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PUBLIC AUDIT OVERSIGHT AND REPORTING CREDIBILITY: EVIDENCE FROM THE PCAOB INSPECTION REGIME

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ABSTRACT

This paper examines how audit oversight by a public-sector regulator affects investors' assessments of reporting credibility. We analyze whether the introduction of the Public Company Accounting Oversight Board (PCAOB) and its inspection regime have strengthened capital-market responses to unexpected earnings releases, as theory predicts when reporting credibility increases. To identify the effects, we use a difference-in-differences design that exploits the staggered introduction of the inspection regime, which affects firms at different points in time depending on their fiscal year-ends, auditors, and the timing of PCAOB inspections. We find that capital-market responses to unexpected earnings increase significantly following the introduction of the PCAOB inspection regime. Corroborating these findings, we also find an increase in abnormal volume responses to firms' 10-K filings after the new regime. Overall, our results are consistent with public audit oversight increasing the credibility of financial reporting.

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1. Introduction

As the accounting scandals in the early 2000s illustrated, reliable financial reporting is a cornerstone of trust in the stock market, which in turn plays a key role for investor participation (Guiso et al., 2008). In an effort to restore trust in financial reporting after the scandals, the U.S. Congress passed the *Sarbanes-Oxley Act* (hereafter, "SOX"). One of its core provisions was the creation of the Public Company Accounting Oversight Board (hereafter, the "PCAOB") and the requirement that the PCAOB inspect all audit firms (hereafter, "auditors") of SEC-registered public companies (hereafter, "firms" or "issuers"). The introduction of the PCAOB represents a major regime shift, replacing self-regulation with public oversight.

Even after years of experience with the new regime, widespread skepticism remains that the PCAOB and its inspection regime have changed the credibility of financial reporting and reassured investors.[1] In response to this skepticism, there has been a call for more economic analysis of the PCAOB's activities and of SOX in general (e.g., House Oversight Committee, 2012; Coates and Srinivasan, 2014). While prior studies examine specific aspects of PCAOB inspections, there is little evidence on the overall capital-market effects of the new oversight regime.[2] At the heart of the debate is the broader economic question of whether audit oversight by a public-sector regulator enhances reporting credibility.[3] This paper examines this question.

The objective of an external audit is to provide outside investors with assurance about corporate reporting in light of numerous agency problems between managers and outside

[1] See, for example, Wall Street Journal (7/2/2010) "A Missed Opportunity to Kill Sarbox" and Washington Post (7/11/2010) "Critics question effectiveness of auditing oversight board."

[2] For example, prior studies examine changes in the composition of the audit market, improvements in audit quality, and market reactions and auditor responses to inspection reports. See Abernathy et al. (2013), DeFond and Zhang (2014) for reviews, and our literature discussion in Section 2.

[3] We define reporting credibility as the extent to which investors trust or have confidence in firms' audited financial statements. Following prior work (e.g., Holthausen and Verrecchia, 1988; Hackenbrack and Hogan, 2002; Wilson, 2008; Chen et al., 2014), we operationalize this construct by measuring how strongly investors respond to a given amount of earnings news. Ceteris paribus, the market response should increase if investors trust the numbers more.

investors. As there are also agency problems between managers and auditors, the credibility of an audit depends crucially on the independence of the auditor as well as the thoroughness with which the audit is conducted (e.g., Watts and Zimmerman, 1983). When auditor independence is questionable, an audit is not likely to provide much assurance to investors. Public oversight could mitigate agency problems in auditing and thereby ensure a certain level of audit quality, which in turn should increase the credibility of financial reporting.

Along the lines of this reasoning, SOX replaced self-regulation and peer reviews with public oversight and PCAOB inspections in an attempt to restore investors' trust in the independence and quality of external auditing. However, prior work in regulatory economics suggests it is not obvious that public audit oversight is an improvement over the prior regime—especially considering the potential problems with public-sector regulators, including resource constraints, inefficient bureaucracies, regulatory capture, and political pressures (e.g., Demsetz, 1968; Stigler, 1971; La Porta et al., 2006). Consistent with these concerns about public regulators, Hilzenrath (2010) states that "the [PCAOB] looks a lot like the system it was designed to replace: slow to act, veiled in secrecy and weak—or weak willed." Similarly, Glover et al. (2009) characterize the PCAOB's inspection model as "inefficient and dysfunctional."

Another complication is that much of the audit process is unobservable to outsiders. Because investors observe only the audit opinion, it is difficult for them to assess the effects of public oversight on external audits. However, the new oversight regime produces a number of publicly observable outcomes (e.g., inspection reports), allowing investors to form updated assessments. In addition, investors are able to observe changes in corporate reporting. For example, prior research documents a large increase in restatements after the introduction of SOX and the PCAOB (Hennes et al., 2008). Our tests presume that investors incorporate this information into their assessments of reporting credibility.[4]

In our primary analysis, we assess changes in reporting credibility after the introduction of the PCAOB inspection regime based on changes in short-window stock market reactions to earnings announcements (i.e., earnings response coefficients or ERCs). We focus on ERCs for two reasons. First, ERCs tie directly into reporting credibility. Conceptually, the ERC is a function of the extent to which investors believe that a surprise in reported earnings reflects economic performance and can be linked to a firm's cost of capital (e.g., Holthausen and Verrecchia, 1988; Kormendi and Lipe, 1987; Collins and Kothari, 1989; Easton and Zmijewski, 1989). Consistent with this interpretation, Wilson (2008) and Chen et al. (2014) show that ERCs decline after firms restate their earnings. Teoh and Wong (1993) show that ERCs are positively associated with audit quality.

Second, ERCs allow us to measure changes in reporting credibility at specific points in time (e.g., before and after a PCAOB inspection), which facilitates the identification of the capital-market effects attributable to public audit oversight. There are many concurrent market and regulatory events that could also affect reporting credibility irrespective of the PCAOB. The challenge is to isolate the effects of the PCAOB regime from these other events. Of particular concern are the market responses to the accounting scandals that ultimately gave rise to SOX as well as other SOX provisions that are unrelated to audit oversight. For instance, it is plausible that, after the Enron scandal, investors would have expected firms to provide more assurance about their financial reporting, even in the absence of a regulatory response (e.g., Leuz and

[4] In essence, our analysis is based on the joint hypothesis that (i) audit oversight has effects and (ii) investors have reasonably accurate assessments of changes in audit oversight. Thus, a no-result in our analysis could have several explanations and could occur even if public audit oversight has improved audit quality.

Schrand, 2009). Similarly, regulatory changes for internal controls (as stipulated by SOX Section 404[b]) could have improved reporting credibility independent of audit oversight.

To overcome these challenges, our identification strategy exploits the staggered introduction of the PCAOB inspection regime, which affects firms at different points in time depending on their auditors, fiscal year-ends, and the timing of PCAOB inspections. The PCAOB introduced its inspections in three distinct phases: (i) one-time limited-scope inspections for the U.S. Big-Four audit firms in 2003 (i.e., Deloitte & Touche, Ernst & Young, KPMG, and PricewaterhouseCoopers); (ii) annual full inspections for audit firms with more than 100 issuers beginning in 2004 (hereafter, "large auditors"); (iii) triennial full inspections for audit firms headquartered in the U.S. that issued a report for at least one, but no more than 100, issuers, beginning in 2004 (hereafter, "small auditors"). We analyze all three phases using a difference-in-differences design comparing ERCs before and after the introduction of the new PCAOB inspection regime. As it is difficult to predict exactly when the market would adjust its assessment of credibility, we consider a window of time, beginning with the conclusion of the PCAOB's fieldwork and ending with the release of the inspection report, during which any updating likely takes place. We estimate effects using the beginning and the end of this window as alternative cutoff dates.

Our first set of analyses examines changes in reporting credibility around limited inspections of U.S. Big-Four auditors and full inspections of large auditors. As initial PCAOB inspections occurred at roughly the same time for all large auditors, we use U.S.-exchange-traded, non-U.S. firms with non-U.S., Big-Four (and Grant Thornton) affiliated audit firms as a benchmark. These cross-listed firms are subject to similar market events as well as other aspects of the U.S. regulatory regime, but their non-U.S. auditors were outside the scope of the

PCAOB's initial inspections. In both analyses, we include country-fixed effects as well as country-specific coefficients for unexpected earnings to control for unobserved heterogeneity in market responses to earnings news across countries.

Consistent with public audit oversight increasing investors' perceptions of financial reporting credibility, we find that the ERCs of firms whose auditors were subject to the new PCAOB inspection regime increase significantly compared to the ERCs of the control sample. The effect is statistically and economically significant when we analyze the effect combining all alternative measurement dates. When we separately analyze each inspection event and measurement date, we find that the ERC effect starts to manifest after the PCAOB releases the limited inspection reports and is most pronounced after the PCAOB conducts its full inspections.

Next, we extend this analysis by allowing the market response to unexpected earnings to differ for profit and loss firms. Losses are more transitory and hence loss firms have lower ERCs (Hayn, 1995). Separately estimating treatment effects for profit and loss firms ensures that changes in the fraction of firms reporting profits and losses across time do not drive our results. As expected, we find that the documented credibility effects stem primarily from firms reporting profits. We also confirm that our results hold allowing for nonlinearities in the ERC. We use fractional polynomial regressions to show that the ERC is s-shaped and tilts upwards after the introduction of the PCAOB inspection regime.

To improve identification, we explore the possibility that other concurrent changes in firms' information environments affect our analysis. We show that neither changes in firms' disclosures prior to the earnings announcement, analysts' forecast bias, management earnings guidance, nor changes in the accrual component of reporting earnings can explain our findings. There is also little evidence that other SOX provisions or market responses to the accounting scandals drive our findings. For example, we find that ERCs increase significantly following PCAOB inspections even for firms that were exempt from SOX Section 404[b] compliance, which is inconsistent with internal control provisions in SOX driving our results.

To further disentangle the impact of the PCAOB inspections from the introduction of other SOX provisions, we examine changes in reporting credibility for firms with small auditors. For these auditors, the PCAOB phased in inspections over three years. The staggered implementation of the inspections reduces overlap with the introduction of other SOX provisions and allows us to estimate ERC changes within the sample of triennially-inspected auditors based solely on variation in the timing of the inspection dates. This analysis greatly mitigates concerns about parallel trends and other economic shocks or regulatory changes (e.g., Leuz, 2007; Coates and Srinivasan, 2014; Leuz and Wysocki, 2015). Consistent with the earlier results, we find that ERCs increase significantly over the rollout of the PCAOB inspection regime for triennially-inspected auditors. Again, we find the effects are concentrated in profit firms.

Finally, because our results rely heavily on ERCs, we use abnormal trading volume around issuers' 10-K filings as an alternative proxy for the credibility of firms' financial reporting. We predict that investors will trade more if firms' audited 10-Ks are more credible. Consistent with this prediction, and the ERC results, we find that abnormal volume responses to firms' 10-K filings increase after the introduction of PCAOB inspections.

Overall, our analyses provide evidence of an increase in financial reporting credibility following the introduction of the PCAOB and its inspection regime. Our findings contribute to the existing literature in several ways. First, we provide evidence that public audit oversight can have substantial capital-market benefits by enhancing the credibility of and investor trust in audited financial reports. This finding adds to the literature on the relative merits of private versus public enforcement for regulation (e.g., La Porta et al., 2006; Jackson and Roe, 2009). It also contributes new evidence to the long-standing question of how to motivate and audit the auditor. As noted in DeFond (2010 and 2012), prior studies focus primarily on litigation and reputation as mechanisms to incentivize auditors. Second, our paper lends further empirical support that financial reporting credibility is priced in markets by exploiting a setting in which a regulatory change could affect reporting credibility, but required disclosures remain largely the same. We show that ERCs are quite sensitive to credibility changes, especially for profit firms and when accounting for nonlinearities. Third, our study answers the call by Coates and Srinivasan (2014) for more evidence on the economic impact of SOX. We provide evidence that an integral part of SOX regulation—the introduction of the PCAOB and its inspection regime—is associated with significant capital-market benefits. While such evidence is important given the significant direct and indirect costs of PCAOB inspections,[5] we hasten to add that our paper neither provides evidence of net benefits nor a complete cost-benefit analysis.

We organize the paper as follows. Section 2 provides further background information on the PCAOB inspection regime, discusses related literature and develops our predictions. Section 3 describes the data. Section 4 presents the results, and Section 5 concludes.

2. Institutional Background, Prior Research, and Empirical Predictions

The PCAOB is a quasi-public agency established by SOX and overseen by the SEC, which approves its budget, standards, and appoints its board members. Section 104 of SOX tasks the PCAOB with the responsibility to inspect registered public accounting firms with respect to their audits of public issuers. For large audit firms, i.e., those that issued audit reports for more than 100 issuers during the prior calendar year, the PCAOB conducts annual inspections—all

[5] These include the costs of establishing and operating the PCAOB, which has an annual budget of $227 million (PCAOB, 2014); auditors' compliance costs; and issuers' audit costs, including potentially higher audit fees.

other audit firms (i.e., small auditors) are subject to triennial inspections.[6] The introduction of the PCAOB inspection regime represents a major shift from peer review to public oversight (Lennox and Pittman, 2010; DeFond, 2010).

In June 2003, the PCAOB began limited inspections of the U.S. Big Four audit firms.[7] The PCAOB conducted fieldwork and released inspection reports at approximately the same time for all limited inspections (see Appendix A, Panel A for details). In 2004, the PCAOB conducted full inspections of large auditors and the first round of triennial inspections of small U.S. auditors. The inspections provide an assessment of an auditor's compliance with SOX, the rules and standards of the PCAOB, SEC rules and professional audit standards (PCAOB, 2004a). A full inspection consists of: (1) reviews of selected audits, (2) evaluations of the sufficiency, documentation, and communication of the quality control systems, and (3) other testing of audit procedures as deemed necessary. The PCAOB uses a risk-based approach to select audits (or engagements) and areas within the audits for inspection.

