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 with firm fixed effects to show that auditors react to the threats and charge higher audit fees to the pilot firms. Further, we find that the impact only exists when auditors are concerned with the bankruptcy risk or when managers are less disciplined by short sellers. Equally importantly, we conduct multiple tests to attempt to parse out the effects of increased audit effort from increased risk premium. We conclude that increased risk premium is more likely to be the dominant channel for the higher audit fees. 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 behavior.

Key Words: Short Selling, Audit Fees, Regulation SHO, Bankruptcy Risk, Earnings Management,

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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 audit fees. Our focus on auditors is directly motivated by the potential litigation risk of auditors imposed by 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 Forest'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

[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.

in higher litigation risk for auditors. In other words, conditioning on the probability of the auditors making mistakes, greater short-selling threats increase the probability of the mistakes being discovered. Thus, higher ex-ante threats of short-selling activities lead to higher litigation risk facing the auditors. To offset such increase in litigation risk, one possibility is that auditors increase their audit effort to reduce the probability of making mistakes. As a result, they ask for higher fees to compensate for their increased effort (audit-effort channel). Another possibility is that auditors shift the burden of increased risk to shareholders, by charging higher fees without increasing their audit effort (risk-premium channel). Through either or both of the audit-effort or risk-premium channels, higher ex-ante short-selling threats should lead to higher audit fees.

Ex ante, however, it is not clear whether these predictions on audit fees 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 to either 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 (2016) show that short-selling threats constrain managers' earnings-management incentives. 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 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 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 behavior. We adopt a difference-in-differences design with firm fixed effects, 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 experience a 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 an overall $261 million extra cost of auditing for the 986 pilot firms from 2005 to 2007.

Motivated by the tension discussed above, we explore cross-sectional variations in the impact of short-selling threats on audit fees. Consistent with our predictions, we find that the impact of the increase of short-selling threats is highly significant for client firms with higher bankruptcy risk, those are the firms for which the change of the short-selling threats is more likely to be realized, but not for firms with lower bankruptcy risk. Also, our results show significant increase in audit fees for firms with less-disciplined managers but not so when the firms' managers are

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

more disciplined. These subsample analyses thus provide strong evidence supporting the impact of short-selling threats on audit fees. Our inferences are robust to alternative partition variables, alternative specifications, and a battery of sensitivity analyses.

Next, we address several potential alternative explanations for our results. First, our results are unlikely driven by short interest as a signal of audit risk because we control for actual (or ex-post) short-interest positions throughout the paper. Second, we analyze SEC comment letters and conclude that our findings are unlikely to be driven by the possibility that the SEC increased scrutiny of pilot firms during the pilot program. Third, we test whether clients *demand* higher audit quality in the presence of short-selling threats. We find no evidence that pilot firms are more likely to switch to higher-quality auditors during the pilot program period. We also find that the effect of short-selling threats on audit fees is only significantly positive for auditors with higher bargaining power, consistent with auditors asking for higher fees rather than clients demanding higher audit quality in the presence of short-selling threats.

As we identify increased risk premium and enhanced audit effort as two channels through which short-selling threats can affect audit fees, we conduct multiple empirical analyses to assess the relative merits of these competing explanations. The major difference is that increased audit effort leads to higher audit quality, while risk-premium shifting does not. This contrast motivates us to rely on audit-quality measures to assess which channel dominates. As DeFond and Zhang (2014) summarize, audit-quality measures can be broadly classified as output- and input-based measures. From the output perspective, we evaluate audit quality using material restatements, auditor communications (i.e., going-concern opinions), financial reporting quality (i.e., discretionary accruals), and market perception (i.e., earnings response coefficients). From the input perspective, we consider whether the auditor spends more time on the audit process. Across all five measures of audit quality, we find no significant evidence that audit quality is improved in the presence of short-selling threats. Further, our main result remains statistically and economically similar after we include all these audit-quality measures as additional controls. In sum, although still indirect, these findings provide strong evidence that increased risk premium rather than higher audit effort, is likely the main channel through which short-selling threats lead to higher audit fees.

This paper contributes to the literature by being among the first to examine third-party consequence of short-selling threats. Several 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 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. In this way, our findings also have implications for regulators' evaluations of costs and benefits of future regulations on short selling. Finally, this article establishes the causal impact of short-selling threats on auditor behavior. Using the pilot program as a controlled experiment, we are able to draw the causal inference that short-selling threats are a determinant of audit fees.

The next section reviews the related literature and develops the hypotheses. Section 3 describes the research design and the sample. Section 4 reports the main results as well as robustness checks. Section 5 addresses three alternative explanations. Section 6 attempts to separate the risk-premium channel from the audit-effort channel. 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 15 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 (2015) find that short-selling pressure can affect corporate innovation efficiency.

Short-selling threats can also affect corporate accounting decisions. Fang et al. (2016) 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 readability 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 (2015) 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), Gul and Goodwin (2010), and Bruynseels and Cardinaels (2014) 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 (actual) short interest and audit fees. Our paper complements theirs but is different for at least three 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).[3] We use the SEC SHO pilot program that arbitrarily removes short-selling restrictions of one third of Russell 3000

[3] It is important to note that the causal inference in Cassell 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 Cassell 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.

index firms to establish causality. Finally, empirically speaking, we control for short interest throughout the paper, indicating that the impact of short-selling threats on audit fees is beyond and above the risk information contained in short interest.

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(θ). Clearly LR(θ) 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. For example, the auditors' mistakes might not have been identified if there were no short sellers.[4] Therefore, the existence of short-selling threats will amplify the auditors' litigation risk if auditors make mistakes. 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). This means that conditioning on the probability of auditors making mistakes, greater short-selling threats increase the probability of the mistakes being discovered. From the auditors' perspective, they would react to the increased litigation risk by charging higher fees, which reflect either increased

[4] 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).

audit effort or a pure shift of risk premium, or both (Simunic and Stein 1996).[5] Therefore, our first hypothesis (stated in the alternative form) is as follows:

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

Next, we consider two cross-sectional hypotheses, both of which 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.

First, for financially healthy firms that are of lower bankruptcy risk, the reduction of the short-selling restrictions is less likely to result in higher short-selling threats as it might not be optimal for short sellers to bet against those firms even without any restrictions. In these cases, changing to lower restrictions might not lead to an actual increase in the threats and would not matter for the auditors' litigation risk and thus pricing decisions. On the other hand, for firms with financial difficulties, such as those of higher bankruptcy risk, short sellers are more likely to take action once the restrictions become lower. 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.[6] In other words, when the bankruptcy risk is high, $LR(\text{No Threat} \mid \theta) < LR(\text{Threat} \mid \theta)$. Therefore, hypothesis 2.1 (stated in the alternative form) is as follows:

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[5] Here we implicitly assume that short sellers trade on "correct" information rather than misleading information. However, our arguments are not affected by whether short sellers are manipulators or fundamental traders. As long as the stock prices drop after the short-selling activities and auditors are faced with higher scrutiny from both regulators and investors, auditors are motivated to charge higher fees because of the increased litigation risk. In addition, although the difference between short sellers' roles as monitors and manipulators is clear at the conceptual level, it is more difficult to distinguish them empirically. For example, in the New York Times best-seller book "*Fooling Some of the People All of the Time*," the short seller David Einhorn was described as a market manipulator for years but finally he turned out to be right in his opinion on the shorted firm - Allied Capital.

