and therefore have larger bargaining power, thus supporting H2.3.

5. Robustness Checks

We conduct several sets of robustness tests to check whether our conclusions are sensitive to research-design choices.

5.1 Alternative Partition Methods

To confirm that our sub-sample inferences are not sensitive to the specific partition variables, we use alternative approaches dividing the full sample. For measure bankruptcy risk, we alternatively construct Ohlson (1980) bankruptcy scores.[16] Firms with higher score have higher probability of bankruptcy. We partition the sample into Low Bankruptcy (lower than sample median) and High Bankruptcy (higher than sample median).

For DA, we instead employ the Modified Jones Model (Dechow et al. 1995) and estimate Equation (3) without the three CFO variables. Also, we adjust for the change in account receivables when calculating non-discretionary accruals. Then we partition our sample according to the sign of the Modified Jones Model Discretionary Accruals (i.e., MJMDA > 0 vs. MJMDA < 0).

For bargaining power, we alternatively focus on the client's bargaining power (rather than the auditor's power in the main analyses). Following prior literature such as Casterella, Francis,

[16] Specifically, we construct the bankruptcy score based on Model 1 (Ohlson 1980, 121) as follows: Ohlson= -1.32 - 0.407SIZEA + 6.03TLTA - 1.43WCTA + 0.0757CLCA - 2.37NITA - 1.83FUTL + 0.285INTWO - 1.72OENEG - 0.521CHIN, where SIZEA is the log of total asset adjusted by GNP price-level index. TLTA is total liabilities divided by total assets. WCTA is working capital divided by total assets. CLCA is the current liabilities divided by current assets. NITA is net income divided by total assets. FUTL is the net operating cash flow divided by total liabilities. INTWO is one if net income was negative for the last two years and zero otherwise. OENEG is one if the total liabilities exceeds total assets and zero otherwise. CHIN is the change in net income (NI), measured as $(NI_t - NI_{t-1})/(|NI_t| + |NI_{t-1}|)$.

Lewis, and Walker (2004), we measure the client bargaining power with the relative size of the client: the proportion of the client's annual sales to all clients' aggregate annual sales with the same auditor. If the proportion is smaller (larger) than sample median, the firm is classified as in the subsample of Low (High) Client Power.

The results are presented in Table 6, Panel A. We observe that the coefficients of *Pilot×During* are significant in Column 1 at the 0.05 level for firms with lower bankruptcy risk based on Ohlson (1980), significant in Column 3 at the 0.01 level for firms with positive DA based on the Modified Jones Model, and significant in Column 5 at the 0.10 level for firms with lower bargaining power.[17] Overall, these results suggest that our main inferences are not contingent on the partition variables we choose.

5.2 Firm Fixed-Effects Estimation

To control for potential time-invariant firm-level omitted variables, we control for firm-fixed effects in Panel B of Table 6. Note that the fixed-effects specification alters the interpretation relative to Equation (1), as we only focus on within-firm variations. In this way, we may in fact wrongly disregard meaningful variations between firms. Indeed, the significance levels of the coefficient of interest are generally smaller than those in Table 5. However, the inferences remain the same even with this strong econometric specification that controls completely for unknown firm characteristics: the coefficients of *Pilot×During* are significant at the 5% level for firms with higher bankruptcy risk and significant at the 10% level for the full sample and for firms with positive DAs (using two-sided tests). Also, the differences between subsamples are all in the

[17] In contrast, the coefficients in Columns 2, 4, and 6 are insignificant. Also, the difference between Columns 3 and 4 is significant at the 0.01 level. But the differences between Columns 1 and 2 and between Columns 5 and 6 are not significant at conventional levels.

predicted direction and are significant at the 10% level for the partitions based on bankruptcy risk and auditor bargaining power.

5.3 Controlling for Short Interest

As discussed, Cassell et al. (2011) argue that as short sellers are sophisticated investors, auditors can take the short interest as a signal of audit risk. While Cassell et al. (2011) argue that short sellers are *a source of information about risk* to auditors, our study examines whether the short sellers are *a source of risk per se* to auditors. To show that the impact of short-selling threats is distinct from that of short interest, in Panel C of Table 6 we explicitly control for the variable used in Cassell et al. (2011): the rank of short interest (*ShortInterestRank*).[18] In Columns 1-7 we find that the results are very similar to those in Table 5 and no inferences are affected. Also, we observe that the coefficient on *ShortInterestRank* is not significant in any test, suggesting that in our sample, short interest is not a signal of audit fees. This difference between our results and those reported in Cassell et al. (2011) is likely because we are using different samples. In Column 8 we use *all* observations with all control variables available and indeed find that the coefficient of *ShortInterestRank* (Larger Sample) is significant.[19]

5.4 An Alternative Explanation: SEC Increased Scrutiny of Pilot Firms

A conceivable alternative explanation of our main results is the possibility that the SEC increased scrutiny of pilot firms, and consequently increased the litigation risk of their auditors. We argue that this possibility is unlikely for at least three reasons. First, note that the pilot

[18] Following Cassell et al. (2011), we use the short-interest ratio (shares in the short position scaled by total shares outstanding) in the month prior to the fiscal year starts. *ShortInterestRank* is constructed by ranking firms into deciles each year based on their short-interest ratio and scaling the decile assignment to range between zero and one.
[19] The number of observations for this test is larger as we do not restrict the sample to Russell 3000 firms.

program was a controlled experiment conducted by the SEC to test the effectiveness of short-sale price tests. It would be extremely unwise for the SEC to change its monitoring behavior conditional on pilot/non-pilot status, as such action would severely confound their experimental interpretations. Second, it is not clear how this possibility can explain our three sets of cross-sectional results: the increase of audit fees only exists for firms with higher bankruptcy risk, with non-disciplined managers, or with powerful auditors.

Third, we analyze the number of comment letters issued by the SEC to listed companies and the number of comment topics covered in each comment letter (coded by Audit Analytics).[20] As comment letters data in Audit Analytics are only available from 2005 onwards, we cannot construct difference-in-differences tests in the same way we do in previous sections. However, the comparison between pilot and non-pilot firms for the *During* and *Post* periods suggests that the SEC did *not* increase scrutiny of the pilot firms. In fact, on average pilot firms receive slightly fewer comment letters during the pilot program (annual mean of 1.270 for pilot firms vs. 1.289 for non-pilot firms) but slightly more in the post-pilot period (annual mean of 1.230 for pilot firms vs. 1.226 for non-pilot firms). More importantly, the mean number of comment letters a company receives is not significantly different between pilot and non-pilot firms in any year. The number of topics covered in the SEC comment letters also exhibits the same pattern. In conclusion, our evidence suggests that the SEC did not increase the scrutiny of pilot firms.