During fieldwork, inspectors might identify potential deficiencies in one or multiple audit engagements. The PCAOB then gives the auditor the opportunity to respond. If the response is not satisfactory, the deficiency is included in the inspection report as a "Part I finding." The inspection report does not reveal which engagements were inspected or which engagement had Part I findings (i.e., the names of the issuers are not publicly revealed). The auditor may later inform the PCAOB that certain audit procedures have changed in response to the inspection and

[6] Technically, the PCAOB inspects small audit firms *at least* once every three years, i.e., some small auditors are inspected more frequently. The distinction between annual and triennial inspections applies to U.S. and non-U.S. auditors. The PCAOB has inspected non-U.S. audit firms since 2005. These inspections are generally carried out in two ways: (i) PCAOB-only inspections, where the PCAOB conducts the inspection on its own in coordination with the home-country regulator; and (ii) inspections conducted jointly with the home-country regulator. The PCAOB continues to be denied access in certain non-U.S. jurisdictions.

[7] Limited inspections involved all components of full inspections, but were scaled down in extent (e.g., the number of individual audit engagements inspected) because at that time the PCAOB was in the process of staffing-up and building-out its inspection regime (PCAOB, 2004b). The U.S. Big Four voluntarily agreed to participate in the limited inspections since the official PCAOB registration process had not yet begun.

the auditor may also re-audit engagements or specific accounts with deficiencies. In addition, the PCAOB evaluates auditors' *firm-wide* quality controls and issues assessments thereof as "Part II findings." If the auditor addresses Part II criticisms to the satisfaction of the PCAOB within a twelve-month remediation period, these findings remain confidential—otherwise the PCAOB publicly releases Part II findings after the expiration of a twelve-month remediation period. In the event of more severe deficiencies or violations, the PCAOB can pursue enforcement actions in conjunction with fines and penalties, as determined by the Board (PCAOB, 2004c).

In sum, the new oversight regime not only enforces the mandated level of audit quality but also provides incentives for auditors, especially those with formal Part I and Part II findings, to improve their (future) audit procedures. Anecdotal evidence around the introduction of the inspection regime is consistent with this characterization.[8] Moreover, DeFond and Lennox (2015) provide evidence that critical inspection reports prompt auditors to remediate their internal control audit procedures and to perform more rigorous evaluations. We analyze whether the new oversight regime translates into greater reporting credibility in capital-markets.

There is a substantial prior literature investigating the new PCAOB inspection regime (see reviews by Abernathy et al., 2013; DeFond and Zhang, 2014). These studies examine the reports themselves (e.g., Gunny and Zhang, 2013) as well as the effects of inspection reports on clients, auditors and investors (e.g., Lennox and Pittman, 2010; Dee et al., 2011; Gramling et al., 2011; Offermanns and Peek, 2011; Abbott et al., 2013; Boone et al., 2014). For example, the aforementioned studies document that clients dismiss small auditors following deficient inspection reports, that small auditors with deficient inspection reports are more likely to issue going-concern opinions, and that investors react to inspection reports. Moreover, DeFond and

[8] See, e.g., Cassell (2003), O'Kelley (2004), Michaels (2004), and Johnson (2004).

Lennox (2011) find that the PCAOB regime led to an exodus of smaller and lower-quality auditors from the public company audit market.

The evidence is less consistent for the effects of inspections on large auditors. For instance, Lennox and Pittman (2010) do not find that unfavorable reports lead to auditor-client realignment and conclude that PCAOB inspection reports do not convey useful information to clients about audit quality. Boone et al. (2014) in turn show that PCAOB sanctions against Deloitte and Touche in 2007 led to negative client responses.

Other studies directly investigate the effect of PCAOB inspections on proxies for audit quality, both in and outside the U.S. (e.g., Lamoreaux, 2013; Fung et al., 2014; Krishnan et al., 2014; DeFond and Lennox, 2015). These studies are informative about the mechanisms through which PCOAB inspections could increase investor confidence. However, many proxies for audit quality are slow moving and computed over several years (e.g., discretionary accruals), which makes it difficult to distinguish the effects of the PCAOB inspections from other concurrent changes (e.g., other SOX provisions). For this reason, most studies exploit jurisdictional variation with respect to foreign inspections. For instance, Lamoreaux (2013) finds that auditors in jurisdictions allowing PCAOB inspections are more likely to report going concern opinions and material weaknesses relative to auditors in jurisdictions barring PCAOB inspections. DeFond and Lennox (2015) provide evidence for U.S. auditors that PCAOB inspections improve audit quality exploiting variation in internal control audit deficiencies reported by PCAOB.

In addition, there is a large literature evaluating the economic consequences of SOX (see Coates and Srinivasan, 2014; Leuz and Wysocki, 2015). Many of these studies assess the effects of SOX as a whole. For example, a series of papers analyzes market reactions to legislative events related to the passage and implementation of SOX to infer costs and benefits of the legislation (e.g., Akhigbe and Martin, 2006; Jain and Rezaee, 2006; Zhang, 2007; Li et al., 2008). Aside from being quite mixed, this evidence does not speak to specific SOX provisions, such as the introduction of public audit oversight.

Our study differs from prior studies and aims to make two contributions. First, we aim to provide regime-level evidence on the capital-market consequences of a core provision of SOX—the introduction of the PCAOB and its inspection regime. Second, we aim to contribute evidence on the question of whether *public* audit oversight enhances reporting credibility and hence on the classic tradeoff between expertise and independence when it comes to self-regulation and government regulation (for this debate see also DeFond, 2012; DeFond and Zhang, 2014).

External audits are intended to provide reasonable assurance that firms have faithfully followed GAAP and that financial statements are free of material misstatements. As such, auditing should enhance reporting credibility. However, the accounting scandals in the early 2000s were a major shock to the credibility of U.S. corporate reporting as well as the assurance provided by external auditors. The scandals highlighted the issue of auditor independence in a setting where firms select their auditor and auditors have incentives appease their clients (e.g., Economist, 2014). SOX and the introduction of public audit oversight were meant to mitigate this issue and restore investor trust in financial reporting and auditing. Given this regulatory motivation, our analysis focuses on investors' assessments of financial reporting credibility.

We define reporting credibility as the extent to which investors trust or have confidence in firms' audited financial statements. We use the strength of investors' responses to earnings news as our primary measure of reporting credibility. The underlying idea is that, ceteris paribus, investors should respond more strongly to a given earnings surprise if they have more confidence that reported earnings truthfully reflect economic performance. Considering that public oversight of external audits does not (necessarily) come with new disclosures, as do the other SOX provisions, public audit oversight is more likely to operate through a reporting credibility channel. Reporting credibility could affect market responses to earnings news in two ways. First, it could increase the signal-to-noise ratio of the earnings surprise. Second, it could reduce the risk premium that investors apply and hence increase the capitalization rate of the earnings surprise. The alternative hypothesis is that the PCAOB and its inspection regime are not effective in increasing investor confidence and reporting credibility, in which case we expect to see no changes in market responses to earnings news.

We acknowledge that stricter audit oversight could have effects beyond reporting credibility and could, for instance, indirectly change corporate disclosure. Facing stricter oversight, auditors could force issuers to provide additional explanations about their accounting choices. If such additional disclosures are provided in the earnings announcement, they are presumably captured by the market response to the earnings surprise. If they occur prior to the fiscal year-end (Ball et al., 2012), they should be reflected in investors' earnings expectations and hence be accounted for in the calculation of earnings news. If such additional disclosures come later and are included only in the 10-K, then they do not affect our main analysis that focuses on the earnings announcement. Thus, our analysis does not capture all reporting effects.

Similarly, it is possible that stricter audit oversight changes the bias in reported earnings. For instance, auditors could be more forceful in reining in earnings management because they are subject to increased oversight. If firms sometimes engage in over-reporting and at other times in under-reporting, and auditors equally reduce both forms of bias due to stricter oversight, then the primary effect is an increase in market responses and hence reporting credibility. However, it is conceivable that the effects are not symmetric, making the effects of audit oversight on market responses at the earnings announcement harder to sign. For instance, less under-reporting of losses (profits) should lead to stronger (weaker) market reactions, all else equal. There could also be differential effects on the composition of transitory and permanent components in earnings. For example, with stricter oversight, auditors could insist more forcefully on the recognition of impairments, which are more transitory in nature. The inclusion of such impairments in earnings likely reduces the ERC (Hayn, 1995). For these reasons, we control for losses and nonlinearities in the ERC relation so that we estimate changes in reporting credibility for otherwise comparable earnings surprises, and also analyze changes in firms' earnings properties (e.g., accruals) to gauge potentially confounding effects (see Section 4.1).

3. Research Design, Sample Selection, and Descriptive Statistics

3.1 Difference-in-Differences Design, Control Firms and Timing of Regime Change

Our identification strategy exploits the staggered introduction of the PCAOB inspection regime, which affects issuers at different points in time depending on their fiscal year-ends, their auditors, and the timing of PCAOB inspections. We examine the effect of public audit oversight on reporting credibility for each of the three distinct phases over which the PCAOB inspection regime was introduced (i.e., limited, full, and triennial inspections). For each phase, we use difference-in-differences estimation to identify the credibility effects of the regime change.

Because the limited and initial full inspections were clustered in time (see Appendix A, Panel A), our tests rely on non-U.S. firms that are cross-listed on U.S. exchanges as a control group.[9] This control group has several desirable features. First, control firms are audited by non-U.S. Big-Four and Grant Thornton affiliates that are not subject to PCAOB inspections in 2003 or 2004. Second, the SEC required cross-listed non-U.S. issuers to comply with several other

[9] Although the PCAOB performed full inspections with less clustering than limited inspections, there is insufficient intertemporal variation to make powerful within-Big-Four/Tier-Two comparisons.

provisions of SOX at the same time as domestic issuers (with one exception discussed later). Third, due to their U.S. listing, these issuers likely face similar macroeconomic conditions, reporting incentives, and information environments as U.S. issuers (Lang et al., 2003). This control group also has limitations. First, cross-listed issuers could be subject to similar treatments in their home countries if they implement audit oversight reforms similar to those prescribed by SOX.[10] Furthermore, it is possible that non-U.S. auditors change their audit procedures because their U.S. affiliates were inspected by the PCAOB. Such regulatory and network spillover effects should make it more difficult to detect the impact of new U.S. audit oversight regime. Finally, the non-U.S. control group is relatively small compared to the treatment sample, which reduces the power of our tests. In addition, one could be concerned about a violation of the parallel-trends assumption, i.e., the possibility that our treatment and control firms differ systematically and would not have had similar ERC trends in the absence of the inspection regime. In the Internet Appendix, we examine past trends in ERCs for our treatment and control firms and find no evidence that calls into question the validity of the parallel-trends assumption.

In the triennial inspection analyses, we can use firms with small auditors that the PCAOB has not yet inspected as a benchmark because the PCAOB phased these inspections in over three years. That is, we can identify the effects of the new oversight regime based solely on differences in the timing of the inspections.[11] The staggered introduction and the within-group design greatly mitigate concerns about unrelated economic shocks, concurrent regulatory changes (e.g., other SOX provisions) and the parallel-trends assumption. The primary drawbacks of this analysis are (i) the relatively small sample of issuers with triennially-inspected, small auditors (and also

[10] For example, in 2004, the U.K. established new audit oversight with the Financial Reporting Council.
[11] It is possible that the PCAOB initially inspected auditors with a higher risk of having deficient audits. However, such selection does not pose an identification problem as long as we estimate the effects for a full three-year cycle *and* the timing of inspections is not systematically associated with other economic shocks *occurring on firms' earnings announcements*. The latter is unlikely given the large number of firms with different announcement days.

analyst coverage) and (ii) the possibility that, in the later inspection years, auditors make anticipatory adjustments in advance of the PCAOB inspections in response to the results of prior inspections of other auditors (small or large).[12] These drawbacks decrease the likelihood that we find a significant effect for these analyses.

Another important research-design challenge is determining when to measure changes in reporting credibility. The introduction of the new oversight regime was known to the market, but the timing of the individual inspections (i.e., when auditors were treated by the new regime) may not have been known until the PCAOB released inspections reports. Moreover, it likely takes some time for auditors to respond to the new regime (e.g., to improve audit procedures in response to inspection findings). Thus, it is difficult to determine a priori when the market adjusts its assessment of reporting credibility (assuming it does) and the adjustment could be more gradually than sharp. For this reason, we define a measurement window during which the auditor's treatment and the market's updating likely takes place. The window starts with the earliest possible date, the completion of the PCAOB's inspection fieldwork for a particular auditor, and ends with the public release of the inspection report. We estimate treatment effects using each of these alternative cutoff dates.