[6] Another way of thinking about this is that short sellers may discipline auditors (just like they discipline managers as per Fang et al. 2016) for catching material and marginally material mistakes, and this is especially true for firms that are at the edge of bankruptcy.

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

Fang et al. (2016) show that managers reduce their upward earnings management when faced with greater threats from short sellers. Such actions could counteract the increased threats, leading to no actual increase in the threats. In other words, the potential increase in short-selling threats after the reduction of the restrictions could be completely offset by the fact that managers react to the change by being "more disciplined." Thus, for firms with managers who are more disciplined, we should see no or smaller increase in audit fees. In contrast, for firms whose managers are not reacting by being more disciplined, we predict greater increase in audit fees. Therefore, hypothesis 2.2 (stated in the alternative form) is as follows:

Hypothesis 2.2: The impact of short selling on audit fees is more pronounced for firms whose managers are less disciplined by the short sellers.

3. Research Design

3.1 Identification Strategy

Our identification strategy is based on the SEC SHO pilot program, which provides a controlled experiment that generates exogenous variations in short-selling threats. 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.[7] In each group, every third stock was selected as a pilot stock. From May 2, 2005 to August 6, 2007, pilot stocks were 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 exclusion 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. (2016) 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.

$$\begin{aligned} LnAF_{i,t} = {} & \alpha_0 + \alpha_1 During_{i,t} + \alpha_2 Pilot_{i,t} \times During_{i,t} + \alpha_3 Post_{i,t} + \alpha_4 Pilot_{i,t} \times Post_{i,t} \\ & + \alpha_5 Size_{i,t} + \alpha_6 Leverage_{i,t} + \alpha_7 BTM_{i,t} + \alpha_8 ROA_{i,t} + \alpha_9 Loss_{i,t} + \alpha_{10} CA/TA_{i,t} \\ & + \alpha_{11} Quick_{i,t} + \alpha_{12} INVREC_{i,t} + \alpha_{13} SizeGrowth_{i,t} + \alpha_{14} FYEnd_{i,t} + \alpha_{15} NBusSeg_{i,t} \\ & + \alpha_{16} BIG4_{i,t} + \alpha_{17} MNC_{i,t} + \alpha_{18} ShortInterest_{i,t} + FirmFE_i + \varepsilon_{i,t} \end{aligned} \tag{1}$$

[7] 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).

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 2013.[8] *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 otherwise; *Post* is one for observations from year 2008 to 2013 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 prior literature and exclude all observations in year 2004. Note the benchmark is the period before the pilot program (i.e., years before 2004). H1 is supported if α_2 is significantly positive and α_4 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; Bruynseels and Cardinaels 2014), we include control variables both related to general firm characteristics and specific audit-related factors. In the first group we include firm size (*Size*), Leverage (*Leverage*), Book-to-Market ratio (*BTM*), and profitability (*ROA*), and in the second an indicator for loss (*Loss*), 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 (*SizeGrowth*), indicator for fiscal year not ending in December (*FYEnd*), number of business segments (*NBusSeg*), indicator for being audited by Big 4 auditors (*BIG4*), indicator for being a multinational company (*MNC*), and the level of short interest (*ShortInterest*).

[8] Audit-fee data are available starting in 2000. If we instead end the sample period in 2010 (as in Fang et al. 2016), no inferences are affected.

Importantly, we also include firm fixed effects in all empirical analyses. By doing so we control for *all* potential time-invariant firm-level omitted variables. By including firm fixed effects, we focus on within-firm variations.[9,10] All variables are defined in the Appendix.

3.3 Sample Selection

Panel A of 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 16,483 firm-year observations for 1,610 individual firms, including 538 pilot firms and 1,072 non-pilot firms. In the full sample, 760 firms (47.2%) are listed on NYSE, 827 firms (51.4%) on NASDAQ, and 23 firms (1.4%) on AMEX.[11] Table 1, Panel B illustrates the year distribution of the final sample. As expected, the number of observations peaks in years closer to 2004 (the year the sample constriction starts) and decreases in years before and after 2004.

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 and the reported t-statistics are based on standard errors clustered by firm (Petersen 2009).[12]

[9] In untabulated analyses we redo the analyses without firm fixed effects but with SIC two-digit industry fixed effects. No inferences are affected.
[10] We control for time-period effects through the inclusion of *During* and *Post*. In untabulated analyses we replace these indicators with year fixed effects and conclusions are unaltered.
[11] Our sample construction is very close to other papers using the same setting. For example, we have 2,595 firms after matching PERMNO and excluding firms not in Russell 2005 index. For comparison, Li and Zhang (2015) have 2,604 firms. Also, while we end up with 1,899 firms on the final list consisting of pilot firms and non-pilot firms, Grullon et al. (2015) have 1,930 firms on the list.
[12] Inferences are unchanged if we cluster by both firm and year as in Fang et al. (2016).

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 2.16 million dollars on average each year in audit fees. Panel B compares pilot firms with non-pilot firms before the pilot program (i.e., year 2000 – 2003). For many variables, pilot firms and non-pilot firms are indistinguishable. However, audit fees are slightly larger for non-pilot firms. Clearly such a difference in the Pre-pilot period is exogenous to the treatment effect in our setting because the SEC arbitrarily chose pilot firms in 2004. A likely explanation is that the SEC picked every third stock as a pilot firm based on trading volume. Thus, firm size is slightly higher on average for non-pilot firms. Although the difference in firm size is not statistically significant, we know from the audit literature that firm size is a very important explanatory variable for audit fees. However, note that we include *firm fixed effects* in our regression analyses and rely on difference-in-differences estimations, thus controlling for such factors.[13,14]

Table 3 provides a Pearson correlation matrix for all variables used in the main analyses. Audit fees are positively correlated with *During*, *Post*, *Size, Leverage, ROA, InvRec, NBusSeg, BIG4*, *MNC*, and *ShortInterest* and negatively correlated with *Pilot*, *BTM*, *Loss, CA/TA, Quick, SizeGrowth,* and *FYEnd*. These correlations are generally consistent with prior literature.

[13] In addition, we conduct a sensitivity analysis (Section 4.5) in which we match very finely on firm size.

[14] Although the magnitudes of the differences are very small, Leverage, ROA, Loss, and Quick also display statistically differences across the two samples. Again, we control for these firm-level characteristics and importantly include firm fixed effects in all empirical tests. Also, if we compare pilot firms with non-pilot firms in 2004 (i.e., the year in which pilot firms were chosen), none of these differences are significant.

4.2 Univariate Analyses

Table 4 presents the univariate difference-in-differences test results. Row 1 in Table 4 again shows the higher audit fees for non-Pilot firms in the pre period. Row 2 shows that such a difference narrows down during the pilot program such that the difference becomes insignificant. It becomes significant at the 1% level in the Post-pilot period, when both pilot and non-pilot firms are exempted from price tests.