5.5 Other Sensitivity Analyses (Untabulated)

Finally, we conduct six additional robustness checks: (1) using a constant sample of firms throughout the sample period; (2) shortening the sample period to 2001 – 2010; (3) including year fixed effects and excluding *During* and *Post* also for the partition analyses; (4) interacting

[20] Prior research uses these two variables to proxy for SEC scrutiny (e.g., Cassell, Dreher, and Myers 2013).

also all control variables with *During*; (5) using control variables in Equation (1) to predict "normal audit fees" and using the "abnormal audit fees" as an alternative dependent variable (e.g., Simunic 1984); and (6) collapsing our firm-year dataset to a firm-period (i.e., Pre/During/Post) dataset to eliminate any time-series correlation in audit fees that could inflate the t-statistics (Bertrand, Duflo, and Mullainathan 2004). No inferences are affected in these analyses. In sum, our conclusions are not sensitive to a variety of research-design choices.

6. Supplemental Analyses

6.1 Other Audit Behaviors: Auditor Switches and Tax Fees

We go one step further and explore auditor switches and tax-service fees. One strategy auditors use to counter litigation risk is to "avoid risk through client retention and acceptance" (DeFond and Zhang 2014). When the litigation risk increases in the presence of short-selling threats, the auditors may find it more sensible to leave the clients with larger threats rather than ask for higher fees. However, note that auditor switches do not happen frequently and are likely much more costly for the client (and the auditor) than raising audit fees. As a result, the power of auditor-switch tests is likely lower than that for audit fees.

Following prior literature, we use the following model:

$$\begin{aligned} Switch_{i,t} = {} & \beta_0 + \beta_1 Pilot_{i,t} + \beta_2 During_{i,t} + \beta_3 Pilot_{i,t} \times During_{i,t} + \beta_4 Post_{i,t} + \beta_5 Pilot_{i,t} \times Post_{i,t} \\ & + \beta_6 Size_{i,t} + \beta_7 Lev_{i,t} + \beta_8 BTM_{i,t} + \beta_9 ROA_{i,t} + \beta_{10} Loss_{i,t} + \beta_{11} AbsAccr_{i,t} \\ & + \beta_{12} CA/TA_{i,t} + \beta_{13} Quick_{i,t} + \beta_{14} INVREC_{i,t} + \beta_{15} SizeGr_{i,t} + \beta_{16} LIT_{i,t} \\ & + \beta_{17} FYEnd_{i,t} + \beta_{18} NBusSeg_{i,t} + \beta_{19} GCO_{i,t} + \beta_{20} BIG4_{i,t} + \beta_{21} MNC_{i,t} \\ & + \beta_{21} LnAF_{i,t} + \beta_{21} Andersen_{i,t} + IND_i + Year_t + \varepsilon_{i,t} \end{aligned} \tag{4}$$

We use a linear probability model to test Equation (4) as we focus on the interaction term (Ai and Norton 2003) and because we include year and industry fixed effects (i.e., the "incidental

parameter problem" – Lancaster 2000). Panel A of Table 7 tabulates the results. Column 1 presents the full sample regression results. Columns 2-3, Columns 4-5, and Columns 6-7 present the cross-sectional tests based on bankruptcy risk, disciplined managers, and auditors' industry leadership, respectively.

The results suggest that only the cross-sectional hypothesis on bankruptcy risk is supported in the auditor-switch setting. Specifically, the coefficient of *Pilot×During* is positive and significant at the 0.10 level in Column 2 but negative and insignificant in Column 3. Equally important, the difference between Columns 2 and 3 is significant at the 0.05 level.

Finally, we examine the impact of short-selling threats on tax-service fees (*LnTaxF*) in Panel B.[21] As tax fees are arguably irrelevant to litigation risk caused by short-selling, we do *not* expect to observe a significant estimated coefficient on *Pilot×During*. In other words, we can interpret this analysis as a *placebo* test. Indeed, Panel B of Table 7 shows that none of the coefficients on *Pilot×During* is significant.

6. 2 Combining Litigation Risk and Bargaining Power

The previous cross-sectional tests illustrate two "necessary conditions" for the impact of short-selling threats on audit fees: (1) auditors perceive the threats caused by the down-side risk to be real, and (2) auditors have the ability to persuade the clients to pay higher fees. The main analyses consider these two necessary conditions separately, whereas we in this section combine them.

We predict that the impact becomes stronger when both conditions are met and becomes weaker when neither condition is met. Table 8 presents the results. As predicted, we find that the

[21] Tax-service fees are a subset of overall non-audit feels. The inferences remain the same if we focus on all non-audit fees.

coefficient of *Pilot×During* becomes larger when the subsample meets multiple requirements all of which predict stronger impact. For example, Column 3 shows that the pilot firms with higher bankruptcy risk *and* auditors with higher bargaining power have 30% (i.e., EXP (0.2636) -1) larger increase in audit fees compared with their non-pilot counterparts, indicating that the impact of short-selling threats on audit fees is substantial for those firms.

6.3 Separating Risk Premium and Increased Effort: Exploratory Analyses (Untabulated)

DeFond and Zhang (2014, 298) comment that most studies on litigation risk "do not address whether high fees are due to increased effort or risk premium." One reason is that it is difficult to observe effort or premium. Even their close proxies such as audit hours (for effort) and billing rates (for premium) are not easily available for large samples of firms. DeFond and Zhang (2014) also argue that the distinction between these two is critical, because additional effort can improve audit quality, while increased premium simply shifts the expected litigation loss to the client.

To understand the channels through which short-selling threats affect audit fees, we follow Knechel and Payne (2001) and infer audit effort using audit-report lag (*LnARL*), measured by the log of the number of days between the fiscal-year end and the audit-report date. We first use *LnARL* as the dependent variable in Equation (1) and find some weak evidence that auditors spend more effort in the presence of short-selling threats.[22] Next, we include *LnARL* as an additional control variable in Equation (1) and find that the coefficients of *Pilot×During* are only slightly smaller than their counterparts in the main analyses (and their statistical significance levels remain). For example, the coefficient of *Pilot×During* in the full sample reduces from 0.0405 in Panel A of Table 5 to 0.0389 when we control for *LnARL*. These two sets of results

[22] Specifically, the coefficients of *Pilot×During* are all positive for the subsamples with high bankruptcy risk, with undisciplined managers, or with powerful auditors, but these coefficients are not significant at conventional levels.

suggest that pilot firms' auditors appear to invest more effort in the presence of short-selling threats, but such additional effort only explains a small proportion of the increase in audit fees (i.e., 1- 0.0389/0.0405=4%). However, we acknowledge that this interpretation is only as good as the extent to which we can infer audit effort from audit-report lags.