Using the fieldwork end date as the cutoff, we define an issuer as treated if its fiscal year-end occurs in, or after, the month inspection fieldwork ends for its auditor.[13] By that time, the auditor can use information gathered from its PCAOB inspection to improve audits that have not advanced out of the planning stage. If the inspection leads to improvements in audit quality

[12] This concern about adjustments ahead of PCAOB inspections also arises in our large-auditor analysis, though to a lesser extent. Anecdotally, the large number of Part I findings from the early inspection reports provides little indication of anticipatory improvements on the part of the auditors. Moreover, even if auditors did make anticipatory changes it is unclear whether market participants would find voluntary changes credible in the post-Enron period.
[13] For the Big Four, the fieldwork stage typically lasts between five to seven months. For small auditors, the inspections are much shorter and hence we add 30 days to the completion of the fieldwork in defining the cutoff date. See Appendix A and Figure 1 for more details and an illustration.

beyond the inspected engagements, and investors learn about these improvements (or expect them to have taken place), reporting credibility should increase shortly after the completion of the PCAOB's fieldwork.[14] Note, however, that many fiscal year-ends occur well after the completion of fieldwork and that there is an additional lag from a firm's fiscal year-end until its earnings announcement. Thus, there is generally a considerable time lag between the completion of the fieldwork and the time we measure the ERC effect (Table 2 provides statistics).

That said, we acknowledge that it could take considerable time for auditors to adjust their audit procedures and for the market to become aware of these changes. We therefore use the release date of the PCAOB inspection report as an alternative cutoff date. The release of the inspection report makes it public information that the inspection has taken place. It is the latest date by which investors learn an auditor has been subject to the new inspection regime. Using the report release as the cutoff, we define a firm as treated if it announces its earnings after the date on which the PCAOB posts the inspection report for the firm's auditor on its website. Importantly, the reports do not reveal the identity of the inspected engagements, but they should provide investors with information about the auditor's quality, including potential future changes in audit procedures arising from the inspection process.[15] Our analysis therefore tests for auditor-wide improvements in reporting credibility and does not focus on specific clients.

[14] For the Big Four and other large auditors, market participants could learn about the timing of PCAOB fieldwork through a variety of public sources such as the news media and public releases on the PCAOB's webpage. For example, PCAOB Chairman William McDonough testified to the Senate Banking Committee on September 23rd "For 2003, limited inspection procedures are already being conducted on the four largest accounting firms, which have agreed to cooperate with the Board prior to their registration. Those inspections are already in process. In 2004, regular inspections will begin for all accounting firms. Inspections of those firms with less than 100 issuer audit clients will be phased in over a three-year period" (PCAOB, 2003).

[15] As all large auditors received multiple Part I findings during their initial inspections, there is no clear way to examine variation across auditors based on the reports. Also, there may have been Part II findings in the initial PCAOB inspections, but none of them were publicly released until 2010 (for Deloitte & Touche), which is after the sample period of our regime change analysis.

In addition, the inspection reports could provide information about the implementation of the new inspection regime itself and investors could update their assessment of public audit oversight (and its effects) relative to their initial expectations. This adjustment could go in either direction. For instance, suppose investors have favorably revised their beliefs of reporting credibility soon after the creation of the PCAOB, then it is conceivable that the inspection reports reveal information suggesting that the oversight regime is less strict than initially expected. In this case, the ERC should decline relative to investors' interim assessment of reporting credibility (even if the new regime still has a positive effect on ERCs overall). For this reason, we do *not* compute incremental changes in the ERC from the end of fieldwork to the report release. We estimate *long-run changes* in (short-window) ERCs *relative* to the pre-inspection-regime period and hence do not benchmark against the market's interim expectation.[16] We also do not estimate market responses when the PCAOB releases its inspection reports but instead measure responses to earnings announcements for two years after the inspection report release.

3.2 Measuring Financial Reporting Credibility

We assess changes in reporting credibility based on changes in the short-term stock market responses to the announcements of unexpected earnings. Prior research establishes both a theoretical and an empirical basis for using ERCs as a proxy for investors' assessments of reporting credibility (see Kothari, 2001; Dechow et al., 2010, for reviews). Holthausen and Verrecchia (1988) build a model with two sequential disclosures (e.g., an analyst forecast and a subsequent earnings announcement) and derive comparative statistics for the price reactions to the two disclosures. They show that, even under very general assumptions about the intertemporal correlation between the two information releases, an increase in the information

[16] To even further avoid contamination from the interim period, we also examine an alternative design that explicitly excludes both pre-period fiscal year-ends that occur during PCAOB fieldwork and pre-period observations that occur before the release of the inspection report. See illustration in Figure 1 and also discussion in Section 4.

quality of the second disclosure (i.e., earnings) has an unambiguous and non-negative effect on the variance of the price reaction (i.e., the ERC). Thus, the model predicts that ERCs increase if public audit oversight increases reporting credibility because higher reporting credibility increases the signal-to-noise ratio of earnings surprises and hence is tantamount to an increase in the information quality of the second disclosure.

Based on this or similar reasoning, many empirical studies use ERCs in audit-specific settings to assess the capital-market effects of audit quality and as a proxy for reporting credibility (e.g., Teoh and Wong, 1993; Hackenbrack and Hogan, 2002; Francis and Ke, 2006; Wilson, 2008; Marshall et al., 2013; Chen et al., 2014). In addition, higher reporting credibility could reduce the cost of capital and hence increase the capitalization rate of the earnings surprise.

Empirically, ERCs are well suited for assessing the impact of the PCAOB inspection regime on reporting credibility. ERCs are less anticipatory in nature than other capital-market outcomes such as returns or the (implied) cost of capital: the market is not expected to change its response to unexpected earnings until after the new regime is in place and auditors have been treated. This feature of ERCs allows us to exploit the staggered rollout of the PCAOB inspection regime in our research design.

However, ERCs also require assumptions and have disadvantages. First, ERCs reflect features of firms' reported earnings beyond reporting credibility (e.g., the extent to which an earnings surprise is persistent or transitory) as well as other firm characteristics (e.g., Collins and Kothari 1989; Hayn 1995). Second, ERCs require a measure of expected earnings in order to determine earnings news. We measure expected earnings using analyst forecasts, which are known to exhibit biases and may not reflect investors' expectations. As a result, ERC estimates can be noisy. As long as the regime change does not also change the bias in analyst forecasts, this issue is mitigated in our setting by the difference-in-differences design. Requiring analyst forecasts also limits the size of our sample, especially for firms with small auditors. Third, ERCs are not directly observable for a given earnings announcement but need to be estimated from a sample of announcements. This requirement likely introduces noise and reduces the power of our analyses. We therefore employ three separate approaches to dealing with outliers in the ERC estimation as well as three distinct difference-in-differences designs spanning different periods and control groups to mitigate these drawbacks. We also consider abnormal volume reactions around the release of firms' 10-Ks as an alternative measure of reporting credibility. Like ERCs, this measure should reflect information quality and reporting credibility.

3.3 Sample Selection and Composition

We obtain: (a) accounting, auditor, and market data from Compustat, (b) additional auditor data from Audit Analytics, (c) analyst forecasts and accounting data from I/B/E/S (d) market data from CRSP, and (e) fieldwork and inspection dates from the PCAOB's website. We merge these datasets into a firm-year panel.

For the limited and full inspection analyses of annually-inspected auditors, we use observations from *two* fiscal-years before *and* after the respective cutoff date. Thus, we include observations over roughly a four-year window surrounding the treatment. For the limited inspections using the fieldwork cutoff date, the sample includes firms with fiscal year-end dates between December 2001 and November 2005. Using the inspection report cutoff date, the sample includes firms with fiscal year-end dates between June 2002 and May 2006. We include the full sample of cross-listed control firms because, at that time, there were no formal cooperative agreements between the PCAOB and home-country regulators of non-U.S. firms to conduct similar inspections in non-U.S. jurisdictions. For the first full inspections for Big-Four and Tier-Two auditors using the fieldwork cutoff date, the sample includes firms with year-end dates between June 2002 and December 2006. Using the report release date, the sample includes firms with year-end dates between July 2003 and November 2007. For the full-inspection control sample, we exclude auditors (and their client firms) from countries that have an inspection agreement with the PCAOB during or before the analysis window.[17] We *include* those countries identified by the PCAOB as being unavailable for inspections.[18]

Panel A of Table 1 provides details on the sample composition, for both the treatment and control groups, by auditor, inspection type, and treatment dates for the limited and full inspection analyses. For the limited inspections, the number of treatment firms is similar across auditors. For the full inspections, the Big Four again contribute a similar number of treatment firms, while the Tier-Two auditors have far fewer firms. The control sample of cross-listed firms is much smaller. Combining limited and full inspections, our treatment sample includes 4,289 unique domestically-audited firms over 37,001 firm-years and the control sample includes 579 unique non-U.S. firms over 3,765 firm-years.[19]

Table 1, Panel B provides a breakdown of the treatment and control samples by the location of the auditor that signs the opinion letter. We consolidate the 19 countries with three or fewer unique firms into the category "Other." By design, all firms in the treatment sample have U.S.-domiciled auditors. Canadian and British auditors audit the most control firms.

[17] The PCAOB commenced full inspections on some non-U.S. Big-Four affiliates in 2005. KPMG LLP Canada was the first inspected, with fieldwork beginning in April 2005 and an inspection report release in February 2007. We exclude Canadian firms with Big-Four auditors from the control group in the full-inspection tests. Australia signed an agreement with the PCAOB on July 16, 2007. We exclude Australian firms from our control sample where there is overlap with the timing of the full inspection report release. We also exclude firms from South Korea, which signed a confidential undated agreement with the PCOAB, from the full inspection control group. See http://pcaobus.org/International/Pages/RegulatoryCooperation.aspx for details.

[18] http://pcaobus.org/International/Inspections/Pages/IssuerClientsWithoutAccess.aspx (Accessed January 2015)

[19] Non-U.S. Grant Thornton affiliates are included in the full inspection control sample. Other Tier-Two auditors are not included because Audit Analytics does not identify foreign affiliates of these auditors. We do not include Grant Thornton in the control group for the limited inspections to facilitate a fully within-Big Four comparison.

Table 1, Panel C provides details on the number of newly-inspected triennial auditors and their clients for each of the two alternative cutoff dates. As expected, there is significant variation in inspection timing because of the triennial cycle. To avoid overlap with the 2008 financial crisis, our analysis excludes fiscal years ending beyond Q2 of 2008. The sample size is 1,229 and 918 firm-year observations, respectively.

3.4 Descriptive Statistics

Panels A and B of Table 2 present descriptive statistics for issuers with annually- and triennially-inspected auditors, respectively. Here, we discuss only the control variables that enter our primary analyses. The remaining variables are discussed along with the corresponding analyses. Across both panels, the median firm has a cumulative abnormal return (*CAR*) and unexpected earnings (*UE*) of (nearly) zero. In Panel A, the median firm has: positive earnings (*LOSS* equals zero), a market cap of about $1.1 billion (*SIZE*), a market value 2.3 times as large as its book value (*Market-to-Book*), liabilities 1.2 times its total equity (*Leverage*), positively auto-correlated earnings (*Persistence*), and a *Beta* coefficient near one. We also count the number of days from the cutoff date (i.e., the end of fieldwork or report release) for the auditor's treatment by the PCAOB regime and the firm's subsequent earnings announcement at which the *first* post-period ERC is measured (*Timing: Treatment to First EA (in days)*). The variable indicates that our design allows for a substantial time lag between the cutoff date and the next earnings announcement, giving time to auditors to adjust audit procedures and to the market to price the regime change. The descriptive statistics for the control variables for the triennially-inspected firms in Panel B are generally similar. As expected, these firms are smaller, more highly levered, and have returns that are less correlated with the market.

4. Empirical Results

4.1 Main Analysis

Our first set of analyses examines changes in reporting credibility for firms whose auditors were subject to the initial limited inspections in 2003 and initial full inspections in 2004. We estimate the following equation (suppressing time and firm subscripts):

$$\begin{aligned} CAR = {} & \alpha + \beta_1 UE + \beta_2 Post + \beta_3 Treated + \lambda_n Controls + \gamma_n FixedEffects + \\ & \beta_4 UE \times Post + \beta_5 UE \times Treated + \beta_n UE \times Controls + \beta_n UE \times FixedEffects + \\ & \beta_6 Post \times Treated + \beta_7 UE \times Post \times Treated + \varepsilon \end{aligned} \quad (1)$$

CAR is the 3-day (*t-1*, *t=0*, and *t+1*) CRSP market-adjusted return centered on the earnings announcement date.[20] *UE* is the difference between the actual, annual EPS and the median-forecasted, annual EPS, both from I/B/E/S. *Treated* is an indicator variable that equals one when a firm's auditor is a U.S. Big-Four or Tier-Two auditor, and zero otherwise. *Post* is an indicator variable coded for an auditor's global network; it equals one for firm-years after the cutoff date defining the treatment of the U.S. affiliate of a firm's auditor through the PCAOB inspection process, and zero otherwise. We use two alternative cutoff dates (see illustrations in Figure 1 and details in Appendix A). For analyses using the fieldwork date, *Post* equals one if a firm's fiscal year-end is in the same month as the final month of fieldwork or later.[21] For analyses using the inspection report date, *Post* equals one if a firm's fourth-quarter earnings announcement falls on or after the release date of the inspection report. Our primary coefficient of interest is β_7. It measures the incremental change in the ERC for firms whose auditors have been treated by the

[20] As a sensitivity test, we calculate abnormal returns using a Fama and French (1993) three-factor model with coefficients determined by daily returns over the fiscal year. All factor returns were gathered from Ken French's data library. The results are similar in economic magnitude and significance using this abnormal return measure.

[21] For analyses using the fieldwork date, we base the post period on the fiscal year-end, rather than the earnings announcement date, so that any adjustments made by the auditor in response to the fieldwork could feasibly be incorporated into upcoming audits.

PCAOB inspection regime. A positive coefficient indicates an increase in the response to earnings news, which we interpret as an increase in reporting credibility.