Analyses on the time-series differences provide similar inferences. Mainly because of the SOX (e.g., Ghosh and Pawlewicz 2009), both pilot and non-pilot firms witness huge increase in audit fees from the Pre- to the During-period, but the difference-in-differences test shows that pilot firms' audit fess increase more. We attribute such additional increase in pilot firms' audit fees to the removal of price tests on them.

From the During- to the Post-period, however, non-pilot firms experienced an increase in short-selling threats because the SEC further removed price tests on these firms. At the same time, pilot firms underwent no such changes, given that the price test had already been removed for them. As a result, we observe a significant increase in audit fess for non-pilot firms and no significant change for pilot firms. To summarize, these results provide preliminary support for our main hypothesis (H1).

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 only general firm characteristics as control variables and Column 2 adds the audit-specific controls. The adjusted R^2s are 92% and most control variables have the same signs as in previous research. The coefficients for both *During* and *Post* are significantly positive, indicating that the average audit fees in the *During* and *Post* periods are higher than the fees in the Pre-pilot period, consistent with the univariate analyses in Table 4. 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_2 *(Pilot* × *During*) and α_4 (*Pilot* × *Post*). We find that α_2 is significantly positive at the 0.05 level (using two-sided tests) but α_4 is insignificant in both regression specifications, 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_2=0.0465) 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 261 million dollars in audit fees had the price test been not exempted.[15] 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 (H2.1 and H2.2)

In this section we test H2.1 and H2.2 by exploring whether there are cross-sectional variations in the impact of short selling on audit fees.

[15] 4.65% × average audit fees of 1.9 million for pilot firms in 2004 × 986 pilot firms × 3 years in pilot program. Note that when the coefficient is small, we can interpret the coefficient of a log-linear model as the percentage of change in the dependent variable.

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 Bharath and Shumway's (2008) "distance to default" model to model bankruptcy risk. Specifically, we classify firms with higher than 5% expected default frequency every year in the pre-Pilot period as the Higher-Bankruptcy-Risk subsample and firms with at least one year lower than 5% as the Lower-Bankruptcy-Risk subsample. H2.1 predicts that the impact of short-selling threats on audit fees only exists (and would be larger) in the Higher-Bankruptcy-Risk subsample.

Columns 1 and 2 of Table 6 present the results. We find that the coefficient of *Pilot* × *During* is significant at the 0.05 level for the Higher Bankruptcy-Risk subsample but not significant for the Lower-Bankruptcy-Risk subsample. In addition, a Chi-square test shows that the difference between these two coefficients is significant (two-sided p-value = 0.06).[16] 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 threats. As discussed, Fang et al. (2016) show that

[16] Alternatively, we add a three-way interaction to the regression to test the differences between coefficients from subsamples. Our inferences remain. Also, recall that we use firm fixed effects in all analyses. Although a strong econometric specification, it is not obvious that this specification is optimal when we partition on firm characteristics. In untabulated analyses we replace firm fixed effects with industry fixed effects and find a two-sided p-value of 0.03 for the difference between subsamples.

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 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 (2016) logic, we classify managers who *ex post* do upward earnings manager as less disciplined.

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.[17] The residuals are labeled as discretionary accruals. We partition the full sample based on whether discretionary accruals in 2005 are positive (Less Disciplined) or negative (More Disciplined). H2.2 predicts that the impact of short-selling threats on audit fees only exists and would be larger for the subsample with Less-Disciplined Managers.

Columns 3 and 4 of Table 5 present the results. We find that the coefficient of *Pilot* × *During* is significant at the 0.05 level for the Less-Disciplined subsample but not significant for the More-Disciplined subsample. The Chi-square test shows that the difference between these two coefficients is weakly significant (two-sided p-value = 0.13).[18] 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.

[17] $TA_{j,t} = \phi_{0,j} + \phi_{1,j} CFO_{j,t-1} + \phi_{2,j} CFO_{j,t} + \phi_{3,j} CFO_{j,t+1} + \phi_{4,j} \Delta Rev_{j,t} + \phi_{5,j} PPE_{j,t} + \nu_{j,t}$, 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.
[18] When firm fixed effects are replaced with industry fixed effects the two-sided p-value is 0.09.

4.5 Untabulated Robustness Checks

We conduct several sets of robustness tests to check whether our conclusions are sensitive to research-design choices. First, we use different sample periods and specifications: (1) shortening the sample period to 2000 – 2007 to only focus on the Pre- and During- periods, (2) including observations in year 2004 as Pre-pilot period, (3) interacting all control variables with *During*, (4) 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 (5) *matching* pilot firms with non-pilot firms very finely on firm size (i.e., smaller than 1% difference in size) to strictly control for the impact of size on audit fees. We observe that all coefficients of *Pilot* × *During* for all these tests are close to those in Table 5 in both magnitude and significance (untabulated). In sum, our conclusions are not sensitive to a variety of research-design choices.

Second, to confirm that our sub-sample inferences are not sensitive to the specific partition variables, we employ alternative approaches. For bankruptcy risk, we alternatively reply on Altman Z-scores (Altman 1968), Ohlson (1980) bankruptcy scores, or Hillegeist, Keating, Cram, and Lundstedt (2004) and obtain qualitatively similar results. For whether managers are disciplined by short-selling threats or not, the results are very close if we calculate DA based on the modified Jones model (Dechow, Sloan, and Sweeney 1995), the DD model (Dechow and Dichev 2002), or adjust by performance-matched firms (Kothari, Leone, and Wasley 2005). In addition, we also draw insights from the recent literature on General Counsel (GC) in top management (e.g., Kwak, Ro, and Suk 2012; Hopkins, Maydew, and Venkatachalam 2015) to use the existence of GC as an ex-ante measure of managers being disciplined.[19] We find that the coefficient of *Pilot* × *During* is

[19] As Kwak et al. (2012, 19) note, *"(GC) plays two key roles for his (her) firm: (a) advising top management on various matters, including legal issues and litigation risk; and (b) monitoring top management's unusual behavior against shareholders' interest.*" Therefore, we expect that those firms with GC in top management would be more likely disciplined by the short-selling threats after the removal of price tests. Following prior literature, we define GC firms

significant for firms with no GC in top management (i.e., Less Disciplined) from 2000 to 2004 and insignificant for other firms. Overall, these results suggest that our inferences are not contingent on the partition variables we choose.

5. Possible Alternative Explanations

5.1 Short Interest as a Signal of Audit Risk

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. As seen in Table 5, we present results both with and without *ShortInterest*. Similar to Cassell et al. (2011), *ShortInterest* loads significantly positive. More important, the coefficient estimate for our test variable is virtually unchanged after controlling for *ShortInterest*. We conclude that our findings are incremental to those reported in Cassell et al. (2011) and therefore unlikely driven by their explanation.