7. Conclusion

There is significant interest in short sellers by both practitioners and researchers. While several studies on short sellers examine the impact of short selling on the targeted firms, this paper extends this stream of literature by showing that short selling also has a real impact on other capital-market participants. More specially, using the SEC SHO pilot project as our identification strategy and implementing difference-in-differences tests, we find that short selling increases audit fees on average. In cross-sectional analyses, we further show that the impact of short selling on audit fees only exists when auditors care more about the down-side risk caused by short sellers (i.e., for client firms with higher bankruptcy risk and with managers who are not disciplined by short selling) and when auditors have greater bargaining power.

We believe this study can enhance our understanding of short sellers' role in capital markets. Although many studies have documented the "intended" consequences of short-selling activities, short sellers may also have "unintended" or spillover consequences on other market participants. Increased audit fees can be recognized (arguably) as a cost to shareholders. In this sense, we also highlight a specific type of cost of short selling. We believe this evidence is potentially useful for regulators when they make cost-benefit analyses for short-selling related rule-makings.

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Appendix: Variable Definitions

Variables	Definitions
LnAF	Log of audit fees. AF refers to the raw number of audit fees
During	One for year 2005 to 2007 and zero otherwise
Post	One for year 2008 to 2012 and zero otherwise
Pilot	One for pilot firms and zero otherwise
Control Variables	
SIZE	Log of total assets (by million)
LEV	Ratio of total liability to total assets
BTM	Ratio of book value to closing market value at fiscal year ends
ROA	Ratio of net income to total assets
Loss	One if net income is negative and zero otherwise
AbsAcc	Absolute total accruals
CA/TA	Ratio of current assets to total assets
Quick	Ratio of current assets (excluding inventory) to current liability
InvRec	Ratio of inventory and accounts receivable to total assets
SizeGr	The growth of total assets
Litigation	One for firms in litigation-related industries and zero otherwise (not used in Firm FE models). Litigation-related industries include those industries with SIC 4-digit ranging from 2833 to 2836, 3570 to 3577, 3600 to 3674, 5200 to 5961, and 7370.
FYEnd	One for firms with fiscal year ends not in December and zero otherwise
NBusSeg	Log of one plus the number of business segments
GCO	One for going concern opinion and zero otherwise
Switch	One for auditor switch and zero otherwise
BIG4	One for big 4 auditor and zero otherwise
MNC	One for firms with foreign operations and zero otherwise
Andersen	One for firms with Arthur Andersen as their auditor and zero otherwise
Cross-Sectional Variables	
Bankruptcy Risk	Observations with Lower Altman Z-score than sample median are defined as High Bankruptcy Risk subsample; Observations with High Altman Z-score than sample median are defined as Low Bankruptcy Risk subsample
DA	Observations with positive Discretionary Accruals (based on McNichols 2002) are defined as DA > 0 subsample; Observations with negative Discretionary Accruals are defined as DA < 0 subsample
Auditor Power	Observations with auditors whose market share (in terms of audit fees) is the largest in a given industry at the city level are defined as High Auditor Power subsample; Observations with auditors whose market share is not the largest in a given industry at the city level are defined as Low Auditor Power subsample;
Variables in the Robustness Section	
Bankruptcy Risk (Ohlson)	Observations with higher Ohlson bankruptcy score than sample median are defined as High Bankruptcy Risk subsample; Observations with lower Ohlson bankruptcy score than sample median are defined as Low

	Bankruptcy Risk subsample
MJMDA	Observations with positive Discretionary Accruals (based on Modified Jones Model (Dechow et al. 1995)) are defined as MJMDA > 0 subsample; Observations with negative Discretionary Accruals are defined as MJMDA < 0 subsample
Client Power	Client bargaining power is calculated as the proportion of the client's annual sales in all clients' aggregate annual sales with the same auditor. Those observations with client bargaining power higher than sample median are defined as High Client Power subsample and those observations with client bargaining power lower than sample median are defined as Low Client Power subsample
ShortInterestRank	The rank of the ratio of shares in short position to the total shares outstanding in the month prior to the fiscal year starts (the rank is calculated using pilot and non-pilot observations and rescaled to range between zero and one)
ShortInterestRank (Larger Sample)	The rank of the ratio of shares in short position to the total shares outstanding in the month prior to the fiscal year starts (the rank is calculated using universe of firms with all variables available and rescaled to range between zero and one)
Variables in Supplemental Analyses	
LnTaxF	Log of tax fees
LnARL	Log of one plus the number of days between fiscal year ends to the audit report date

Table 1: Sample Selection

Sample Selection Steps	Pilot Firms	Non-Pilot Firms	Total
Russell 3000 firms in 2004	986	2,012	2,998
Also in Russell 2005	859	1,733	2,592
List at NYSE, NASDAQ, and AMEX	726	1,455	2,181
Excluding financial and utilities firms	528	1,069	1,597
Final sample with all variables available	494	991	1,485

Table 2: Summary Statistics

Panel A: Full Sample

Variables	N	Mean	S.T.D	p25	p50	p75
AF ($)	13,323	2.123M	3.707M	0.467M	1.016M	2.146M
LnAF	13,323	13.830	1.178	13.050	13.830	14.580
Pilot	13,323	0.343	0.475	0.000	0.000	1.000
During	13,323	0.272	0.445	0.000	0.000	1.000
Post	13,323	0.373	0.484	0.000	0.000	1.000
SIZE	13,323	6.941	1.560	5.854	6.796	7.884
LEV	13,323	0.483	0.264	0.295	0.471	0.625
BTM	13,323	0.481	0.693	0.254	0.424	0.655
ROA	13,323	0.010	0.211	0.001	0.047	0.087
Loss	13,323	0.248	0.432	0.000	0.000	0.000
AbsAcc	13,323	-0.076	0.166	-0.097	-0.055	-0.022
CA/TA	13,323	0.490	0.227	0.324	0.487	0.661
Quick	13,323	2.415	2.812	1.036	1.567	2.662
InvRec	13,323	0.245	0.162	0.115	0.228	0.344
SizeGr	13,323	0.150	0.600	-0.027	0.056	0.167
Litigation	13,323	0.308	0.462	0.000	0.000	1.000
FYEnd	13,323	0.334	0.472	0.000	0.000	1.000
NBusSeg	13,323	1.114	0.447	0.693	1.099	1.386
GCO	13,323	0.007	0.082	0.000	0.000	0.000
Switch	13,323	0.056	0.230	0.000	0.000	0.000
BIG4	13,323	0.902	0.297	1.000	1.000	1.000
MNC	13,323	0.645	0.479	0.000	1.000	1.000
Andersen	13,323	0.034	0.182	0.000	0.000	0.000

This table summarizes the descriptive statistics for variables in the main tests. All variables are defined in the Appendix.