We include controls for a variety of firm characteristics. We include *Loss*, an indicator variable that equals one if a firm experiences an accounting loss (and zero otherwise), and *UE*× *Loss*, the interaction of *UE* and *Loss*. Because losses are expected to be less persistent than profits, the earnings response to negative earnings is likely to be lower than for positive earnings (Hayn, 1995). We also include several additional control variables shown by prior literature to affect the magnitude of a firm's ERC, including *Size*, *Market-to-Book*, *Leverage*, *Persistence*, *Beta*, and the interaction of each of these variables with *UE* (e.g., Collins and Kothari, 1989; Easton and Zmijewski, 1989; Dhaliwal et al., 1991). We include fixed effects for the global auditor network, the auditor's country of domicile, the year-quarter of the firm's fiscal year end, and interactions of these fixed effects with *UE* as indicated in the tables. The inclusion of auditor-network and the country fixed effects controls for cross-sectional differences in the ERC coefficient across auditor networks and countries; the year-quarter fixed effects flexibly account for changes in ERCs over time (e.g., due to changes in market sentiment). We truncate all continuous variables, with the exception of *UE*, at the 1% and 99% level. Unexpected earnings are known to exhibit large outliers, especially in the left tail (e.g., Collins and Kothari, 1989; Teoh and Wong, 1993). Hence, we truncate *UE* at the 2.5% and 97.5% level.[22] In all tests, we cluster standard errors by firm. We provide definitions of each variable in the Appendix B.

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[22] Prior studies use a variety of approaches to deal with extreme *UE* observations, including deleting observations for which *UE* exceeds a specified percentage of price (e.g., 100%) and deleting observations with large standardized residuals (e.g., Collins and Kothari, 1989; Teoh and Wong, 1993; Francis and Ke, 2006; Chen et al., 2014). We analyze the empirical distribution of *CAR* relative to *UE* in our sample and find a large number of extremely large outliers. In the Internet Appendix, we provide scatter plots for untrimmed and truncated data across a variety of truncation levels. Inferences are similar, but statistically weaker, if we instead truncate *UE* at 1% and 99%. The precision and the magnitude of the baseline ERC increase and our results become stronger, if we truncate *UE* even further (e.g. at 5% and 95% or 10% and 90%).

In Table 3, Panel A, we present results from a difference-in-differences analysis using a pooled sample that combines limited and full inspections and fieldwork-end and inspection-report-release dates (hereafter, the "combined" sample)—which effectively provides the average change in ERC across each of the four alternative measurement windows. To facilitate a comparison with prior ERC studies, Column (1) provides ordinary least squares (OLS) regression results for a baseline ERC model including all control variables, but excluding the audit oversight regime indicators. *UE* is positive and significant at the 1% level. The magnitude of the estimated ERC coefficient (1.377) is consistent with prior literature (e.g., Kothari, 2001). Firms with accounting losses (*UE×Loss*) and higher leverage (*UE×Leverage*) have significantly smaller ERCs (e.g., Hayn, 1995; Collins and Kothari, 1989, respectively). High beta (*UE×Beta*) and high growth firms (*UE×M2B*) have larger ERCs (e.g., Easton and Zmijewski, 1989). The interactions of *UE* with the other controls are statistically insignificant.

Columns (2)-(5) present the primary tests of our main empirical prediction using three alternative specifications to address well-known problems with noise in ERC estimates, e.g., due to *UE* outliers (e.g., Beaver et al., 1980; Kothari, 2001). Column (2) provides estimates from a standard OLS regression, where UE is truncated at 2.5% and 97.5%. In Column (3), we estimate a robust regression, which places less weight on estimates with large absolute residuals.[23] We rely on the robust regression as our primary specification because we view it as an effective and standardized (non-discretionary) way to reduce the influence of outliers. Column (4) presents results based on an OLS regression using the percentile rank of *UE* (where we truncate *UE* only

[23] More specifically, we perform robust regressions using Stata's "rreg" procedure. This approach eliminates any observations with a Cook's distance greater than one and weights the remaining observations based on the absolute residuals using a combination of Huber and bi-weighting. We indicate the number of observations lost through this procedure for each robust regression in corresponding table row ("zero-weighted observations"). For all robust regressions, we calculate robust, firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression. Prior research has used similar approaches to deal with extreme values of UE (e.g., Francis and Ke, 2006; Chen et al., 2014). We also consider alternative robust regression estimation methods, including several MM-estimators, and reach similar conclusions.

at 1% and 99%). Column (5) repeats the analysis in Column (3) including year-quarter fixed effects. For brevity, we suppress the coefficients on the main effects and (non-interacted) control variables in all columns.

In Column (2), the coefficient of interest, *UE×Post×Treated*, is positive and statistically significant at the 10% level (two-sided). Consistent with the concern about outliers in *UE* and resulting noise in ERC estimates, the results are statistically stronger in Columns (3) and (4). The coefficient on *UE×Post×Treated* is positive and has a similar magnitude as in Column (2), but significant at the 1% and 5% levels, respectively. Results in Column (5), controlling for year-quarter fixed effects, are very similar to Column (3).[24] In all four cases, the effects are economically substantial. To gauge the magnitude of the coefficients, following Kothari (2001), we use a 10% as a benchmark cost of equity capital and assume that the earnings surprise persists in perpetuity. Using this approach, the estimated coefficient in Column (3) implies a decline in the cost of equity capital of approximately 63 basis points.[25]

Table 3, Panel B, presents the robust regression results of Eq. (1) separately using each of the four alternative measurement dates: limited inspection fieldwork (Column 1), limited inspection report release (Column 2), full inspection fieldwork (Column 3), and full inspection report release (Column 4). Because there is significant overlap in the measurement windows, the estimated effects for each of the four measurement dates cannot be interpreted cumulatively (or

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[24] Although our regressions contain a relatively large number of interaction terms, variation inflation is not a concern if it stems from products of the same variable with other variables. Besides, the variance inflation factor for the variable of interest, *UE×Post×Treated*, is only near 10 in all specifications. We are able to reject the null hypothesis for this coefficient and have reasonably tight confidence intervals. Thus, multi-collinearity is not a significant concern for our inferences (see also O'Brien, 2007).

[25] The small magnitude of ERC coefficient estimates, relative to their expected magnitudes, is a ubiquitous feature of prior research (e.g., Kothari, 2001). To assess the economic magnitude of our estimated treatment effects, we assume that the general downward bias in the baseline ERC stays roughly constant through time and base our assessments on the estimated change relative to a reasonable theoretical benchmark ERC. For a cost of equity capital of 10%, the benchmark ERC is 11 (1/.10+1) assuming a permanent shock to earnings. The 63-basis point decline in cost of capital, for example, is calculated as 0.1000 – 0.0937, where .0937 is the cost of capital implied by an increase in the ERC of 0.677 (1/r+1=11 + 0.677). If we were to assume that the earnings surprise is less permanent, the benchmark ERC would decrease, suggesting a larger impact on the cost of capital.

incrementally); they simply provide alternative estimates for the effect of the regime change. In Panel B, Row (1), we present results based on our main design described in Section 3. In Panel B, Row (2), we present results from an alternative design excluding pre-period fiscal year-ends that occur during PCAOB fieldwork and pre-period observations until the release of the inspection report. The alternative design reduces potential contamination effects from overlap in the pre- and post-period for the alternative cutoff dates (e.g., the fact that in the main design the pre-period for the report release overlaps with the post-period for the fieldwork). Figure 1 provides an illustration of the main and the alternative design with "dropped observations." We use the design without dropped observations as our main specification, noting that an early market reaction to ongoing fieldwork and the described overlap bias against us finding results.

In Column (1), we repeat the results from the combined specification reported in Column (5) of Panel A for comparison. In Column (2), *UE×Post×Treated* is positive but statistically insignificant. In Column (3), the treatment effect is significant at the 10% level. In Columns (4) and (5), *UE×Post×Treated* is positive and significant at the 5% level (at least) and ranges in magnitude between 1.149 and 1.600. Results for the dropped observation analysis are similar, with the exception of Column (5), for which the estimated ERC effect is much larger, which is consistent with the described overlap biasing against our results. Overall, these results indicate that the ERC effect starts to manifest after the PCAOB releases the limited inspection reports and is most pronounced after the PCAOB conducts its full inspections.

In Table 3 Panel C, we extend the analysis accounting for two important properties of ERCs—limited market responses to losses and extreme values of UE—allowing for a more general treatment response. As noted before, profitable firms are expected to exhibit a larger and more consistent ERC than loss firms due to the transitory nature of losses (Hayn, 1995).[26] Consistent with this reasoning, the baseline model in Table 3, Panel A, Column (1) shows that the ERC for negative earnings (*UE*×*Loss*) is near zero (0.334). While the inclusion of the *Loss* indicator and its interaction with *UE* already account for this property, it is possible that the proportion of profit and loss firms changes around the introduction of the new regime, which in turn could bias our estimated treatment effects. We therefore estimate the effects of the regime change separately for profit and loss firms. Given the low ERCs of losses, the credibility effect is expected to be concentrated in profitable firms.

Similarly, Freeman and Tse (1992) demonstrate that, because extreme realizations of unexpected earnings are likely to be less persistent, the relation between unexpected earnings and returns can be nonlinear (i.e., return responses decrease as the absolute magnitude of unexpected earnings increases). For the same reason, any credibility effect for extreme values of *UE* is likely to be smaller. Moreover, changes in the fraction of extreme and non-extreme values of *UE* across time could bias the estimated treatment effect. Aside from macroeconomic shocks, such changes could arise in our setting if auditors are more likely to force firms to recognize impairments in the post-inspection regime. While the latter could be the result of stricter audit oversight, it would contaminate the ERC analysis, which aims to estimate changes in reporting credibility from the pre- to the post-period for otherwise comparable earnings surprises.

Following prior research (e.g., Chen et al., 2014), we model the nonlinear relation using the interaction between *UE* and the absolute value of *UE* and include this variable along with losses interacted with *UE* in Column (1) of Table 3 Panel C. Consistent with prior research, we find that these interactions are significantly negative. Moreover, the baseline ERC considerably

[26] Note that the concept of an accounting loss is distinct from a negative UE—firms with negative UEs can still have positive earnings, and vice versa.

increases, indicating that the low baseline ERC is in part attributable to not accounting for naturally less persistent surprises. We consider the effect of loss observations and nonlinearity on the estimated change in ERCs in Columns (2)-(4) of Panel C—first individually and then in conjunction. Across all three columns, the estimated treatment effect is stronger in each of these three specifications, indicating that ERCs are nonlinear and that the credibility effects are concentrated in profitable firms, as expected. A likelihood ratio test comparing the difference in the adjusted-R^2 for Column (4) in Panel C to the corresponding specification in Column (5) in Panel A indicates that the inclusion of the *Loss* and *Nonlinear* interactions significantly improves the model fit (p-value<0.01).

To provide further intuition for the importance of nonlinearities in the earnings-return relation, we plot our estimated ERC function including and excluding the nonlinear term in a simple scatterplot of *CAR* and *UE* (Figure 2), limiting the graph to *UE* that fall within 1% of a firm's stock price. The function that includes the nonlinear term clearly shows the reduced return response to extreme values of unexpected earnings. A likelihood ratio test of the difference in the adjusted-R^2 for each of these specifications indicates that the model that includes the nonlinear term fits the data better (p-value<0.01).

To further explore the nonlinear relation between unexpected earnings and returns, we estimate a fractional polynomial regression in which we determine the nonlinear function that best fits the data using fractional powers from -2 to +3.[27] The regression results indicate that a fractional polynomial that includes cubic terms provides the best fit for the relation between *CAR* and *UE*, supporting our *Nonlinear* specification which essentially allows for a cubic term. Figure

[27] We use the "fp" function in Stata to perform the fractional polynomial optimization procedure. Fractional polynomials differ from regular polynomials in that they allow for logarithms, non-integer powers, repeated powers, and thus a more diverse set of functional forms. We consider non-linear transformations only of unexpected earnings, but include our full set of control variables in the estimation.

3A provides a graph of this function before and after the introduction of the PCAOB inspection regime using all treatment firms. In Figure 3B, we plot the fractional polynomial using only profitable firms. Both figures clearly indicate the predicted s-shape of the ERC, consistent with a decreasing response to extreme values of unexpected earnings. In both figures, the ERC function for the post-period sample (solid line) exhibits an upward shift in the (absolute) return response to unexpected earnings. This upward shift represents the increased reporting credibility of earnings surprises following the introduction of the PCAOB inspection regime.[28]

4.2 Sensitivity Analyses

In this section, we conduct two separate sensitivity analyses to increase confidence in our interpretation of the results in Table 3. First, we explore the possibility that other contemporaneous changes in firms' information environments could affect firms' ERCs and hence the estimated treatment effect. A maintained assumption of our Table 3 analysis is that the PCAOB inspection regime affects financial reporting credibility, but does not change other elements used in the construction of the ERCs (e.g., analysts' forecasts). However, it is possible that the PCAOB inspections could have an effect on firms' information environments. We consider three distinct changes: 1) the type, amount, and/or timing of firms' disclosures (including management guidance) prior to the earnings announcement changes; 2) the properties of firms' reported earnings change (e.g., the composition with respect to accruals); 3) analysts' forecasting behavior changes (e.g., the extent of private information production).