5.2 SEC Increased Scrutiny of Pilot Firms

A conceivable alternative explanation of our main results is the possibility that the SEC increased scrutiny of the 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 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

as those firms in which at least one among the top five highest-paid officers have titles containing the word "counsel," "law," "legal," or other variant. During our sample period, there are 36.5% firms are identified as GC firms, which is very close to the statistic of 37% reported in Hopkins et al. (2015).

pilot/non-pilot status, as such action would severely confound their experimental interpretations. Second, it is not clear how this possibility can explain our cross-sectional results: the increase of audit fees only exists for firms with higher bankruptcy risk and with less-disciplined managers.

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-letter 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. As we can see from Panel A of Table 7, on average pilot firms are slightly less likely to receive comment letters during the pilot program (27.8% for pilot firms vs. 29.1% for Non-Pilot firms) but slightly more likely in the Post-pilot period (48.6% for pilot firms vs. 46.8% for Non-Pilot firms). In conclusion, our evidence suggests that the SEC did not increase the scrutiny of pilot firms.

5.3 Auditors' Response to Clients' Demand for Higher Audit Quality

We interpret our results as auditors charging higher fees for increased litigation risk. However, an alternative explanation would be that clients demand more work from auditors in the presence of heightened short-selling threats. In Section 6 we further explore whether the increased fees are because of increased audit effort or risk premium. This section provides two reasons why our

[20] Prior research uses receiving comment letter to proxy for the SEC scrutiny (e.g., Cassell, Dreher, and Myers 2013).

results are better aligned with the explanation that auditors ask for higher fees in the presence of higher litigation risk.

First, auditor switches can help to disentangle these two explanations. Specifically, if the observed result reflects clients' demand for higher audit quality (and thereby higher fees), they would also be more likely to switch to higher-quality auditors. By contrast, if auditors' concern over litigation risk leads to higher audit fees, clients would be more likely switching to lower-quality auditors to avoid higher fees.

Panel B of Table 7 presents the results based on univariate analyses. We define *Upward Switch* as switches from Non-Big4 to Big4 and *Downward Switch* as switches from Big4 to Non-Big4 auditors. We present the *Up/Downward switch* statistics both conditional and unconditional on switches. For example, in Row 2 of the *Upward Switch* table, there are 161 switch events for non-pilot firms and 87 for pilot firms. Conditional on these switch events, 7.5% out of the 161 non-pilot switches and 3.4% out of the 87 pilot switches are *Upward Switches*. Unconditionally speaking, 0.4% of 2,842 non-pilot observations and 0.2% of 1,459 pilot observations witness *Upward Switches*.

We observe that in both Pre- and Post-pilot periods, there is little difference in either *Upward* or *Downward Switches* between pilot and non-pilot firms. However, in the During-pilot period, pilot firms have fewer *Upward Switches* but significantly more *Downward Switches*. These findings are consistent with clients not demanding higher audit quality in the presence of short-selling threats, but rather auditors asking for higher audit fees because of increased litigation risk (therefore driving some of the clients to lower-quality auditors).

Second, auditor bargaining power can provide additional insights. 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." If the audit fees are driven by auditors' concern over litigation risk, whether auditors can charge higher fees depends on how much they can persuade their clients, either by credibility or power, or both. By contrast, if the audit fees increase is a result of the clients' demand, we should not observe such variation in auditors' bargaining power. We follow the prior literature (e.g., Numan and Willekens 2012) and use the auditor's industry leadership at the city level as a proxy for superior bargaining power. 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. Panel C of Table 7 shows that the coefficient of *Pilot* × *During* is only significantly positive for auditors with higher bargaining power (i.e., industry leaders every year in Pre-pilot period). As explained earlier, such evidence is consistent with our proposed explanation of auditors asking for higher fees rather than clients demanding more work in the presence of short-selling threats.

6. Separating Risk Premium and Increased Effort

DeFond and Zhang (2014, 298) observe 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 as 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. However, this insight suggests a way to separate the two channels based on their different implications on audit quality. Specifically, we can first examine whether audit quality increases for Pilot-firms in the presence of greater short-selling threats, and then check whether our results hold after controlling for audit-quality measures.

As summarized by DeFond and Zhang (2014, 283), two main ways to infer audit quality are to "consider outputs of the audit process" and to "consider audit inputs." In particular, we follow their suggestion to use measures across four output-measure categories, including material misstatements, auditor communication, financial reporting quality, and investors' perceptions. In addition, we also apply one input-based measure: audit-report lag. Finally, we explore whether short-selling threats affect auditor resignations and auditor dismissals.

6.1 Material Misstatements

As Lobo and Zhao (2013) summarize, audit theory predicts a negative relation between audit effort and the future restatement of the current-year financial reports, because audit effort reduces the probability of undetected errors. Further, they argue that audit effort matters more for annual reports, which are audited by auditors, than quarterly reports, which are only reviewed. Following this logic, if audit effort increases in the presence of short-selling threats, we would find that Pilot firms have lower likelihood of restating During-period annual reports, benchmarking on the likelihood of restating During-period quarterly reports.

Following Lobo and Zhao (2013), we use the Audit Analytics Advanced Non-Reliance Restatement database and define *ANNRestate* as one for fiscal years having annual financial reports restated and zero otherwise, and *QTRRestate* as one for fiscal years having *only* quarterly financial reports restated and zero otherwise. In Column 1 of Table 8, we use *ANNRestate* as the dependent variable and find that the coefficient of *Pilot* × *During* is positive and significant at the 10% level. As a benchmark, in Column 2, we replace *ANNRestate* with *QTRRestate* and observe that *Pilot* × *During* is also positive but not significant. Univariate analysis shows that pilot firms have both higher likelihood of quarterly restatements (6.92% vs. 5.70%) and annual restatements (11.4% vs. 9.43%) than non-Pilot firms in the During-pilot period, but the differences between pilot and non-pilot firms are almost identical for both QTR and ANN restatements: (6.92% - 5.70%)/5.70% =0.2140 and (11.4% - 9.43%)/9.43%=0.2142. In other words, the restatement results suggest that the audit quality is largely unaffected by the short-selling threats.

6.2 Auditor Communications

Going-concern opinions (GCO) represent an important way in which the auditor communicates with the shareholders about her substantial doubt that the client can continue as a going concern. GCO represents mainly the independence dimension of audit quality, because managers are motivated to avoid GCO given its serious capital-market consequences. For example, Krishnan (1994) shows that auditors would likely lose the clients after issuing GCOs.

In Column 3 of Table 8 we use *GCO* (defined as one for firm-years with GCOs and zero otherwise) as the dependent variable. The coefficient of *Pilot* × *During* is positive but not significant, indicating that there is no significant evidence that pilot firms are more likely to receive GCOs (i.e., higher audit quality) than non-pilot firms in the presence of short-selling threats.

6.3 Financial Reporting Quality

High-quality audits are expected to detect earnings management and improve financial reporting quality. As one recent example, Lennox, Wu, and Zhang (2016) compare the pre-audit and audited values of pre-tax accounting numbers in China and confirm that audit adjustments improve accruals quality. To be consistent with our cross-sectional tests, we use McNichols (2002) to model normal accruals and define *DA* as the residuals. In Column 4 of Table 8 we use DA as the dependent variable and find that the coefficient of *Pilot* × *During* is negative but not significant.