Panel B: Comparison between Pilot vs. Non-Pilot firms before the Pilot Program (i.e., 2003)

	Non-Pilot Firms		Pilot Firms		
Variables	N	Mean	N	Mean	Difference
AF ($)	894	1.247M	449	1.041M	0.206M
LnAF	894	13.33	449	13.22	0.104*
SIZE	894	6.673	449	6.613	0.060
LEV	894	0.466	449	0.443	0.023
BTM	894	0.395	449	0.409	-0.015
ROA	894	0.018	449	0.024	-0.007
Loss	894	0.256	449	0.232	0.025
AbsAcc	894	-0.065	449	-0.060	-0.006
CA/TA	894	0.497	449	0.509	-0.012
Quick	894	2.612	449	2.854	-0.242
InvRec	894	0.234	449	0.243	-0.009
SizeGr	894	0.187	449	0.215	-0.028
Litigation	894	0.295	449	0.327	-0.032
FYEnd	894	0.337	449	0.370	-0.033
NBusSeg	894	1.086	449	1.078	0.008
GCO	894	0.003	449	0.000	0.003
Switch	894	0.029	449	0.040	-0.011
BIG4	894	0.969	449	0.978	-0.009
MNC	894	0.617	449	0.630	-0.013
Andersen	894	0.001	449	0.000	0.001

This table compare the means of the variables between pilot firms and non-pilot firms before the pilot program was announced (i.e., year 2003). All variables are defined in the Appendix.

Table 3: Pearson Correlations among all Regression Variables

1. LnAF	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
2. Pilot	***-0.04***																				
3. During	***0.22***	0.00																			
4. Post	***0.31***	0.01	***-0.48***																		
5. SIZE	***0.76***	***-0.04***	***0.02***	***0.15***																	
6. LEV	***0.31***	***-0.06***	***-0.03***	***0.07***	***0.34***																
7. BTM	-0.01	***0.02***	***-0.11***	***0.12***	-0.01	***-0.20***															
8. ROA	***0.14***	0.00	***0.08***	0.02	***0.23***	***-0.11***	***-0.09***														
9. Loss	***-0.15***	0.00	***-0.10***	0.00	***-0.23***	***0.08***	***0.12***	***-0.62***													
10. AbsAcc	***-0.03***	-0.01	***-0.07***	***0.10***	***-0.05***	***0.18***	***0.05***	***-0.12***	***0.11***												
11. CA/TA	***-0.20***	***0.02***	0.01	***-0.02***	***-0.43***	***-0.31***	***-0.07***	***-0.11***	***0.12***	***-0.17***											
12. Quick	***-0.31***	***0.03***	-0.01	***-0.06***	***-0.32***	***-0.45***	***-0.04***	***-0.13***	***0.19***	***-0.15***	***0.43***										
13. InvRec	***0.08***	0.02	0.00	0.00	***-0.07***	***0.04***	***0.11***	***0.16***	***-0.17***	***-0.05***	***0.41***	***-0.30***									
14. SizeGr	***-0.09***	0.00	***0.04***	***-0.13***	***0.02***	***-0.10***	***-0.12***	***0.18***	***-0.11***	***-0.21***	0.01	***0.13***	***-0.09***								
15. Litigation	***-0.10***	***0.02***	0.01	0.01	***-0.07***	***-0.15***	-0.02	***-0.15***	***0.15***	-0.01	***0.26***	***0.24***	***-0.09***	***0.03***							
16. FYEnd	-0.02	0.02	0.02	***0.02***	-0.01	***-0.12***	***0.05***	***0.07***	***-0.07***	***-0.02***	***0.14***	***-0.02***	***0.17***	***-0.04***	***0.12***						
17. NBusSeg	***0.38***	0.00	-0.01	***0.05***	***0.33***	***0.16***	***0.03***	***0.11***	***-0.11***	***0.03***	***-0.19***	***-0.21***	***0.13***	***-0.07***	***-0.24***	***0.00***					
18. GCO	***-0.02***	***-0.02***	***-0.02***	0.01	***-0.05***	***0.12***	-0.01	***-0.17***	***0.11***	***0.05***	-0.01	***-0.02***	***-0.03***	***-0.05***	0.00	***-0.02***	-0.02				
19. Switch	***-0.09***	0.00	0.01	***-0.11***	***-0.06***	0.00	***0.03***	***-0.03***	0.02	0.01	-0.01	-0.01	0.01	0.00	***-0.03***	0.00	0.00	0.01			
20. BIG4	***0.20***	0.01	***0.05***	***0.02***	***0.20***	***0.08***	***-0.06***	-0.02	-0.01	***-0.03***	***-0.06***	***-0.02***	***-0.07***	0.00	***0.03***	0.01	***0.05***	0.00	***-0.06***		
21. MNC	***0.35***	0.01	***-0.02***	***0.06***	***0.19***	***-0.05***	***-0.03***	***0.09***	***-0.08***	***-0.09***	***0.10***	***-0.05***	***0.16***	***-0.03***	***-0.10***	0.01	***0.20***	***-0.02***	***-0.02***	***0.04***	
22. Andersen	***-0.19***	0.00	***-0.12***	***-0.14***	***-0.06***	0.01	***0.02***	-0.01	0.01	0.00	***-0.02***	0.00	0.02	***0.04***	***-0.04***	***-0.03***	***-0.02***	0.00	***0.04***	***-0.57***	***-0.05***

This table presents the Pearson correlations between each two variables used in the main tests. All variables are defined in the Appendix. The correlation coefficients in **bold** and *italic* are significant at the 0.05 level.

Table 4: By-Year Comparison of Audit Fees (*LnAF*) between Pilot and Non-Pilot Firms

Year	Total	Non-Pilot firms		Pilot Firms		Difference	t-stat	p-value
	N	N	LnAF	N	LnAF			
2000	815	544	12.780	271	12.650	0.133	1.580	0.115
2001	1,261	821	12.870	440	12.760	0.107	1.741	0.082
2002	1,307	870	13.070	437	12.940	0.133	2.091	0.037
2003	1,343	894	13.330	449	13.220	0.104	1.706	0.088
2004	1,372	914	13.920	458	13.800	0.113	1.850	0.065
2005	1,308	870	14.210	438	14.150	0.053	0.939	0.348
2006	1,204	795	14.300	409	14.210	0.085	1.474	0.141
2007	1,114	728	14.350	386	14.270	0.086	1.452	0.147
2008	1,074	698	14.390	376	14.260	0.135	2.255	0.024
2009	1,045	681	14.340	364	14.210	0.129	2.136	0.033
2010	997	646	14.310	351	14.190	0.129	2.079	0.038
2011	948	618	14.360	330	14.210	0.150	2.307	0.021
2012	907	590	14.420	317	14.270	0.151	2.233	0.026
Overall	14,695	9,669	13.896	5,026	13.780	0.116	1.834	0.090

This table provides by-year comparison of *LnAF* (log of audit fees) between pilot firms and non-pilot firms. Note that for completeness, we also include year 2004 in this table, a year excluded in the regression analyses.