As an attempt to mitigate these concerns, we examine changes in seven separate proxies for changes in firms' information environments and the properties of reported earnings subsequent to the introduction of the PCAOB. We use our primary difference-in-differences

[28] For completeness, we also provide two additional fractional polynomial regression plots in the Internet Appendix: one for non-profitable (loss) treatment firms in the pre- and post-inspection periods, and one for profitable firms with triennially-inspected auditors in the pre- and post-inspection periods.

research design and apply it to each of the seven measures, i.e., we replace *CAR* in Eq. (1) with the measure and investigate whether there is any change in that proxy after the onset of the PCAOB inspection regime relative to the control group). Given the results of the proceeding analyses, we present results for both the pooled sample as well as separately for profit firms.[29]

First, we investigate changes in unexpected earnings (*UE*). If the PCAOB inspections lead to changes in the properties of unexpected earnings or affect the extent of bias in analysts' forecasts, we expect to see systematic changes in *UE* among our treatment group. Second, we examine changes in analysts' earnings forecasts (*Forecast*). A systematic change in the level of analysts' earnings forecasts (relative to the firm's stock price) could indicate a change in analysts' forecasting behavior or a change in the earnings capitalization rate.

Third, we explore the concern that firms alter their pre-earnings announcement disclosures in the presence of a higher quality audit environment (Ball et al., 2012). We examine the timeliness with which information is incorporated into prices (*Timeliness*) using an approach similar to that in Beekes and Brown (2006). This measure (described in detail in Appendix B) captures how quickly a firm's stock price converges to its year-end level. A firm with more timely disclosures should have a larger proportion of the total information released relatively early in the year. If treatment firms systematically change the timing of their disclosures over the course of the fiscal year following the introduction of PCAOB inspections, we expect to observe a significant change in *Timeliness*, relative to the control firms.

Fourth, and for the same reason, we examine the relative amount of information firms reveal prior to the earnings announcement as a proportion of the total amount of information released during the year, including the earnings announcement (*Relative Information*). Our

[29] This choice is guided by parsimony. The inferences are essentially the same if we choose the nonlinear specification instead.

measure of *Relative Information* is described in detail in Appendix B.[30] If treatment firms alter the amount of information they disclose prior to the earnings announcement in the post-PCAOB-inspection period, we expect to observe a significant change in *Relative Information*.

Fifth, we examine changes in firms' accruals (*Scaled Raw Accruals*). Changes in the magnitude of a firm's accruals are one potential measure of earnings manipulation. If treatment firms alter the extent of bias in earnings because of stricter audit oversight, we expect to observe a significant change in *Scaled Raw Accruals* relative to the control sample.

Finally, we test for changes in management guidance. For our sixth and seventh proxies, we examine whether the presence of management earnings guidance (*Earnings Guidance*) or the bundling of the earnings announcement with management guidance (*Guidance Bundle*) changes differentially for treatment and control firms in the post-PCAOB inspections period.

We present descriptive statistics for each of the information environment proxies in Panel A of Table 2. Table 4 presents the regression results. In each specification, we include our set of control variables and auditor-, country-, and year-quarter fixed effects. Across all five of the information environment proxies, the treatment effect, for both the pooled sample of profit and loss firms and for separately for profit firms, *Post×Treated,* is economically small, and generally not significant. The coefficient is significant for *UE* in Column (1) and for *Relative Information* in Column (4). The documented decrease in *UE* in Column (1) suggests that analyst forecast bias decreases for treated firms in the post period, resulting in a smaller surprise for positive earnings. This effect, however, is naturally controlled for with the main effect of *UE* in Eq. (1).

The observed increase in *Relative Information* in Column (4) suggests that, in the post-inspection period, treated firms release more of the total information for the year prior to the

[30] To avoid capturing any decrease in *Relative Information* arising mechanically from the increased response at the earnings announcement, we use the predicted return in the three-day earnings announcement window for a given level of earnings surprise using the a firm's ERC from two prior years.

earnings announcement. Earlier information release is likely to lead to a smaller response to any earnings surprise, and thus likely works against us finding an increase in the ERC. We confirm in an untabulated test that our results are robust to including *Relative Information* as an additional control (and interaction) variable in Eq. (1). Consistent with our expectation, the coefficient on *UE×Post×Treated* increases slightly (0.876) and is significant at the 1% level. Overall, the results of this analysis provide no evidence that significant changes in pre-earnings announcement disclosures, management guidance, properties of earnings, and/or analyst forecast behavior explain or alter our findings.

In our second set of sensitivity analyses, we address the possibility that the documented effects could be attributable to a market response following the accounting scandals or, alternatively, attributable to other aspects of SOX legislation, rather than PCAOB inspections. Leuz and Schrand (2009) show that, in response to the 2001-2002 accounting scandals, firms voluntarily made efforts to improve their financial disclosures. Although our use of U.S. registered non-U.S. firms as a control group mitigates this concern, it is possible that U.S.-domiciled firms respond more strongly to these scandals.

To directly gauge this concern, we examine firms audited by Arthur Andersen in 2000 and 2001. Leuz and Schrand (2009) show that former Arthur Andersen clients responded more strongly (i.e., with a larger increase in disclosure) to the revelations at Enron than other firms with other auditors. Thus, if our results reflect market responses rather than the PCAOB regime, we expect to see larger ERC changes for Arthur Andersen clients. Columns (1) and (2) of Table 5 present the results of this analysis. The treatment effect excluding Arthur Andersen clients is positive and significant (*UE×Post×Treated*=0.854). In contrast, the treatment effect for former Arthur Andersen clients is smaller and the difference between Columns (1) and (2) is significant at the 10% level. This finding is not consistent with the alternative explanation, suggesting that our findings are not driven by a shift in reporting incentives.

Next, we address the possibility that the observed ERC change could be attributable to other SOX provisions. In particular, three SOX provisions stand out as potentially affecting financial reporting credibility: 1) rules regarding audit committee independence, 2) Section 302 rules regarding executive certification of the financial statements, and 3) Section 404[b] rules regarding the assessment of internal control. Rules regarding audit committee independence became effective on April 25, 2003 for both domestic and foreign issuers, and thus affect both our treatment and control groups simultaneously (SEC Release Nos. 33-8220; 34-47654). Similarly, Section 302 had an effective date of August 29, 2002 for all domestic and foreign issuers (SEC Release No. 33-8124). Section 404[b] became effective for fiscal year-end dates on or after November 15, 2004 for U.S. accelerated filers. Because of concerns about the potential implementation costs of 404[b], the SEC deferred its implementation for firms with market capitalizations of less than $75 million (i.e., non-accelerated filers). In 2010, the Dodd-Frank Act made this exemption permanent. Foreign private issuers (classified as accelerated filers) were not subject to Section 404[b] until July 15, 2006 or July 15, 2007, depending on their size.

Prior research documents that firms with disclosed internal control weaknesses have lower quality financial reporting (e.g., Doyle et al., 2007; Ashbaugh-Skaife et al., 2008; Feng et al., 2009; Li et al., 2010) and that the market responds negatively to the disclosure of these weaknesses (e.g., Hammersley et al., 2008). If investors anticipate that firms improve their internal controls to avoid these negative outcomes, and better internal controls lead to more credible reporting, then it is possible that the effects documented in Table 3 could be attributable to the implementation of SOX 404[b], rather than the PCAOB inspections.

We conduct two additional analyses to separate the effects of the PCAOB inspections from SOX 404[b]. First, following an approach similar to Iliev (2010), we separately examine "accelerated" and "non-accelerated" filers, based on the Audit Analytics designation. If the documented increase in earnings credibility is attributable to the PCAOB inspections, rather than 404[b], we expect to find similar results for accelerated and non-accelerated filers. Results in Columns (3) and (4) of Table 6 are consistent with this prediction. The treatment effect for non-accelerated filers is 1.094 versus 0.787 for accelerated filers. These coefficients are not significantly different and, if anything, indicate a larger ERC change for non-accelerated filers.

Second, we separately examine the treatment observations based on whether Compustat identifies the firm-year as having an internal control opinion from its auditor, be it an effective, adverse, or disclaimer opinion. If markets view earnings in a regime with internal control opinions from external auditors as more credible than earnings without such opinions, we expect a larger treatment effect for the former firms. The results, presented in Columns (5) and (6) of Table 5, are inconsistent with this conjecture. The estimated treatment effect for firms without a SOX 404[b] internal control opinion (0.791) is larger than that for firms with a disclosed 404[b] auditor opinion (0.028)—the coefficient difference is statistically significant at the 5% level.

In column (7), we examine the effect of simultaneously controlling for both SOX 404[b] and SOX 302[a]. We control for these provisions by including the interactions of the *SOX404b* and *SOX302a* with *UE* as additional control variables in our primary specification. Our estimated treatment effect remains significant and is similar to that in Table 3.

In Panel B of Table 5, for each of the above specifications, we report results separately estimating the effects for profit firms and draw conclusions identical to those for the sample pooling profit and loss firms. Overall, the results of this section mitigate the concern that the documented increase in earnings credibility shown in Table 3 is attributable to a market response following the accounting scandals or the implementation of other key SOX provisions.

4.3 Triennial Inspections

Next, we examine initial full inspections of U.S.-registered, small audit firms. The PCAOB began these inspections in 2004. The staggered introduction of the triennial inspections has two advantages. First, for clients of triennially-inspected auditors, the introduction of SOX and public audit oversight mainly occur at different points in time.[31] This design is also less susceptible to confounding events arising from firm-specific responses to the accounting scandals of 2001-2002. Second, the substantial variation in the timing of the PCAOB inspections enables us to estimate ERC effects against other not-yet-inspected triennial auditors. Other clients of triennially-inspected auditors provide a more homogenous control group, which mitigates concerns about the parallel-trends assumption. With this control group, the identification of the treatment effect comes solely from *within-sample* variation in the timing of the inspections. The main drawback of this setting is that the sample of issuers with triennially-inspected auditors is fairly small.

We perform difference-in-differences tests to measure the effect of the triennial full inspections, estimating the following equation (suppressing time and firm subscripts):

$$\begin{aligned} CAR = \alpha + \beta_1 UE + \beta_2 Post + \lambda_n Controls + \gamma_n Fixed\ Effects \\ \beta_n UE \times Controls + \beta_n UE \times Fixed\ Effects + \beta_3 UE \times Post + \varepsilon \end{aligned} \quad (2)$$

We include *CAR*, *Post, UE*, and controls as indicated in the table and defined above. We also include auditor- and year-quarter fixed effects, plus the interactions of these fixed effects with

[31] We explicitly gauge the overlap in the timing of the PCAOB triennial inspections and the implementation of SOX provisions 404[b] and 302[a]. We find that the timing of the first SOX 404[b] opinion coincides with the initial inspection year for only 10.5% (6.8%) of our sample firms using the fieldwork (inspection report) cutoff date. The timing of the first SOX 302[a] opinion coincides with the initial inspection year for only 2.5% (2.7%) of our sample firms using the fieldwork (inspection report) cutoff date. We provide these analyses in the Internet Appendix.

UE. The coefficient β_3, the interaction between *UE*×*Post*, captures the treatment effect of the triennial PCAOB inspections. We include all available firm-year observations for firms with small auditors from 2001 through 2007.[32] We exclude fiscal year-ends subsequent to Q2 of 2008 to mitigate the potentially confounding effects of the financial crisis. We require that an auditor have at least one client observation both prior to and after treatment to be included in the sample. As in Table 3, we combine samples using alternative measurement dates (fieldwork and inspection report release) to increase the power of our tests.

In Appendix A, Panel B, we provide some specific examples for how we code the *Post* indicator for a variety of fiscal year-end dates and inspection years. For triennially-inspected firms, the fieldwork is shorter and it is less clear that the market is aware of the timing of the fieldwork. We therefore code the *Post* variable equal to one for any earnings announcement occurring 30 days after the end of the PCAOB's inspection fieldwork (or alternatively the day following the inspection report release). However, for most triennial firms, there is a significant lag (on average 306 days, see Table 2 Panel B) from the end of fieldwork to the measurement of the ERC. This lag occurs because inspections are not aligned with issuers' fiscal year-ends and earnings announcements occur several months after the fiscal year-end.

Table 6 presents results for this analysis. In Column (1), we estimate an OLS regression of Eq. (2) for the combined sample. The estimated treatment effect of 0.357 is positive and significant at the 10% level. In Column (2), we estimate Eq. (2) using a robust regression. *UE*×*Post* is positive (0.110), but not statistically significant. Using the coefficient in Column (1) and a 10%-benchmark, the estimated treatment effect amounts to a decline in the cost of capital of approximately 34 basis points. While this magnitude is still economically meaningful, it is

[32] To increase sample size for the triennial inspections, we extent the window over which we measure the median analyst forecast based on which *UE* is computed from 95 days to 360 days.

smaller than for annually-inspected auditors. In this regard, it is important to note that the within-sample estimation is quite stringent and requires that the ERC effect manifests in the year the treatment indicator switches on. Thus, getting the timing of when the market updates its assessment of reporting credibility "right" is critical. If, for instance, the market response takes two years to manifest, then our coding would result in a downward-biased estimate of the effect.

Furthermore, smaller issuers could be more prone to losses and extreme earnings surprises. Thus, accounting for losses and extreme earnings surprises in the ERC estimation is likely more important for the triennial analysis. In Columns (3) and (4), we consider an alternative specification that partitions the sample into profit and loss firms for the both the OLS and robust regression specifications. In Columns (4) and (5), we include the loss partitioning, *Nonlinear*, and the interaction of *Nonlinear* with the treatment indicators. Across all four columns, *UE*×*Post* is positive and statistically significant (at the 5% level or better). The estimated magnitude of the treatment effect is now much larger than in Columns (1) and (2), and is more consistent with the earlier analyses.

In Panel B of Table 6, we examine the change in ERCs separately for each alternative cutoff date. In both specifications, *UE*×*Post* is positive and statistically significant. The coefficient magnitude is more than twice as large for the inspection report release, which is consistent with less publicized fieldwork dates. In Column (3), we report results for the "dropped observations" design, which excludes the post-fieldwork period from the pre-inspection report release sample to avoid overlap and contamination, as in Panel B of Table 3. We find that the treatment effect is substantially larger (2.472). In Column (4) of Table 6, we include additional controls for SOX 404[b] and 302[a], and find similar results, which indicates that the increases in reporting credibility are not attributable to other SOX provisions.