6.4 Investors' Perceptions

High-quality audits should increase the credibility of audited accounting numbers and lead to larger market reactions for a given level of reported earnings. Teoh and Wong (1993) show that the earnings response coefficients (ERCs) of Big-8 clients are significantly higher than for non-Big-8 clients. We use all quarterly earnings announcements for each firm in each of the three periods (i.e., Pre-, During-, or Post-pilot period) and regress the three-day CARs centered on the announcement dates on the unexpected EPS (benchmarking on the most recent analyst consensus), controlling for an indicator for negative EPS, its interaction with unexpected EPS, and the accumulated returns from the analyst consensus date to the earnings-announcement date.[21] *ERC* is the coefficient of unexpected earnings for each firm in a given period. Then we use *ERC* as a dependent variable in Column 5 of Table 8 and find that the coefficient of *Pilot* × *During* is negative but insignificant.

6.5 Input-Based Measure

In addition to the previous four output-based measures, we consider one input-based audit-quality measure. Specifically, 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

[21] We acknowledge that the removal of price tests might affect the price-discovery process and thus the ERCs, so the reader should be aware of the limitations of using this perception-based quality measure in our setting. However, it serves as an investor-based complement to the other measures.

and the audit-report date. We use *LnARL* as the dependent variable in Column 6 of Table 8, and we find that the coefficient of *Pilot* × *During* is positive but not significant.

6.6 Auditor Switches

Next, we consider two types of auditor switches: dismissals and resignations. This analysis will not only help us understand the auditor-switches better, but also has the potential to further differentiate between the audit-effort and risk-premium shift channels. The difference between the two types of switches is believed to be that dismissals are originated by clients, while resignations by auditors. The two different channels (risk premium and audit effort) might lead to switches initiated by different parties as they have different impacts on shareholders (or on the risk-benefit sharing between shareholders and auditors). If the auditors only charge higher fees to shift the burden of increased risk, the increased fees are a pure cost for shareholders, thus clients dismissing auditors is more likely conditioning on a switch happening. In contrast, if the increased fee is to compensate for auditors' higher effort, the increased fees might not be costly and can even be beneficial to shareholders. A switch is then more likely to be caused by auditor resignation. To summarize, we expect more dismissals if the risk-premium channel dominates, and more resignations if the audit-effort channel dominates.

Notwithstanding this logic, we acknowledge the complex nature of auditor switches and in particular the possibility that both types of switches could happen with either of the channels.[22] With this caveat in mind, we use *Dismissal* (defined as one for firm-years with auditor dismissals) and *Resignation* (defined as one for firm-years with auditor resignations) as dependent variables in Equation (1). The coefficients of *Pilot* × *During* are positive for the *Dismissal* regression but

[22] As long as one party is incurring economic losses, they will refrain from the relation. It is also worth mentioning that the incidence of both types of switches is low and might affect the statistical significance.

negative for *Resignation* regression, with both being insignificant. This weakly suggests that risk premium is a more likely the dominant channel that leads to auditors charging higher fees.

6.7 Controlling for Audit-Quality Measures and Switches

None of the above audit-quality measures suggest significant improvement in audit quality in the presence of short-selling threats. As a final step, we include all the above audit-quality measures and two types of auditor switches as additional control variables in Equation (1) to check whether these variables can explain away the significance of *Pilot×During*. In Column 9 of Table 8 we find that the coefficient of *Pilot×During* is largely unchanged compared with Table 5 (Coeff. = 0.0475 vs. 0.0465 in Table 5). These results suggest that risk premium, rather than audit effort (and thus audit quality), is the most likely channel through which auditors charge higher fees in the presence of short-selling threats. However, we caution readers in interpreting these findings. Although we come to this conclusion using multiple measures and different specifications, we acknowledge that we do not have a direct measure of risk premium and have to reply on the above indirect approaches. As a result, the interpretation is only as good as the extent to which we can infer audit effort from these output- and input-based audit-quality measures.

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 with firm fixed effects, we find that higher short-selling threats increase audit fees. In cross-sectional analyses, we further show that the impact of short selling on audit fees only exists when auditors care more about the bankruptcy risk and when the managers are less disciplined by short-selling threats. Further, we employ various audit-quality measures and conclude that audit fees increase because auditors charge higher premium, not because they increase efforts and therefore improve audit quality.

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.[23]

[23] We caution excessive interpretation of our results. We show that the auditors charge higher fees in response to the increase in short-selling threats because of litigation risk. This is a third-party consequence that probably did not factor into regulators' concerns when they designed the regulations. Our results do not necessarily imply that auditors welcome the removal of short-selling restrictions. As Section 6 shows, if the increased fees are used to compensate the higher litigation risk, it is not clear whether and how auditors' utility will be affected. In fact, it is possible that auditors have utility losses because of the removal of price tests. Regarding the welfare implications to the society, we identify a specific type of costs to the shareholder community in the presence of short-selling threats to the extent that audit quality is not sufficiently increased. However, evaluating the overall (or net) effects of short-selling threats to society in general is beyond the scope of this paper.

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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 2013 and zero otherwise
Pilot	One for Pilot firms and zero otherwise
Control Variables	
Size	Log of total sales
Leverage	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
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
SizeGrowth	The growth of total assets
FYEnd	One for firms with fiscal year ends not in December and zero otherwise
NBusSeg	Log of one plus the number of business segments
BIG4	One for big 4 auditor and zero otherwise
MNC	One for firms with foreign operations and zero otherwise
ShortInterest	The ratio of shares in short position to the total shares outstanding in the month prior to the fiscal year starts
Cross-Sectional Variables	
Bankruptcy Risk (EDF)	Firms with expected default frequency (EDF; Bharath and Shumway 2008) larger than 5% every year from 2000 – 2004 are defined as High Bankruptcy Risk subsample; firms with EDF smaller than 5% at least one year from 2000 to 2004 are defined as Low Bankruptcy Risk subsample
Disciplined Managers (Earnings Management)	Firms with positive Discretionary Accruals (based on McNichols 2002) in 2005 are defined as Less Disciplined subsample; firms with negative Discretionary Accruals in 2005 are defined as More Disciplined subsample
Variables in the Alternative Explanations Section	
Upward Switch	Observations switching from Non-Big4 auditors to Big4 auditors
Downward Switch	Observations switching from Big4 auditors to Non-Big4 auditors
Auditor Power	Firms with city-leader auditors every year from 2000 – 2004 are defined as Higher Auditor Power subsample; firms without city-leader auditors at least one year from 2000 – 2004 are defined as Lower Auditor Power subsample. A city-leader auditor is an auditor whose market share (in terms of audit fees) is the largest in a given industry at the city level.
Variables in Analyses related to Audit Effort vs. Risk Premium	
ANNRestate	One if the annual financial statement is restated and zero otherwise
QTRRestate	One if financial statements of at least one quarter of the fiscal year are restated but the annual statement is not restated and zero otherwise