Table 5: The Impact of Short-Selling Threats on Audit fees

Panel A: Full Sample

DV=LnAF	(1)	(2)
Pilot	-0.0641***	-0.0626***
	(-2.83)	(-2.66)
During	0.9942***	
	(10.23)	
Pilot×During	0.0405***	0.0391**
	(2.64)	(2.35)
Post	0.8956***	
	(8.93)	
Pilot×Post	0.0271	0.0253
	(1.39)	(1.22)
SIZE	0.4902***	0.4900***
	(43.27)	(44.80)
LEV	0.2719***	0.2724***
	(5.58)	(5.51)
BTM	0.0146	0.0173*
	(1.19)	(1.81)
ROA	-0.3156***	-0.3437***
	(-5.26)	(-5.80)
Loss	0.0942***	0.0926***
	(6.53)	(6.92)
AbsAcc	0.2266**	0.2285***
	(2.42)	(3.09)
CA/TA	0.5060***	0.4713***
	(6.13)	(5.95)
Quick	-0.0317***	-0.0301***
	(-6.56)	(-6.67)
InvRec	0.2371**	0.3012***
	(2.19)	(2.82)
SizeGr	-0.0474***	-0.0269**
	(-3.90)	(-2.03)
Litigation	-0.0575*	-0.0557
	(-1.65)	(-1.59)
FYEnd	-0.0948**	-0.1107***
	(-2.47)	(-2.80)
NBusSeg	0.2982***	0.2979***
	(12.30)	(12.28)
GCO	0.1365	0.1605**
	(1.48)	(2.05)
Switch	-0.0600	-0.0698
	(-1.14)	(-1.49)
BIG4	0.1107**	0.1067**
	(2.36)	(2.25)
MNC	0.3367***	0.3378***
	(12.73)	(12.82)
Andersen	-0.1320	0.0442
	(-1.29)	(0.86)

Year FE	NO	YES
SIC2 FE	YES	YES
Constant	9.4504***	9.2092***
	(88.70)	(114.31)
Observations	13,323	13,323
Adjusted R^2	0.821	0.828

This Panel presents the results of testing H1 (i.e., full sample analyses). Column 1 is based on Equation (1) while Column 2 includes year fixed effects but excludes *During* and *Post* to avoid multicollinearity.
All variables are defined in the Appendix. *t* statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests)

Panel B: Subsample Analyses

DV =LnAF	(1) High Bankruptcy Risk	(2) Low Bankruptcy Risk	(3) DA>0	(4) DA<0	(5) High Auditor Power	(6) Low Auditor Power
Pilot	-0.0952***	-0.0191	-0.0804***	-0.0487**	-0.0696*	0.0059
	(-2.91)	(-0.67)	(-2.67)	(-2.10)	(-1.93)	(0.14)
During	0.9248***	1.0678***	0.9722***	1.0053***	0.9456***	1.0434***
	(8.72)	(11.64)	(9.71)	(10.21)	(10.39)	(11.20)
Pilot×During	0.0912***	-0.0040	0.0990***	-0.0068	0.0760**	-0.0682*
	(3.55)	(-0.18)	(3.36)	(-0.39)	(2.22)	(-1.70)
	(Diff: p-value=0.006)		(Diff: p-value=0.002)		(Diff: p-value=0.006)	
Post	0.8448***	0.9379***	0.9056***	0.8995***	0.8457***	0.9736***
	(7.79)	(9.91)	(8.75)	(8.98)	(8.97)	(9.93)
Pilot×Post	0.0452	0.0176	0.0363	0.0128	0.0161	-0.0319
	(1.41)	(0.65)	(1.16)	(0.55)	(0.39)	(-0.67)
SIZE	0.5063***	0.4717***	0.4910***	0.4930***	0.4980***	0.4214***
	(27.52)	(38.37)	(31.26)	(42.15)	(37.78)	(21.00)
LEV	0.1814***	0.3000***	0.3225***	0.2090***	0.2164***	0.3207***
	(3.05)	(3.94)	(4.57)	(4.14)	(3.25)	(4.38)
BTM	-0.0056	0.0879*	0.0153	0.0097	0.0019	0.0145
	(-0.48)	(1.93)	(1.14)	(0.66)	(0.07)	(0.63)
ROA	-0.2368***	-0.4510***	-0.2987***	-0.3740***	-0.4475***	-0.1742
	(-3.19)	(-4.34)	(-3.14)	(-3.62)	(-4.73)	(-1.60)
Loss	0.1036***	-0.0077	0.1109***	0.0761***	0.0479***	0.0612
	(4.66)	(-0.22)	(4.24)	(3.98)	(2.74)	(1.59)
AbsAcc	0.2000*	0.2269*	0.1186	0.2748**	0.4196***	0.1033
	(1.95)	(1.79)	(0.64)	(2.16)	(3.33)	(0.90)
CA/TA	0.7253***	0.4523***	0.5036***	0.4757***	0.4058***	0.3831***
	(5.39)	(5.00)	(4.32)	(6.10)	(4.21)	(3.14)
Quick	-0.0427***	-0.0317***	-0.0268***	-0.0398***	-0.0169***	-0.0345***
	(-6.16)	(-5.98)	(-4.52)	(-6.80)	(-3.25)	(-6.16)
InvRec	0.3823**	0.0166	0.2893**	0.2525**	0.4461***	0.4456***
	(2.23)	(0.13)	(2.10)	(1.99)	(3.20)	(2.97)
SizeGr	-0.0771***	-0.0355	-0.0368**	-0.0527***	-0.0581***	-0.0705***
	(-5.60)	(-1.53)	(-2.42)	(-3.27)	(-4.78)	(-2.63)
Litigation	-0.0013	-0.1199***	-0.0824**	-0.0278	0.0370	-0.0628
	(-0.03)	(-2.70)	(-1.96)	(-0.70)	(0.74)	(-1.19)
FYEnd	-0.1030**	-0.0717*	-0.0967**	-0.1083**	-0.1380***	-0.0632
	(-2.22)	(-1.90)	(-2.41)	(-2.45)	(-2.98)	(-1.31)
NBusSeg	0.3164***	0.2373***	0.2746***	0.3054***	0.3382***	0.2739***
	(9.31)	(7.44)	(7.61)	(9.73)	(10.00)	(6.08)
GCO	0.1323	1.0279***	0.1191	0.1581	-0.0761	0.1377**
	(1.46)	(6.89)	(1.10)	(1.17)	(-0.48)	(2.46)
Switch	-0.0795*	-0.0567	-0.0450	-0.0750	-0.1054**	-0.0782
	(-1.89)	(-0.69)	(-0.81)	(-1.52)	(-2.11)	(-1.39)
BIG4	0.1034*	0.1134**	0.1187**	0.1336***	-0.0936	0.1471**
	(1.77)	(1.99)	(2.02)	(2.83)	(-1.06)	(2.34)
MNC	0.3375***	0.3003***	0.3763***	0.3010***	0.2960***	0.3271***