Overall, the results from our analysis of the triennial PCAOB inspections are consistent with a significant increase in financial reporting credibility following the introduction of the PCAOB and its inspection regime.

4.4 Falsification Test

In this section, we conduct a falsification test in which we counterfactually vary the timing of our treatment window. This analysis provides a more formal test that the ERC changes occur when predicted based on the timing of the PCAOB inspections. We focus on the initial full inspections for large auditors and separately estimate regressions using the end of fieldwork and the report release as alternative cutoff dates. We then vary the timing of the PCAOB inspection year-by-year from two years prior to two years subsequent to the actual cutoff date for the onset of the inspection regime. We expect to observe an attenuation of the treatment effect in both directions. Moreover, an insignificant coefficient of interest in the pre-period suggests that, all else equal, the ERCs of treatment and control firms are similar and that the parallel-trends assumption is likely to be reasonable.

Table 7 Panel A (Panel B) presents results using the fieldwork completion (inspection report release) as cutoff dates estimating the specifications reported in Table 3, Panel B(1), Columns (4) and (5). In both specifications, the treatment effect is the largest for the actual treatment date and dissipates as we move the treatment period away from these dates. Note that, because we measure the treatment effect over a two-year window, for the fieldwork (inspection report release) window the period *t+1* (*t-1*) overlaps with the inspection report (fieldwork) window. Consistent with this overlap, in Table 7 Panel A (Panel B) there is a significant effect in *t+1* (*t-1*). As noted earlier, the insignificant pseudo-treatment effect in Column (1) of both panels provides further evidence of the validity of the parallel-trends assumption. In the Internet

Appendix (Figure 5), we also map out the estimated treatment effect in event time to further illustrate that the ERC increase occurs around the introduction of the inspection regime.

4.5 Abnormal Trading Volume around 10-K filings as an Alternative Credibility Proxy

In this section, we consider an alternative proxy for reporting credibility. We examine abnormal trading volume around the filing of firms' annual financial statements (hereafter, "10-Ks") with the SEC as an alternative measure of market participants' response to the PCAOB inspections.[33] While prior literature generally interprets abnormal trading volume as a measure of the information content of a disclosure (e.g., Asthana and Balsam, 2002; Asthana et al., 2004; Leuz and Schrand, 2009), it is also likely a function of the credibility of the information released. In a standard model of diverse private information with a public information release, increases in the signal-to-noise ratio of disclosures in the 10-K should increase trading volume (e.g., Kim and Verrecchia, 1991). Like ERCs, this measure is not anticipatory in nature and can be measured over short intervals around the 10-K release dates. Unlike the ERC, it does not have to be estimated for a sample but can be observed at the firm-year level.

Following prior literature (e.g., Asthana et al, 2004; Leuz and Schrand, 2009), we calculate abnormal volume, *Abnormal 10-K Volume*, using trading volume within a window that begins one trading day prior to the 10-K and ends three trading days after. We normalize raw trading volume by subtracting the mean trading volume in the 45-trading-days beginning five trading days prior to the 10-K release and dividing by the standard deviation of trading volume calculated over the same window. We exclude from this calculation any days in the three-day earnings announcement window. We then define *Abnormal 10-K Volume* as the mean of the normalized trading volume in the five-day (from *t-1* to *t+3*) window surrounding the 10-K.

[33] We use "10-K" as shorthand, but include for any initial filing with the SEC that includes the annual financial statements, including 10-K, 10-K405, 10-KSB, 20-F, etc.

We perform difference-in-differences tests of changes in *Abnormal 10-K Volume* after the introduction of the PCAOB inspection regime by estimating the following equation:

$$Abnormal\ 10\text{-}K\ Volume = \alpha + \beta_1 Post + \beta_2 Treated + \beta_3 Post \times Treated + \beta_n Controls + \beta_n Fixed\ Effects + \varepsilon \quad (3)$$

We again use two alternative measurement dates, the completion of fieldwork and the issuance of the inspection report, and pool data across limited and full inspections in a single combined analysis. We use the same treatment and control samples as in our primary analyses. Following Leuz and Schrand (2009), we include several controls from the ERC tests including *Size*, *Market-to-Book*, *Leverage*, *Beta*, and *Loss*. We control for the number of days from a firm's fiscal year-end to the 10-K release (*Filing Delay after FYE*) and from the earnings announcement to the 10-K release (*Filing Delay after EA*) following Asthana et al (2004). We also include *Analyst Following*, which is a count of the number of unique analysts who make a forecast in the year leading up to the earnings announcement.

We present descriptive statistics for this analysis in the Internet Appendix. While the sample size is much larger than the ERC analysis, the majority of the sample observations (89%) are again from the treatment group. On average, *Abnormal 10-K Volume* is positive. At the median, a firm files its 10-K 83 days after the fiscal year-end and 36 days after the earnings announcement. Four analysts follow the median firm. Across the other control variables, the sample is fairly comparable to the main ERC tests.

We present regression results in Table 8. In Column (1), we estimate Eq. (3) using OLS and include auditor-, country-, and year-quarter fixed effects. In Column (2), we estimate the robust regression specification. In Column (3), we replace auditor- and country-fixed effects with firm-fixed effects.[34] Across Columns (1)-(3), the treatment effect, *Post×Treated*, is positive and significant (at the 5% level or greater).[35] In Column (2), for example, the coefficient of interest has a magnitude of 0.044, which translates into an increase in abnormal trading volume of 4.4% of one standard deviation of the *Abnormal 10-K Volume* measure. In Column (4), we include additional controls for SOX provisions 404[b] and 302[a] and find results of a similar magnitude to those in Columns (1)-(3).

Overall, the results in this section suggest that abnormal trading volume at the 10-K release increased for firms whose auditors were subject to PCAOB inspections, and thus are consistent with those in our primary, ERC-based, analyses.

5. Conclusion

This paper examines how mandated audit oversight by a public-sector regulator affects the assessment of reporting credibility in capital markets. We provide evidence on this question by analyzing whether the introduction of the PCAOB and its inspection regime has strengthened capital-market responses to issuers' earnings surprises, as would be expected if the new oversight regime for the audit profession enhances the credibility of reported earnings.

We use a difference-in-differences design that exploits the staggered introduction of the inspection regime, which affects firms at different points in time depending on their fiscal year-ends, auditors, and the timing of PCAOB inspections. Consistent with an increase in reporting credibility after the introduction of the PCAOB, we find that capital-market responses to unexpected earnings increase significantly following the introduction of the inspection regime.

[34] We do not consider firm-fixed effects in the ERC analyses because we would have to interact the firm-fixed effect with *UE*, which is problematic given the short-time series for each sample firm.

[35] Although abnormal 10-K volume can be more precisely estimated than ERCs, and thus outliers are less of an issue, for consistency with our earlier analyses, we also estimate a percentile rank regression of Eq. (3) and find results consistent with those in Table 8. Relatedly, we also consider the log transformation of actual (rather than abnormal) volume as an alternative dependent variable. In this specification, we include firm-fixed effect to control for a firm's normal level of volume. Results with this alternative dependent variable are similar to those in Table 8.

The effects are present for firms with Big Four, other annually-inspected auditors as well as smaller, triennially-inspected auditors. The findings do not appear to be driven by other SOX provisions unrelated to audit oversight. Corroborating these results, we find that abnormal trading volume reactions to 10-K filings increase after the introduction of the inspection regime.

Overall, our study provides large-sample evidence on the capital-market benefits of the PCAOB inspection regime, which was an integral part of SOX. Our evidence suggests that public audit oversight can have capital-market benefits by enhancing the credibility of financial reporting. It also provides further support for the notion that financial reporting credibility is priced in capital markets.

Despite many sensitivity analyses, the aforementioned results should be interpreted cautiously as our study is subject to several limitations. First, although our analyses show sustained increases in reporting credibility for at least two years, ERCs are based on investor perceptions and hence can change as more information about the inspection regime (as well as reporting and audit quality) comes to the market. Second, the attribution of the credibility effect to the PCAOB regime depends critically on our ability to control for other concurrent changes in regulation and in markets. We employ difference-in-differences analyses around the staggered implementation of the inspection regime to tackle this issue, but this design relies on the parallel-trends assumption. Third, ERCs are difficult to measure and as a result can be fairly noisy. Therefore, the magnitude of our estimates needs to be interpreted carefully. Fourth, the introduction of public audit oversight was a key provision in SOX, but there were many others. While we provide evidence that SOX's internal control provisions are likely not the driver of our results, it is still possible and difficult to rule out that our results reflect the joint effect of certain SOX provisions and public audit oversight. Fifth, it is important to recognize that our study focuses on capital-market benefits of public audit oversight but does not examine the costs of the new regime. Thus, we neither show *net* benefits nor do we attempt to provide a complete cost-benefit analysis. Finally, our analysis is limited to equity investors. Other studies show that well-functioning accounting systems (Costello and Wittenberg-Moerman, 2011) and assurance by auditors (Minnis, 2011) can increase the usefulness of financial reporting for debt contracting. Thus, it is conceivable that public audit oversight also provides benefits to (and has costs for) other stakeholders. We leave this question to future research.

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Appendix A – Details on the Introduction of the PCAOB Inspection Regime

Panel A: Annually-Inspected Auditor Fieldwork and Inspection Report Release Dates

Auditor	Fieldwork		Report Date
	Commences	Concludes	
Limited Inspections			
Big-Four Auditors			
Deloitte & Touche	June 2003	December 2003	Aug 28, 2004
Ernst & Young	June 2003	December 2003	Aug 28, 2004
KPMG	June 2003	December 2003	Aug 28, 2004
PricewaterhouseCoopers	June 2003	January 2004	Aug 28, 2004
Full Inspections			
Big-Four Auditors			
Deloitte & Touche	May 2004	November 2004	Oct 06,2005
Ernst & Young	July 2004	December 2004	Nov 17, 2005
KPMG	June 2004	October 2004	Sep 29, 2005
PricewaterhouseCoopers	May 2004	January 2005	Nov 17, 2005
Tier-Two Auditors			
BDO	May 2004	July 2004	Nov 17, 2005
Crowe Chizek	November 2004	December 2004	Jan 19, 2006
Grant Thornton	May 2004	March 2005	Jan 19, 2006
McGladrey & Pullen	October 2004	December 2004	Nov 30, 2005

Panel A provides the beginning and end dates for PCAOB fieldwork and the inspection report release dates for both limited and full inspections by auditor.

Appendix A –Details on the Introduction of the PCAOB Inspection Regime

Panel B: Examples illustrating our coding of the *Post* variable for triennially-inspected auditors

	Fiscal Year-End												
Issuer	Q4 2003	Q1 2004	Q2 2004	Q3 2004	Q4 2004	Q1 2005	Q2 2005	Q3 2005	Q4 2005	Q1 2006	Q2 2006	Q3 2006	Q4 2006
Nu Horizons Electronics (February FYE) Auditor: Lazar Levine & Felix LLP Auditor Inspected: 11/8/04 – 11/18/04		**0** 5/5 N/A				**1** 5/11 174				**1** 5/9 537			
Mediware Info Systems (June FYE) Auditor: Eisner LLP Auditor Inspected: 5/10/04 – 6/5/04			**0** 8/31 N/A				**1** 9/2 454				**1** 9/6 823		
Bio Reference Labs (October FYE) Auditor: Moore Stephens PC Auditor Inspected: 5/10/04 – 5/14/04				**1** 1/6 237				**1** 1/5 601				**1** 12/19 949	
Madden Steven LTD (December FYE) Auditor: Eisner LLP Auditor Inspected: 5/10/04 – 6/5/04	**0** 2/26 N/A				**1** 3/1 269				**1** 3/2 635				**1** 3/1 999
TXCO Resources Inc. (December FYE) Auditor: Akin Doherty Klein & Feuge PC Auditor Inspected: 5/17/05 – 5/20/05	**0** 3/5 N/A				**0** 3/14 N/A				**1** 3/8 292				**1** 3/12 661
First Merchants Corp (December FYE) Auditor: BKD LLP Auditor Inspected: 5/22/06–5/25/06	**0** 1/21 N/A				**0** 1/28 N/A				**0** 1/27 N/A				**1** 1/23 243

Panel B provides examples illustrating how we code the *Post* variable for analyses using the end of the inspection fieldwork as the cutoff date. For triennially-inspected auditors, *Post* is an indicator variable that equals one for any firm fiscal year-end 30 days after the conclusion of PCAOB inspection fieldwork of the firm's auditor, and zero otherwise. As illustrated by the examples above, the inspection dates, and therefore the time series of the *Post* variable, vary across auditors. Each 0/1 coded cell (emphasized in bold) represents a firm-year observation. Each cell also includes the earnings announcement date and the time interval, in days, between the end of PCAOB fieldwork and the earnings announcement date of the firm. The latter highlights that there is often a substantial lag between the conclusion of the PCAOB inspection and the client's earnings announcement, giving auditors time to adjust their audit procedures. Although the issuers listed in the table are clients of the inspected auditor, the table does not imply that the specific engagement with the issuer was or was not inspected (this information is not publicly available). The purpose of the analysis is to examine whether treatment of the auditor with the PCAOB inspection regime increases reporting credibility of the issuers, irrespective of inspections of specific engagements (and their outcomes).