GCO	One for going concern opinion and zero otherwise
DA	Discretionary Accruals (based on McNichols 2002)
ERC	The earnings response coefficient specific to a firm in a given period (i.e., Pre-, During-, or Post- Pilot period). It is the coefficient of the unexpected EPS in a regression regressing CAR(-1, 1) around quarterly earnings report date on unexpected EPS (adjusted by the most recent analyst consensus), controlling for an indicator of negative EPS, its interaction with the unexpected EPS, and the cumulative CARs from the analyst consensus date to the earnings announcement date. All quarterly earnings announcements of the same firm in the same period are used for one regression.
LnARL	Log of one plus the number of days between fiscal year ends to the audit report date
Dismissal	One for firm-years with auditor dismissals and zero otherwise
Resignation	One for firm-years with auditor resignations and zero otherwise

Table 1: Sample

Panel A: Sample-Selection Steps

	Pilot Firms	Non-Pilot Firms	Total
Russell 3000 companies on June 30, 2004	986	2,012	2,998
Matched with PERMNO	979	2,003	2,982
Also in the Russell 3000 index on June 30, 2005	860	1,735	2,595
All matched with GVKEY	856	1,727	2,583
Excluding financial and utilities firms	635	1,264	1,899
Matched with AuditAnalytics data	603	1,206	1,809
Matched with all control variables	538	1,072	1,610
Final firm-year sample	5,614	10,869	16,483

Panel B: Sample Distribution by Year

Fiscal year	Freq.	Percent	Cum.
2000	962	5.84%	5.84%
2001	1,481	8.99%	14.82%
2002	1,533	9.30%	24.12%
2003	1,575	9.56%	33.68%
2005	1,539	9.34%	43.01%
2006	1,433	8.69%	51.71%
2007	1,329	8.06%	59.77%
2008	1,261	7.65%	67.42%
2009	1,212	7.35%	74.77%
2010	1,155	7.01%	81.78%
2011	1,097	6.66%	88.44%
2012	1,051	6.38%	94.81%
2013	855	5.19%	100.00%
Total	16,483	100.00%	

Table 2: Summary Statistics

Panel A: Full Sample

Variables	N	Mean	S.T.D	P25	P50	P75
AF ($)	16,483	2.16 M	3.67 M	0.48 M	1.05 M	2.24 M
LnAF	16,483	13.860	1.180	13.080	13.860	14.620
Pilot	16,483	0.341	0.474	0.000	0.000	1.000
During	16,483	0.261	0.439	0.000	0.000	1.000
Post	16,483	0.402	0.490	0.000	0.000	1.000
Size	16,483	6.751	1.816	5.701	6.753	7.881
Leverage	16,483	0.500	0.287	0.310	0.481	0.636
BTM	16,483	0.472	0.775	0.251	0.422	0.656
ROA	16,483	0.003	0.230	-0.004	0.046	0.085
Loss	16,483	0.260	0.439	0.000	0.000	1.000
CA/TA	16,483	0.482	0.228	0.313	0.479	0.653
Quick	16,483	2.349	2.795	1.009	1.519	2.566
InvRec	16,483	0.241	0.162	0.110	0.222	0.339
SizeGrowth	16,483	0.145	0.591	-0.031	0.055	0.165
FYEnd	16,483	0.324	0.468	0.000	0.000	1.000
NBusSeg	16,483	1.144	0.469	0.693	1.099	1.609
BIG4	16,483	0.899	0.301	1.000	1.000	1.000
MNC	16,483	0.637	0.481	0.000	1.000	1.000
ShortInterest	16,483	0.048	0.068	0.005	0.028	0.065

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., 2000-2003)

Variables	Non-Pilot Firms		Pilot Firms		Difference
	N	Mean	N	Mean	
AF ($)	3,679	0.95 M	1,872	0.82 M	0.12 M**
LnAF	3,679	13.02	1,872	12.95	0.074**
Size	3,679	6.299	1,872	6.271	0.027
Leverage	3,679	0.482	1,872	0.459	0.023***
BTM	3,679	0.490	1,872	0.489	0.001
ROA	3,679	-0.027	1,872	-0.011	-0.016*
Loss	3,679	0.326	1,872	0.288	0.038***
CA/TA	3,679	0.487	1,872	0.490	-0.003
Quick	3,679	2.521	1,872	2.861	-0.340***
InvRec	3,679	0.244	1,872	0.243	0.0020
SizeGrowth	3,679	0.243	1,872	0.254	-0.010
FYEnd	3,679	0.298	1,872	0.305	-0.007
NBusSeg	3,679	1.105	1,872	1.111	-0.006
BIG4	3,679	0.867	1,872	0.873	-0.006
MNC	3,679	0.595	1,872	0.600	-0.005
ShortInterest	3,679	0.016	1,872	0.018	-0.002

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
2. Pilot	***-0.02***																
3. During	***0.20***	0.00															
4. Post	***0.33***	0.01	***-0.49***														
5. Size	***0.71***	***-0.02***	***0.02***	***0.16***													
6. Leverage	***0.25***	***-0.05***	-0.02	***0.07***	***0.26***												
7. BTM	***-0.04***	***0.02***	***-0.05***	***0.03***	-0.01	***-0.33***											
8. ROA	***0.14***	0.00	***0.06***	***0.02***	***0.33***	***-0.18***	***0.04***										
9. Loss	***-0.15***	-0.01	***-0.08***	-0.01	***-0.35***	***0.11***	0.01	***-0.55***									
10. CA/TA	***-0.20***	0.00	0.01	***-0.02***	***-0.32***	***-0.25***	***-0.03***	***-0.11***	***0.12***								
11. Quick	***-0.29***	***0.03***	***-0.02***	***-0.05***	***-0.49***	***-0.37***	-0.01	***-0.10***	***0.17***	***0.42***							
12. InvRec	***0.06***	0.00	-0.01	-0.01	***0.23***	***0.02***	***0.08***	***0.13***	***-0.16***	***0.42***	***-0.28***						
13. SizeGrowth	***-0.12***	0.00	0.00	***-0.12***	***-0.13***	***-0.12***	***-0.02***	***0.08***	-0.01	***0.04***	***0.20***	***-0.10***					
14. FYEnd	***-0.02***	0.00	***0.03***	0.01	***0.06***	***-0.11***	***0.05***	***0.05***	***-0.06***	***0.15***	***-0.02***	***0.17***	***-0.05***				
15. NBusSeg	***0.38***	0.01	-0.01	***0.06***	***0.35***	***0.12***	***0.02***	***0.10***	***-0.11***	***-0.18***	***-0.20***	***0.12***	***-0.07***	-0.01			
16. BIG4	***0.22***	0.01	***0.05***	***0.03***	***0.18***	***0.05***	***-0.04***	***0.03***	***-0.03***	***-0.06***	***-0.02***	***-0.07***	***-0.02***	0.00	***0.05***		
17. MNC	***0.36***	0.01	-0.01	***0.07***	***0.19***	***-0.04***	***-0.03***	***0.07***	***-0.06***	***0.12***	***-0.04***	***0.17***	***-0.05***	***0.02***	***0.19***	***0.04***	
18. ShortInterest	***0.12***	**0.00**	***0.12***	***0.21***	***0.03***	***0.04***	***-0.05***	***0.04***	***-0.02***	0.01	***-0.03***	***0.00***	***-0.03***	***0.03***	***-0.03***	***0.04***	***-0.04***