	(9.51)	(9.22)	(12.47)	(10.18)	(7.95)	(8.07)
Andersen	-0.1710	-0.0912	-0.0800	-0.1412	-0.4219***	-0.0500
	(-1.45)	(-0.91)	(-0.67)	(-1.40)	(-3.54)	(-0.47)
SIC2 FE	YES	YES	YES	YES	YES	YES
Constant	8.9585***	9.5047***	8.7821***	9.4586***	8.6351***	9.0924***
	(45.84)	(60.34)	(71.58)	(64.04)	(63.98)	(49.33)
Observations	6,658	6,657	5,815	6,282	4,402	3,842
Adjusted R^2	0.816	0.823	0.822	0.824	0.838	0.795

This Panel presents the subsample analyses of testing H2.1 (Columns 1-2), H2.2 (Columns 3-4), and H2.3 (Columns 5-6). Column 1(2) includes firms with high (low) bankruptcy risk based on Altman Z-score (1968). Column 3(4) includes firms with positive (negative) discretionary accruals based on McNichols (2002). Column 5(6) includes firms with auditors whose market share in the SIC 2-digit industry is (not) the largest in the city.

The "Diff: p-value" compares the difference between two coefficients of *Pilot*×*During* in the same partition using Chi-square tests.

All variables are defined in the Appendix. *t* statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests)

Table 6: Robustness Checks

Panel A: Subsample Analyses based on Alternative Partition Approaches

DV =LnAF	(1) High Bankruptcy Risk (Ohlson)	(2) Low Bankruptcy Risk (Ohlson)	(3) Modified Jones Model DA>0	(4) Modified Jones Model DA<0	(5) Low Client Power	(6) High Client Power
Pilot	-0.0810***	-0.0391	-0.0694**	-0.0453	-0.0522*	-0.0667**
	(-2.72)	(-1.45)	(-2.37)	(-1.57)	(-1.83)	(-2.09)
During	0.9563***	1.0421***	0.9692***	1.0279***	1.0407***	0.9745***
	(9.77)	(10.41)	(9.40)	(10.85)	(9.94)	(9.72)
Pilot×During	0.0562**	0.0114	0.0836***	-0.0572	0.0419*	0.0332
	(2.33)	(0.60)	(2.89)	(-1.60)	(1.85)	(1.32)
	(Diff: p-value=0.144)		(Diff: p-value=0.002)		(Diff: p-value=0.798)	
Post	0.8753***	0.9199***	0.9012***	0.9010***	0.9591***	0.8620***
	(8.56)	(9.07)	(8.61)	(9.26)	(8.91)	(8.27)
Pilot×Post	0.0256	0.0086	0.0272	0.0099	0.0151	0.0352
	(0.81)	(0.35)	(1.01)	(0.30)	(0.55)	(1.12)
Controls	YES	YES	YES	YES	YES	YES
SIC2 FE	YES	YES	YES	YES	YES	YES
Constant	9.4585***	9.0178***	8.6640***	8.9072***	8.7583***	9.4178***
	(47.36)	(55.08)	(60.81)	(70.90)	(52.36)	(51.15)
Observations	6,659	6,659	7,816	4,281	6,661	6,660
Adjusted R^2	0.811	0.839	0.826	0.813	0.765	0.838

This Panel presents t a set of robustness check for the subsample analyses of testing H2.1 (Columns 1-2), H2.2 (Columns 3-4), and H2.3 (Columns 5-6). Column 1(2) includes firms with high (low) bankruptcy risk based on Ohlson bankruptcy score (1980). Column 3(4) includes firms with positive (negative) discretionary accruals based on Modified Jones Model (Dechow et al. 1995). Column 5(6) includes firms whose ratio of sales to the aggregate sales of all clients by the same auditor is smaller (larger) than sample median.

The "Diff: p-value" compares the difference between two coefficients of *Pilot×During* in the same partition using Chi-square tests.

All variables are defined in the Appendix. *t* statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests)

Panel B: Firm Fixed-Effects Estimation[23]

DV =LnAF	(1) Full Sample	(2) High Bankruptcy Risk	(3) Low Bankruptcy Risk	(4) DA>0	(5) DA<0	(6) High Auditor Power	(7) Low Auditor Power
During	1.0403***	0.8962***	1.1156***	1.0056***	1.0471***	0.9916***	1.0646***
	(10.57)	(10.20)	(11.90)	(10.09)	(12.12)	(9.11)	(12.26)
Pilot×During	0.0397*	0.0942**	0.0045	0.0717*	0.0149	0.0616	-0.0552
	(1.67)	(2.48)	(0.14)	(1.89)	(0.61)	(1.33)	(-1.33)
		(Diff: p-value=0.074)		(Diff: p-value=0.209)		(Diff: p-value=0.061)	
Post	0.9922***	0.8699***	1.0673***	0.9647***	1.0122***	0.9290***	1.0577***
	(9.78)	(9.54)	(10.85)	(9.61)	(11.97)	(8.10)	(12.39)
Pilot×Post	0.0085	0.0519	-0.0170	0.0333	-0.0084	0.0479	-0.1049**
	(0.35)	(1.34)	(-0.56)	(0.86)	(-0.29)	(0.93)	(-2.42)
Controls	YES	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES	YES
Observations	13,301	5,107	6,513	5,637	6,113	4,260	3,729
Adjusted R^2	0.926	0.925	0.933	0.929	0.928	0.935	0.932

This Panel presents the firm fixed effects estimation of testing H1 (Column 1), H2.1 (Columns 2-3), H2.2 (Columns 4-5), and H2.3 (Columns 6-7). Column 1 is the full sample analysis. Column 2(3) includes firms with high (low) bankruptcy risk based on Altman Z-score (1968). Column 4(5) includes firms with positive (negative) discretionary accruals based on McNichols (2002). Column 6(7) includes firms with auditors whose market share in the SIC 2-digit industry is (not) the largest in the city.

The "Diff: p-value" compares the difference between two coefficients of *Pilot×During* in the same partition using Chi-square tests.

All variables are defined in the Appendix. *t* statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests)

[23] We use the Stata command "xtivreg2" to implement the firm fixed-effects estimation with two-way clustering. "xtivreg2" reports no estimated constant. We also use the Stata command "xtreg" with one-way clustering (i.e., by firm). All coefficients are exactly the same and the t-values are only marginally smaller.