Appendix B - Variable definitions

Variables Used in Calculating Earnings Response Coefficients

$CAR_{i,t}$	A firm's 3-day return, centered on the earnings announcement date, less the CRSP market return over the same period. The earnings announcement date is defined as the earliest date available on Compustat or I/B/E/S. If the earnings announcement date is taken from I/B/E/S, the announcement date is the same (next) trading day if the announcement time is earlier (later) than 4pm EST.
$UE_{i,t}$	The difference between the I/B/E/S actual, annual EPS and the median I/B/E/S forecast of annual EPS from each analyst's most recent forecast in a window beginning 95 calendar days prior to the earnings announcement and ending 3 days prior to the earnings announcement scaled by the CRSP price from 2 days prior to the earnings announcement. For the triennially-inspected-auditor analysis, we supplement these forecasts by including the difference between the I/B/E/S actual, annual EPS and the median I/B/E/S forecast of annual EPS from each analyst's most recent forecast in a window beginning 360 calendar days prior to the earnings announcement and ending 3 days prior to the earnings announcement when the shorter window, detailed above, does not contain a forecast.

PCAOB Inspection Indicators

$Post_{i,t}$	An indicator variable, based on an auditor's global network, that equals one for all firm-years subsequent to a firm's auditor's U.S. affiliate's treatment through the PCAOB inspection process, defined for each event as follows: 1) Big Four limited and full inspection fieldwork and Tier Two full inspection fieldwork: *Post* equals one if a firm's fiscal year-end is in the same month as the final month of fieldwork (as indicated in Appendix A Panel A) or later, and zero otherwise; 2) triennially-inspected auditor full inspection fieldwork: *Post* equals one if a firm's fiscal year-end is after the auditor-specific fieldwork end date plus 30 days, and zero otherwise; 3) Big Four limited and full inspection report release, triennially-inspected auditors' inspection report release, and Tier Two full inspection report release: *Post* equals one if a firm's fourth quarter earnings announcement falls on or after the release date of the inspection report (as indicated in Appendix A Panel A), and zero otherwise.
$Treated_{i,t}$	An indicator variable coded as one if a firm is audited by an auditor subject to a (limited or full) PCAOB inspection, and zero otherwise. In the limited and annual full inspection settings, this variable is collinear with the USA fixed effect.

Control Variables

Analyst $Following_{i,t}$	The count of the number of unique analysts who issue at least one forecast on I/B/E/S in a window beginning 360 days prior to the earnings announcement and ending 3 days prior to the earnings announcement. When no forecasts are observed, we set this count to zero.
$Beta_{i,t}$	The coefficient from regressing excess daily returns for firm *i* on excess market returns over one calendar year, ending on the fiscal year-end date. The risk free rate is collected from Ken French's data library.
Filing Delay after $EA_{i,t}$	The count of the number of days between the earnings announcement date defined as the earlier of that available on Compustat or I/B/E/S and the filing date of the 10-K defined as the earlier of the date reported by Audit Analytics or WRDS SEC Analytics.
Filing Delay after $FYE_{i,t}$	The count of the number of days between the firm's fiscal year-end date from Compustat and the filing of the 10-K, defined as the earlier of the date reported by Audit Analytics or WRDS SEC Analytics.
$Leverage_{i,t}$	The ratio of total liabilities to total equity, measured at the fiscal year-end, from Compustat.
$Loss_{i,t}$	An indicator variable coded as one when basic earnings per share excluding extraordinary items (Compustat epspx) is less than zero, and zero otherwise.

Market-to-Book$_{i,t}$	The ratio of the market value of equity to the book value of equity, measured at the fiscal year-end, from Compustat.
Nonlinear$_{i,t}$	A variable equal to $UE_{i,t} \times \lvert UE_{i,t} \rvert$, equivalent to using a cubic term in the regression.
Persistence$_{i,t}$	The coefficient from regressing basic EPS excluding extraordinary items from Compustat on lagged EPS using (where available) up to 10 years of data.
Size$_{i,t}$	The log of market value of equity, measured at fiscal year-end, from Compustat.
SOX302a$_{i,t}$	An indicator variable coded as one when the "IS EFFECTIVE" variable in the Audit Analytics SOX 302 data set is coded as a '0', '1', or '2', and zero otherwise. This variable is only coded 1 for domestic firms.
SOX404b$_{i,t}$	An indicator variable coded as one when the auditor internal control opinion (AUOPIC) variable in Compustat shows an adverse, qualified, or unqualified indicator, and zero otherwise. This variable is only coded 1 for domestic firms.

Alternative Dependent Variables

Abnormal 10-K Volume$_{i,t}$	The mean abnormal trading volume from one day prior to the filing date of the 10-K to three days after. Abnormal trading volume is defined as raw volume less mean daily volume over a window from 49 days prior to the annual financial statement report release to five days prior to the report release (excluding any 3-day earnings announcement window days) divided by the standard deviation of daily volume over the same window. All volume data is from CRSP. The 10-K filing date is defined as the earlier of the date reported by Audit Analytics (as long as it is after the earnings announcement date) and the first observable 10-K date from WRDS SEC Analytics in a 180-calendar-day window beginning on the earnings announcement date.
Earnings Guidance$_{i,t}$	An indicator variable coded as one when a guidance observation, quarterly or annual, is available for the fiscal year-end date on either First Call or I\B\E\S, and zero otherwise.
Forecast$_{i,t}$	The median I/B/E/S forecast of annual EPS from each analyst's most recent forecast in a window beginning 95 days prior to the earnings announcement and ending 3 days prior to the earnings announcement scaled by the CRSP price from 2 days prior to the earnings announcement.
Guidance Bundle$_{i,t}$	An indicator variable coded as one when management provides earnings guidance for any fiscal period, quarterly or annual, within one day of the earnings announcement on either First Call or I\B\E\S, and zero otherwise.
Relative Information$_{i,t}$	This variable captures the share of information arriving prior to the earnings announcement relative to the total amount over a firm's fiscal year. Calculated as the sum of the absolute value of daily, market-adjusted CRSP returns from 345 calendar days prior to the earnings-announcement window until the day before the earnings-announcement window, divided by the same plus predicted returns (based on the implied return to a given level of earnings surprise using the firm's estimated ERCs) for the 3-day earnings announcement window, scaled by 100. $$100 \cdot \frac{\sum_{d=-345}^{0} \lvert r_{i,d} - r_{M,d} \rvert}{\left\lvert \widehat{\alpha_{lag2}} + UE_i \cdot \widehat{ERC_{lag2}} + Loss_i \cdot \widehat{\beta_{lag2}^{Loss}} + UE_i \cdot Loss_i \cdot \widehat{\beta_{lag2}^{LossERC}} \right\rvert + \sum_{d=-345}^{0} \lvert r_{i,d} - r_{M,d} \rvert}$$ Returns are from CRSP and *d* represents the number of calendar days relative to two trading days prior to the earnings announcement. To increase the precision of the measurement, we allow separate ERC coefficients for profits and losses.
Scaled Raw Accruals$_{i,t}$	The difference between net income and cash flow from operations scaled by average total assets from Compustat.

Timeliness$_{i,t}$	This variable captures how quickly market prices impound the information reflected in price at $p_{d=0}$, calculated following Beekes and Brown (2006), given by the equation: $$-1 \cdot \frac{\sum_{d=-345}^{0} \left| \log(p_{d=0}) - \log(p_d) \right|}{\sum_{d=-345}^{0} 1_d}$$ We multiply by -1 so the measure is increasing in timeliness. Prices are from CRSP and *d* represents the number of calendar days relative to two trading days prior to the earnings announcement. The indicator function in the denominator turns on when *d* is a trading day.

Throughout the table, subscripts *i* and *t* refer to a particular firm and fiscal year, respectively.

Figure 1: Design of our Tests around the Introduction of the PCAOB Inspection Regime

Panel A: Limited Inspections

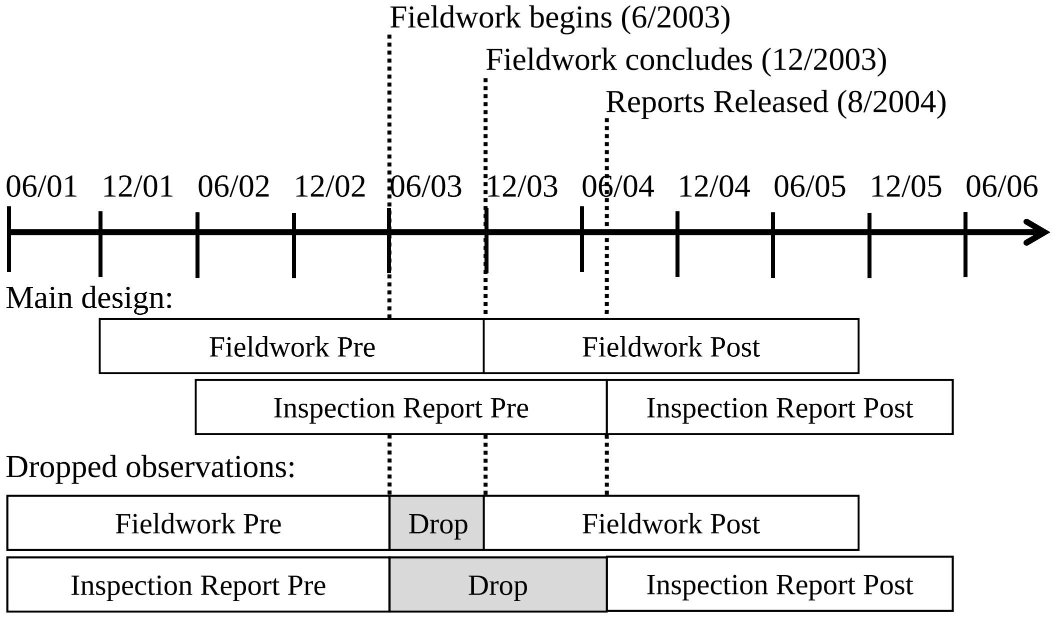
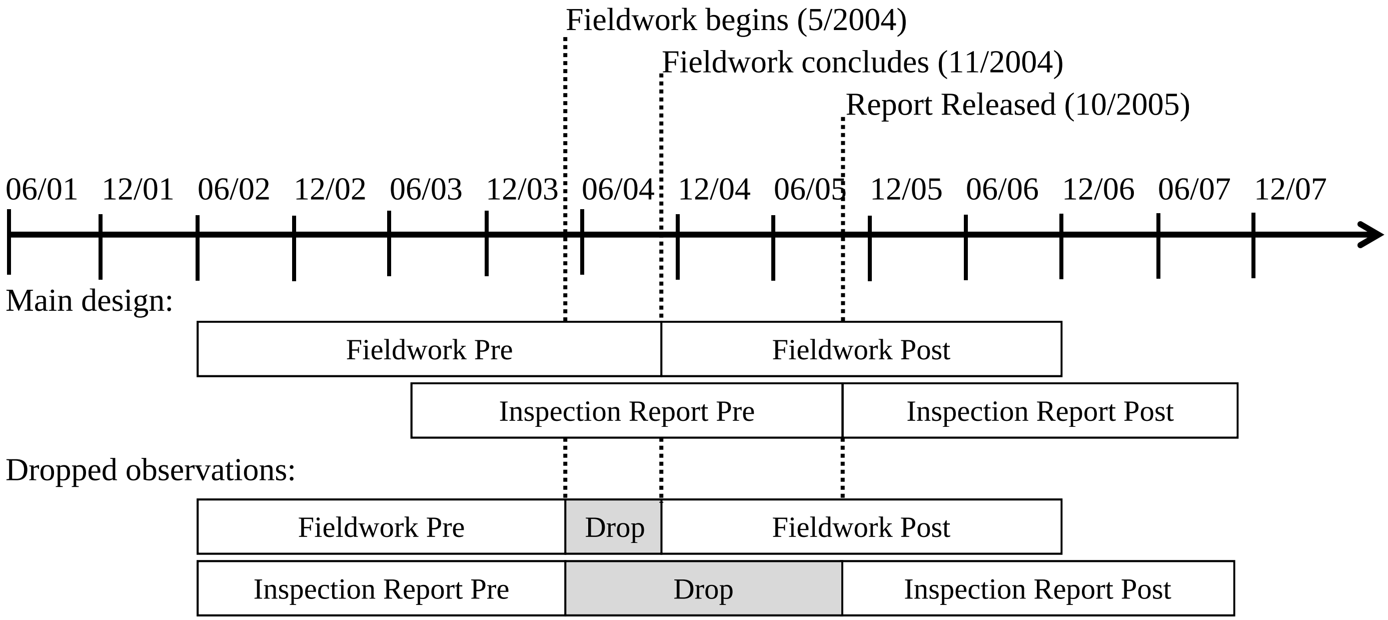


Figure 1 illustrates our research design. Panel A describes the coding of the *Post* variable around the Limited Inspections. We use two different designs. In the "Main design," we use the conclusion of the fieldwork or the release of the inspection report as alternative cutoff dates to define adjacent pre and post periods. We then use two earnings announcements in the pre- and the post period for a given firm. In the "Dropped observations" design, we exclude fiscal year-ends that occur *during* PCAOB fieldwork when using the fieldwork end as the cutoff date and fiscal year-ends that occur between the start of fieldwork and the release of the inspection report when using the inspection report release as the cutoff date. The idea of the dropped observations design is to avoid contamination. Timeline dates are presented MM/YY.