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: Univariate Difference-in-Differences Analyses

Variable=LnAF	Non-Pilot Sample	Pilot Sample	Cross-sectional Difference	
Pre-Pilot (2000-2003)	13.02 (N=3,679)	12.95 (N=1,872)	-0.074**(t=-2.40)	
During-Pilot (2005-2007)	14.26 (N=2,842)	14.24 (N=1,459)	-0.026 (t=-0.85)	
Post-Pilot (2008-2013)	14.36(N=4,348)	14.27 (N=2,283)	-0.088*** (-3.54)	
Time-series Difference				Diff-in-Diff
During - Pre	1.243*** (t=47.6)	1.291***(t=36.6)		0.048 (t=1.09)
Post - Pre	1.338***(t=57.6)	1.324***(t=42.5)		-0.014(t=-0.36)
Post – During	0.095***(t=4.03)	0.033(t=1.05)		-0.062(t=-1.57)

This table presents the univariate Diff-in-Diff analyses. Specifically, Column 3 compares the difference between Pilot and Non-Pilot firms in each period. Rows 5-7 compare time-series differences within Pilot and Non-Pilot firms. Column 4 presents the Diff-in-Diffs.

Table 5: The Impact of Short-Selling Threats on Audit Fees: Full-Sample Analyses

	(1)	(2)
	LnAF	LnAF
Pilot×During	0.0476**	0.0465**
	(1.99)	(1.98)
Pilot×Post	0.0192	0.0146
	(0.76)	(0.61)
During	1.0969***	1.0709***
	(65.73)	(65.49)
Post	1.0619***	1.0318***
	(54.94)	(54.43)
Size	0.3234***	0.2993***
	(19.54)	(18.63)
Leverage	0.1000***	0.0959***
	(2.77)	(2.97)
BTM	0.0260***	0.0204***
	(4.20)	(3.64)
ROA	-0.1948***	-0.1250***
	(-5.72)	(-4.07)
Loss		0.0706***
		(6.29)
CA/TA		-0.2364***
		(-4.18)
Quick		0.0001
		(0.03)
InvRec		-0.5033***
		(-5.97)
SizeGrowth		-0.0007
		(-0.09)
FYEnd		-0.0508
		(-0.86)
NBusSeg		0.1859***
		(8.35)
BIG4		0.2148***
		(9.97)
MNC		0.1270***
		(5.95)
ShortInterest		0.3886***
		(5.57)
Firm FE	YES	YES
Constant	10.8950***	10.8093***
	(100.59)	(94.09)
Observations	16,483	16,483
Adjusted R^2	0.915	0.920

This table presents the results of testing H1 (i.e., full sample analyses). Column 1 excludes these control variables that are specific to audit fee regressions and Column 2 includes these control variables. All variables are defined in the Appendix. *t* statistics in parentheses are based on standard errors clustered by firm. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table 6: The Impact of Short-Selling Threats on Audit Fees: Sub-Sample Analyses

	(1) Higher Bankruptcy Risk	(2) Lower Bankruptcy Risk	(3) Less Disciplined Managers	(4) More Disciplined Managers
Pilot×During	0.1363**	0.0206	0.0766**	0.0028
	(2.47)	(0.78)	(2.53)	(0.07)
Pilot×Post	0.0314	0.0182	0.0497	-0.0379
	(0.53)	(0.66)	(1.62)	(-0.95)
During	1.0001***	1.0752***	1.0388***	1.1082***
	(26.12)	(56.12)	(48.75)	(40.06)
Post	0.9340***	1.0314***	0.9963***	1.0776***
	(21.58)	(45.86)	(40.14)	(33.37)
Size	0.2459***	0.3222***	0.2993***	0.3043***
	(9.23)	(15.44)	(14.90)	(9.91)
Leverage	0.0369	0.0940**	0.0940*	0.1158**
	(0.55)	(2.40)	(1.96)	(2.32)
BTM	0.0089	0.0293***	0.0330***	0.0160**
	(1.27)	(3.06)	(3.89)	(2.00)
ROA	-0.0881**	-0.1435***	-0.1865***	-0.0899*
	(-2.16)	(-3.12)	(-4.27)	(-1.91)
Loss	0.0488**	0.0757***	0.0544***	0.0764***
	(2.53)	(5.26)	(3.70)	(4.04)
CA/TA	0.0369	-0.2793***	-0.2223***	-0.2811***
	(0.30)	(-3.85)	(-3.06)	(-2.93)
Quick	-0.0134**	0.0031	-0.0009	0.0058
	(-2.15)	(0.75)	(-0.19)	(1.19)
InvRec	-0.6406***	-0.4664***	-0.4613***	-0.3925***
	(-3.15)	(-4.64)	(-4.29)	(-2.94)
SizeGrowth	-0.0251	0.0043	0.0030	-0.0001
	(-1.36)	(0.42)	(0.28)	(-0.01)
FYEnd	-0.2809**	0.0393	-0.1010	0.0174
	(-2.32)	(0.67)	(-1.25)	(0.17)
NBusSeg	0.1750***	0.1873***	0.2215***	0.1759***
	(3.25)	(7.14)	(7.61)	(5.25)
BIG4	0.2645***	0.2088***	0.1982***	0.2378***
	(4.76)	(8.42)	(7.44)	(5.91)
MNC	0.0768	0.1282***	0.1302***	0.1149***
	(1.41)	(5.43)	(4.36)	(3.78)
ShortInterest	0.2518	0.3980***	0.3317***	0.4120***
	(1.20)	(5.01)	(3.41)	(3.95)
Firm FE	YES	YES	YES	YES
Constant	11.2993***	10.6420***	10.8272***	10.6563***
	(56.46)	(71.46)	(72.47)	(50.11)
Observations	2,738	12,142	9,183	5,921
Adjusted R^2	0.897	0.923	0.930	0.908

This table presents the subsample analyses of testing H2.1 (Columns 1-2) and H2.2 (Columns 3-4). Column 1(2) includes firms with high (low) bankruptcy risk based on expected default frequency (Bharath and Shumway 2008). Column 3(4) includes firms with less (more) disciplined managers based on positive (negative) discretionary accruals (McNichols 2002). All variables are defined in the Appendix. *t* statistics in parentheses are based on standard errors clustered by firm. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table 7: Alternative Explanations

Panel A: SEC Increased Scrutiny of Pilot Firms: Proportion of Firms Receiving the SEC Comment Letters

	Non-Pilot Firms	Pilot Firms	Difference
During-Pilot (2005-2007)	0.291 (N=2,842)	0.278 (N=1,459)	-0.012 (t=-0.85)
Post-Pilot (2008-2013)	0.468 (N=4,348)	0.486 (N=2,283)	0.018 (t=1.39)

This panel compares proportion of firms receiving comment letters from the SEC between Pilot and Non-Pilot firms in During- and Post-Pilot periods. Comment letter data are not available in the Pre-Pilot period.