Panel C: Controlling for the Signaling Effect of Short Interest

DV=LnAF	(1) Full Sample	(2) High Bankruptcy Risk	(3) Low Bankruptcy Risk	(4) DA>0	(5) DA<0	(6) High Auditor Power	(7) Low Auditor Power	(8) Including non-Russell 3000 index firms
ShortInterestRank	-0.0086	0.0159	-0.0454	0.0118	-0.0130	-0.0299	0.0120	
	(-0.19)	(0.35)	(-1.03)	(0.25)	(-0.27)	(-0.50)	(0.27)	
Pilot	-0.0641***	-0.0951***	-0.0180	-0.0806***	-0.0487**	-0.0696*	0.0058	
	(-2.83)	(-2.91)	(-0.63)	(-2.68)	(-2.10)	(-1.92)	(0.13)	
During	0.9966***	0.9218***	1.0844***	0.9692***	1.0092***	0.9537***	1.0395***	
	(10.21)	(8.65)	(11.91)	(9.64)	(10.16)	(10.35)	(11.28)	
Pilot×During	0.0406***	0.0907***	-0.0048	0.0989***	-0.0065	0.0771**	-0.0676*	
	(2.63)	(3.52)	(-0.21)	(3.35)	(-0.38)	(2.23)	(-1.70)	
		(Diff: p-value=0.152)		(Diff: p-value=0.014)		(Diff: p-value=0.008)		
Post	0.8980***	0.8420***	0.9563***	0.9027***	0.9034***	0.8530***	0.9697***	
	(8.95)	(7.75)	(10.23)	(8.73)	(8.97)	(8.92)	(10.09)	
Pilot×Post	0.0271	0.0445	0.0146	0.0362	0.0128	0.0170	-0.0316	
	(1.39)	(1.38)	(0.54)	(1.16)	(0.55)	(0.41)	(-0.66)	
ShortInterestRank (Larger Sample)								0.5998***
								(6.74)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
SIC2 FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	9.4508***	8.9524***	9.5236***	8.7807***	9.4644***	8.6326***	9.0785***	9.4304***
	(88.48)	(49.95)	(59.76)	(66.95)	(62.58)	(64.03)	(47.33)	(379.40)
Observations	13,323	6,658	6,657	5,815	6,282	4,402	3,842	58,136
Adjusted R^2	0.821	0.816	0.823	0.822	0.824	0.838	0.795	0.811

This Panel presents the results of controlling short interest to test H1 (Column 1), H2.1 (Columns 2-3), H2.2 (Columns 4-5), and H2.3 (Columns 6-7). Column 1 is the full sample analysis. Column 2(3) includes firms with high (low) bankruptcy risk based on Altman Z-score (1968). Column 4(5) includes firms with positive (negative) discretionary accruals based on McNichols (2002). Column 6(7) includes firms with auditors whose market share in the SIC 2-digit industry is (not) the largest in the city. Column 8 includes all non-Russell 3000 index firms with available control variables in year 2000 to 2012.
The "Diff: p-value" compares the difference between two coefficients of *Pilot×During* in the same partition using Chi-square tests.
All variables are defined in the Appendix. *t* statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests)

Table 7: The Impact of Short-Selling Threats on Audit Switches and Tax-Service Fees

Panel A: The Impact of Short-Selling Threats on Audit Switches

DV=Switch	(1) Full Sample	(2) High Bankruptcy Risk	(3) Low Bankruptcy Risk	(4) DA>0	(5) DA<0	(6) High Auditor Power	(7) Low Auditor Power
Pilot	0.0055***	-0.0116	0.0186	0.0107*	-0.0012	0.0001	-0.0098**
	(2.95)	(-0.80)	(1.53)	(1.76)	(-0.19)	(0.03)	(-2.40)
During	-0.0134	-0.0262	0.0013	-0.0071	-0.0161	-0.0228	-0.0027
	(-0.37)	(-0.57)	(0.05)	(-0.21)	(-0.42)	(-0.61)	(-0.07)
Pilot×During	-0.0019	0.0277*	-0.0218	-0.0022	0.0046	0.0233	0.0018
	(-0.20)	(1.79)	(-1.24)	(-0.15)	(0.46)	(0.91)	(0.17)
		(Diff: p-value=0.034)		(Diff: p-value=0.698)		(Diff: p-value=0.436)	
Post	-0.0496	-0.0589	-0.0426	-0.0531	-0.0413	-0.0481	-0.0490
	(-1.33)	(-1.22)	(-1.53)	(-1.53)	(-1.03)	(-1.32)	(-1.28)
Pilot×Post	-0.0066	0.0068	-0.0135	-0.0093	-0.0010	0.0023	0.0092
	(-1.54)	(0.43)	(-1.02)	(-1.22)	(-0.08)	(0.22)	(0.69)
Controls	YES	YES	YES	YES	YES	YES	YES
SIC2 FE	YES	YES	YES	YES	YES	YES	YES
Constant	0.2194*	0.3367***	0.5114**	0.2264	0.1966**	0.3128***	0.3176***
	(1.68)	(4.23)	(2.02)	(1.63)	(2.55)	(4.02)	(3.06)
Observations	13,323	6,658	6,657	5,815	6,282	4,402	3,842
Pseudo R^2	0.021	0.023	0.023	0.024	0.018	0.029	0.017

This Panel presents the results examining the impact of short selling threats on auditor switch. Column 1 is the full sample analysis. Column 2(3) includes firms with high (low) bankruptcy risk based on Altman Z-score (1968). Column 4(5) includes firms with positive (negative) discretionary accruals based on McNichols (2002). Column 6(7) includes firms with auditors whose market share in the SIC 2-digit industry is (not) the largest in the city.
The "Diff: p-value" compares the difference between two coefficients of *Pilot×During* in the same partition using Chi-square tests.
All variables are defined in the Appendix. *t* statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests)

Panel B: The Impact of Short-Selling Threats on Tax Fees

DV=LnTaxF	(1) Full Sample	(2) High Bankruptcy Risk	(3) Low Bankruptcy Risk	(4) DA>0	(5) DA<0	(6) High Auditor Power	(7) Low Auditor Power
Pilot	-0.2963	-0.4565	-0.0851	-0.3439**	-0.3071	-0.2204	0.0802
	(-0.99)	(-1.09)	(-0.40)	(-1.96)	(-0.80)	(-0.43)	(0.36)
During	2.5504	2.4336	2.6449	2.6553	2.4868	2.2079	2.6235
	(1.43)	(1.39)	(1.42)	(1.55)	(1.31)	(1.23)	(1.50)
Pilot×During	0.2836	0.2164	0.3195	0.2774	0.3918	0.0237	0.3292
	(0.76)	(0.44)	(0.92)	(0.93)	(0.80)	(0.04)	(0.86)
		(Diff: p-value=0.864)		(Diff: p-value=0.842)		(Diff: p-value=0.662)	
Post	2.1531	2.2595	2.0048	2.0900	2.2003	1.7556	2.3556
	(1.22)	(1.31)	(1.10)	(1.25)	(1.19)	(0.98)	(1.37)
Pilot×Post	0.8372**	0.8940*	0.9133**	0.8746***	0.8148*	0.9078	0.6549
	(2.32)	(1.78)	(2.50)	(2.68)	(1.81)	(1.50)	(1.52)
Controls	YES	YES	YES	YES	YES	YES	YES
SIC2 FE	YES	YES	YES	YES	YES	YES	YES
Constant	0.4064	-1.3020	-1.1810	-3.6068	-4.1693*	-9.8855***	-2.1300
	(0.20)	(-0.37)	(-1.38)	.	(-1.71)	(-10.20)	(-1.02)
Observations	13,323	6,658	6,657	5,815	6,282	4,402	3,842
Adjusted R^2	0.198	0.211	0.195	0.187	0.206	0.207	0.178

This Panel presents the results examining the impact of short selling threats on tax fees. Column 1 is the full sample analysis. Column 2(3) includes firms with high (low) bankruptcy risk based on Altman Z-score (1968). Column 4(5) includes firms with positive (negative) discretionary accruals based on McNichols (2002). Column 6(7) includes firms with auditors whose market share in the SIC 2-digit industry is (not) the largest in the city.