Panel B: Clients Increase the Demand for Higher Audit Quality: The Direction of Auditor Switches

Upward Switch: From Non-Big-4 to Big-4 Auditors			
	Non-Pilot Firms	Pilot Firms	Difference
Pre-Pilot[24] (2000-2003)	0.690 (N=323) *[0.060 (N=3,679)]*	0.665 (N=167) *[0.059 (N=1,872)]*	-0.025 (t=-0.58) *[-0.001 (t=-0.20)]*
During-Pilot (2005-2007)	0.075(N=161) *[0.004 (N=2,842)]*	0.034 (N=87) *[0.002 (N=1,459)]*	-0.040 (t=-1.26) *[-0.002 (t=-1.14)]*
Post-Pilot (2008-2013)	0.122(N=115) *[0.003 (N=4,348)]*	0.114 (N=61) *[0.003 (N=2,283)]*	-0.007 (t=-0.14) *[-0.000 (t=-0.11)]*

Downward Switch: From Big-4 to Non-Big-4 Auditors			
Pre-Pilot (2000-2003)	0.068 (N=323) *[0.006 (N=3,679)]*	0.072 (N=167) *[0.006 (N=1,872)]*	0.004 (t=0.15) *[0.000 (t=0.19)]*
During-Pilot (2005-2007)	0.472(N=161) *[0.027 (N=2,842)]*	0.586 (N=87) *[0.035 (N=1,459)]*	0.114* (t=1.72) *[0.008 (t=1.51)]*
Post-Pilot (2008-2013)	0.226(N=115) *[0.006 (N=4,348)]*	0.213 (N=61) *[0.006 (N=2,283)]*	-0.013 (t=-0.20) *[-0.000 (t=-0.14)]*

This panel compares the auditor switches between Pilot and Non-Pilot firms in different periods. The upper part focuses on *Upward Switch* while the bottom part focuses on *Downward Switch*. Within each cell, the first line describes conditional statistics (i.e., given switches), and the second line (in the bracket and *in italic*) describes unconditional statistics (i.e., for all observations).

[24] The *Upward Switch* in Pre-pilot period is unusually high because of the bankruptcy of Arthur Andersen (AA). We label AA as a Non-Big-4 here simply because Enron case ruined AA's reputation as a high-quality auditor. However, our inference, which is mainly based on the comparison in During-Pilot period, is not affected if we label AA as a Big-4 (and therefore high-quality auditor).

Panel C: The Role of Auditor Bargaining Power

	(1) Higher Auditor Bargaining Power	(2) Lower Auditor Bargaining Power
Pilot×During	0.0701**	0.0272
	(2.14)	(0.82)
Pilot×Post	0.0523	-0.0174
	(1.53)	(-0.52)
During	1.0277***	1.1097***
	(45.10)	(48.13)
Post	0.9751***	1.0851***
	(37.44)	(40.23)
Controls	YES	YES
Firm FE	YES	YES
Constant	11.0756***	10.5823***
	(67.68)	(67.24)
Observations	7,770	8,412
Adjusted R^2	0.924	0.913

This panel compares subsamples with higher (lower) auditor bargaining power. Column 1(2) includes firms with higher (lower) auditor bargaining power based on their status as city-leaders in their industry. All variables are defined in the Appendix. *t* statistics in parentheses are based on standard errors clustered by firm. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests).

Table 8: Separating Risk Premium and Increased Effort: Regression Analyses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ANNRestate	QTRRestate	GCO	DA	ERC	LnARLag	Dismissal	Resignation	LnAF
Pilot×During	0.3073*	0.1583	0.5625	-0.0024	-3.5716	0.0044	0.1006	-0.4817	0.0475**
	(1.86)	(0.61)	(0.67)	(-0.38)	(-1.43)	(0.36)	(0.57)	(-0.80)	(2.02)
Pilot×Post	0.1311	-0.1938	0.4478	-0.0026	-2.8963	0.0103	-0.0911	0.3219	0.0091
	(0.71)	(-0.74)	(0.65)	(-0.39)	(-1.46)	(0.86)	(-0.48)	(0.44)	(0.38)
Pilot	-0.0510	0.1591	-1.0451*				0.0240	0.2855	
	(-0.43)	(0.72)	(-1.84)				(0.25)	(0.54)	
During	-0.9816***	1.3024***	-0.1755	0.0039	6.4386***	0.0399***	-0.5672***	1.7698***	1.0630***
	(-9.95)	(7.83)	(-0.48)	(1.03)	(4.20)	(4.92)	(-5.06)	(4.76)	(64.16)
Post	-0.9405***	0.7719***	0.5928*	0.0175***	-6.5343***	0.0104	-1.2056***	-0.1243	1.0238***
	(-8.18)	(4.50)	(1.85)	(4.03)	(-4.91)	(1.27)	(-10.40)	(-0.24)	(52.73)
ANNRestate									-0.0412***
									(-2.89)
GCO									0.0761
									(1.38)
DA									0.0893**
									(2.47)
ERC									-0.0002
									(-0.89)
LnARLag									0.0724***
									(3.18)
Resignation									0.0630
									(1.02)
Dismissal									-0.0722***
									(-4.28)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	NO	NO	NO	YES	YES	NO
Firm FE	NO	NO	NO	YES	YES	YES	NO	NO	YES
Constant	-1.0132**	-4.3343***	-3.9683***	0.1825***	11.2105**	4.6222***	-0.5886	-1.9894	10.4791***
	(-2.09)	(-6.16)	(-3.73)	(5.71)	(1.97)	(92.80)	(-1.56)	(-1.50)	(65.44)
Observations	16,386	16,183	12,230	16,483	16,208	16,483	16,338	12,946	16,208
Adjusted R^2				0.293	0.434	0.400			0.920
Pseudo R^2	0.064	0.058	0.435				0.058	0.148	

This table is intended to separate risk premium and audit effort as two channels through which litigation risk affects audit fees. In Columns 1, and 3 through 6 we use five proxies for audit quality to examine whether audit quality increases in the presence of short selling threats. In Column 2 we focus on QTRRestate as a benchmark for ANNRestate in Column 1. In Columns 7 and 8 we use dismissals and resignations as dependent variables. In Column 9 we include these seven proxies as additional control variables for Equation (1). All variables are defined in the Appendix. *t* statistics in parentheses are based on standard errors clustered by firm. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ (two-sided tests). Note that in Columns 1-3 and 7-8, in which the dependent variables are indicator variables, we use Logit model with industry fixed effects rather than firm fixed effects. No inferences change if we use Logit with firm fixed effects; however, we would have a much smaller sample size because only firms with both values of 0 and 1 as dependent variable remain in such regressions.