The "Diff: p-value" compares the difference between two coefficients of *Pilot×During* in the same partition using Chi-square tests.

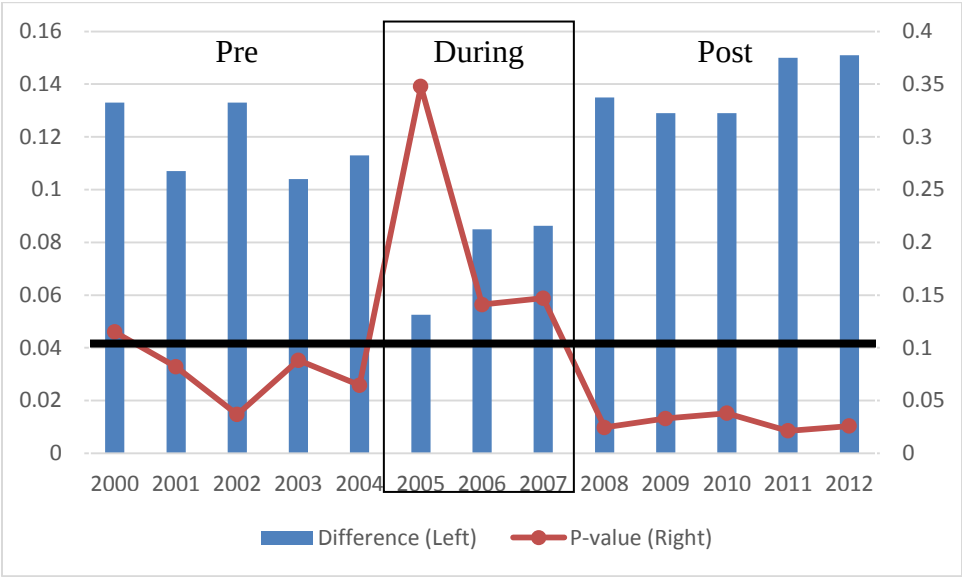
All variables are defined in the Appendix. *t* statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided test

Table 8: Intersection Analyses

DV=LnAF	(1) High Bankruptcy Risk & DA>0	(2) Low Bankruptcy Risk & DA<0	(3) High Bankruptcy Risk & High Auditor Power	(4) Low Bankruptcy Risk & Low Auditor Power	(5) DA>0 & High Auditor Power	(6) DA<0 & Low Auditor Power	(7) High Bankruptcy Risk & DA>0 & High Auditor Power	(8) Low Bankruptcy Risk & DA<0 & Low Auditor Power
Pilot	-0.0720*	0.0045	-0.1607***	-0.0062	-0.0765	0.0329	-0.1217**	-0.0197
	(-1.83)	(0.14)	(-3.39)	(-0.14)	(-1.58)	(0.69)	(-2.14)	(-0.30)
During	0.8971***	1.0411***	0.8461***	1.0906***	0.9402***	1.0788***	0.8557***	1.0427***
	(8.59)	(11.14)	(8.05)	(13.01)	(8.84)	(11.34)	(7.87)	(13.18)
Pilot×During	0.1075**	-0.0384	0.2636***	-0.0656	0.1082	-0.0493	0.2437***	0.0049
	(2.44)	(-1.22)	(5.92)	(-1.41)	(1.62)	(-1.04)	(3.20)	(0.07)
	(Diff: p-value=0.006)		(Diff: p-value=<0.001)		(Diff: p-value=0.054)		(Diff: p-value=0.022)	
Post	0.8622***	0.9197***	0.7517***	0.9897***	0.8613***	1.0088***	0.7909***	0.9942***
	(8.19)	(9.78)	(6.98)	(10.51)	(7.66)	(10.10)	(6.94)	(10.96)
Pilot×Post	-0.0118	-0.0180	0.1411**	0.0252	-0.0334	-0.0290	0.0353	0.0491
	(-0.28)	(-0.52)	(2.31)	(0.38)	(-0.54)	(-0.57)	(0.39)	(0.57)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
SIC2 FE	YES	YES	YES	YES	YES	YES	YES	YES
Constant	8.3138***	9.1756***	8.7344***	9.2891***	9.0582***	9.9214***	8.9363***	9.0650***
	(19.20)	(31.00)	(49.57)	(48.02)	(51.91)	(35.53)	(42.75)	(31.58)
Observations	3,328	3,609	2,282	2,001	1,858	1,761	1,116	1,050
Adjusted R^2	0.814	0.824	0.831	0.817	0.828	0.794	0.821	0.805

This table examines the intersections of different partitions. Column 1(2) includes firms with high (low) bankruptcy risk based on Altman Z-score (1968) and with positive (negative) discretionary accruals based on McNichols (2002). Column 3(4) includes firms with high (low) bankruptcy risk based on Altman Z-score (1968) and with auditors whose market share in the SIC 2-digit industry is (not) the largest in the city. Column 5(6) includes firms with positive (negative) discretionary accruals based on McNichols (2002) and with auditors whose market share in the SIC 2-digit industry is (not) the largest in the city. Column 7(8) includes firms with high (low) bankruptcy risk based on Altman Z-score (1968), with positive (negative) discretionary accruals based on McNichols (2002), and with auditors whose market share in the SIC 2-digit industry is (not) the largest in the city.
The "Diff: p-value" compares the difference between two coefficients of *Pilot×During* in the same partition using Chi-square tests.
All variables are defined in the Appendix. *t* statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests)

Figure 1: By-year Comparison of Audit fees (LnAF) between Pilot and Non-pilot Firms



This figure illustrates the by-year comparison of LnAF between pilot and non-pilot firms. The bar (Left) indicates how much the mean LnAF of non-pilot firms is larger than pilot firms in a given year. The line (Right) indicates the p-values of the difference. We can find that the blue bars are lowest (i.e., smaller than 0.1) in During-pilot period (i.e., 2005-2007) and the p-values are largest (i.e., around 0.15 or higher).