Figure 1: Design of our Tests around the Introduction of the PCAOB Inspection Regime (continued)

Panel B: Full Inspections (specific dates presented from Deloitte & Touche as an example)



Panel B describes the coding of the *Post* variable around the Full Inspections. We use two different designs. In the "Main design," we use the conclusion of the fieldwork or the release of the inspection report as alternative cutoff dates to define adjacent pre and post periods. We then use two earnings announcements in the pre and the post period for a given firm. In the "Dropped observations" design, we exclude fiscal year-ends that occur *during* PCAOB fieldwork when using the fieldwork end as the cutoff date and fiscal year-ends that occur between the start of fieldwork and the release of the inspection report when using the inspection report release as the cutoff date. The idea of the dropped observations design is to avoid contamination. Timeline dates are presented MM/YY.

Figure 2: Scatterplot and Fitted Values Including and Excluding *Nonlinear*

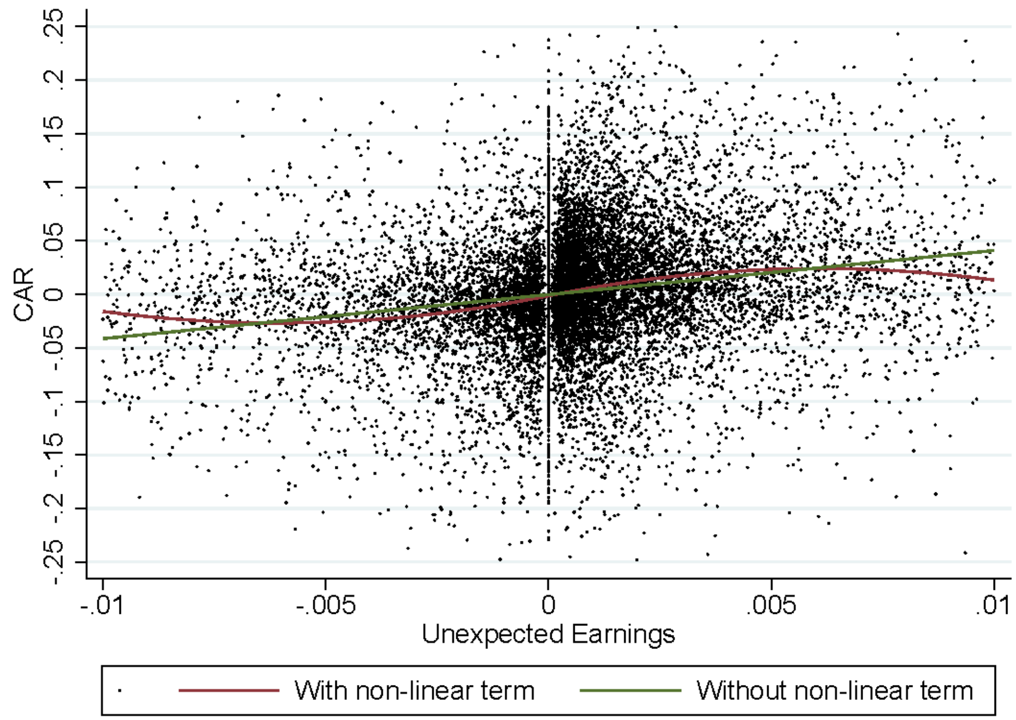
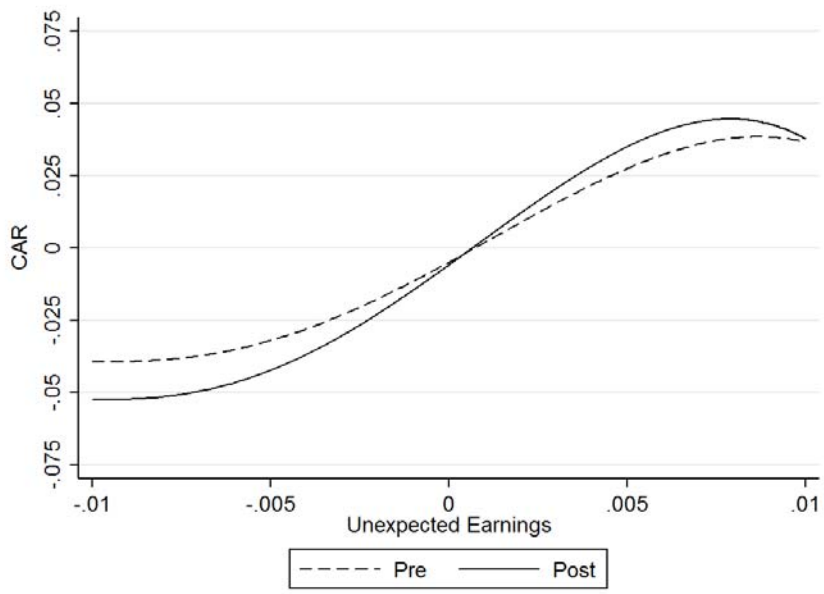


Figure 2 presents a scatterplot and fitted values for cumulative abnormal returns (*CAR*) and unexpected earnings (*UE*). We plot all firm-year observations from the combined analysis (stacking limited and full inspections and using both alternative cutoff dates) provided unexpected earnings are within ±1% of price. This sample comprises 36,962 observations (or 90.7% of the combined sample). The green fitted-value plot is based on a (linear) regression of *CAR* on *UE*. The red-fitted value plot is based on a regression of *CAR* on *UE* and *Nonlinear*, essentially allowing for a cubic term. We provide detailed variable definitions in Appendix B.

Figure 3: Fractional Polynomial Regression Plots

Panel A: Treatment Pre vs. Post



Panel B: Treatment Pre vs. Post, Profitable Firms Only

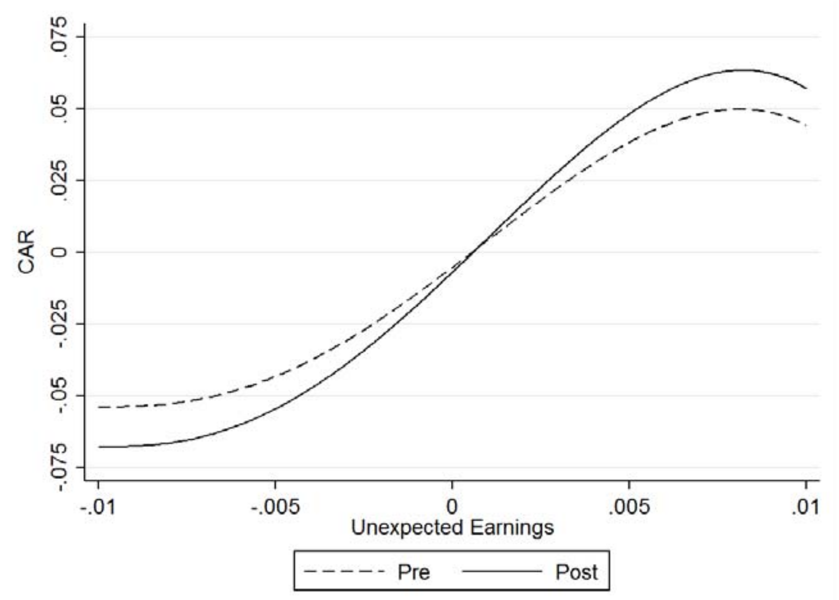


Figure 3 presents plots of fractional polynomial regressions of cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*) using Eq. (1). Fractional polynomial regressions provide flexible parameterization for continuous variables without predetermining the shape. The procedure searches over a set of possible polynomial functions for the model that best fits the data. We use Stata's 'fp' function, which by default allows for the following non-integer powers (-2, -1, -0.5, ln(x), 0.5, 1, 2, 3) along with repeated powers multiplied by ln(x). We include powers for unexpected earnings only, but include the full set of control variables. The sample comprises treated firms from the combined analyses, provided unexpected earnings are within ±1% of price. Panel A plots the fractional polynomial for treatment firms in the pre-inspection (the dashed line) and the post-inspection (the solid line) periods. The sample comprises 33,908 observations (or 91.6% of the combined treatment sample). Panel B plots the fractional polynomial using only profitable treatment firms in the pre-inspection (the dashed line) and the post-inspection (the solid line) periods. This sample comprises 28,682 observations (or 97.4% of the combined treatment sample of profitable firms). We provide detailed variable definitions in Appendix B.

Table 1: Sample Composition

Panel A: Number of Firms by Auditor, Inspection Type, and Measurement Cutoff Date

	(1)	(2)	(3)	(4)	(5)	(6)
	Firms					Firm-Years
	Limited Inspections		Full Inspections		Combined	
Treatment Sample	**Fieldwork**	**Reports**	**Fieldwork**	**Reports**		
Big Four Auditors						
Deloitte & Touche	679	714	768	728	825	7,456
Ernst & Young	986	1,028	1,122	1,044	1,198	10,878
KPMG	772	787	830	760	881	8,066
PwC	888	873	920	844	999	9,249
Tier Two Auditors						
BDO Seidman	-	-	118	117	124	464
Crowe Chizek	-	-	46	43	46	185
Grant Thornton	-	-	166	167	179	566
McGladrey & Pullen	-	-	33	36	37	137
Subtotal	**3,325**	**3,402**	**4,003**	**3,739**	**4,289**	**37,001**
Control Sample						
Big Four Auditors						
Deloitte & Touche	95	109	63	59	126	746
Ernst & Young	108	123	89	81	137	953
KPMG	122	125	67	61	138	891
PwC	156	158	95	76	176	1,169
Tier Two Auditor						
Grant Thornton	-	-	2	2	2	6
Subtotal	**481**	**515**	**316**	**279**	**579**	**3,765**
Total	**3,806**	**3,917**	**4,319**	**4,018**	**4,868**	**40,766**

Table 1 provides details on the sample composition for our limited, full, and triennial-inspection analyses. Panel A describes the sample composition for the limited and full inspections by auditor, inspection type, and measurement cutoff date. Columns (1) through (4) report the count of firms with available data for each of the four separate measurement dates (limited inspection fieldwork end, limited inspection report release, full inspection fieldwork end, and full inspection report release). We define the exact timing for each of these events in Appendix A, Panel A. In Column (5), we report the number of unique firms in the combined analysis in which we stack all inspections and measurement periods. The combined analysis therefore includes the same firm up to four times. In Column (6), we report the number of firm-years for the combined analysis. We include any firm fiscal year-end that is within two years (before or after) of the respective cutoff date. We require that a firm have available data on Audit Analytics, Compustat, CRSP, and I/B/E/S.

Table 1: Sample Composition (continued)

Panel B: Breakdown of the Treatment and Control Samples by Auditor Location

	(1)	(2)
	All Events	
Auditor Country	**Firms**	**Firm-Years**
USA	4,289	37,001
ARGENTINA	5	11
AUSTRALIA	11	85
BERMUDA	12	47
BRAZIL	9	33
CANADA	151	786
CHILE	15	66
CHINA	46	280
FRANCE	26	176
GERMANY	16	139
GREECE	11	58
INDIA	8	37
IRELAND	11	102
ISRAEL	46	346
ITALY	9	54
JAPAN	18	166
KOREA (SOUTH)	6	24
LUXEMBOURG	4	20
MEXICO	18	71
NETHERLANDS	18	167
NORWAY	5	62
SINGAPORE	4	32
SOUTH AFRICA	6	37
SPAIN	4	35
SWEDEN	7	57
SWITZERLAND	9	85
TAIWAN (CHINA)	9	108
UNITED KINGDOM	60	483
Other	35	198
Total	**4,868**	**40,766**

Panel B provides a breakdown of the treatment and control samples by auditor location. Column (1) reports the number of unique firms by country. Column (2) reports the number of firm-years by country for the combined analysis of all inspections and measurement periods.

Table 1: Sample Composition (continued)

Panel C: Number of Newly-Treated, Triennially-Inspected Auditors and Firm-Years

	(1)	(2)	(3)	(4)
	Fieldwork		**Inspection Reports**	
Calendar Year	**Newly Inspected Auditors**	**Firms**	**Newly Reported-on Auditors**	**Firms**
2004	13	66	-	-
2005	16	57	15	47
2006	40	284	21	126
2007	8	31	30	171
2008	-	-	2	17
Total	**77**	**438**	**68**	**361**
Total Firm-Years		**1,229**		**918**

Panel C provides a sample breakdown of the number of newly-treated, triennially-inspected auditors and the number of their unique client firms and firm-years. We include all firm-years on Compustat with fiscal years ending between Q2 2001 and Q2 2008 that meet the following requirements: 1) the firm has available data on Audit Analytics, Compustat, CRSP, and I/B/E/S, and 2) there are at least four client observations per auditor, with at least one observation in the pre-inspection period and one observation in the post-inspection period. At the end of the sample period, all auditors in our sample have been inspected. Column (1) [Column (2)] reports the number of newly-treated auditors by calendar year, using the inspection fieldwork [report release] as cutoff date. Column (3) [Column (4)] reports the number of client firms associated with the newly-inspected auditors. In the last row, we report the number of firm-years contributed by these firms.

Table 2: Descriptive Statistics for the Limited, Full, and Triennial Inspection Samples

Panel A: Annually-Inspected Auditors

Variable	N	Mean	Std. Dev	P25	Median	P75
CAR	40,766	0.002	0.064	-0.030	0.001	0.034
UE	40,766	0	0.008	-0.001	0	0.002
Loss	40,766	0.181	0.385	0	0	0
Size	40,766	7.119	1.583	5.987	7.001	8.142
Market-to-Book	40,766	2.968	2.619	1.584	2.256	3.494
Leverage	40,766	2.660	4.100	0.504	1.150	2.593
Persistence	40,766	0.280	0.454	0	0.282	0.552
Beta	40,766	1.078	0.551	0.688	1.019	1.424
Forecast	40,353	0.033	0.073	0.025	0.046	0.064
Timeliness	40,329	-0.203	0.157	-0.258	-0.157	-0.094
Relative Information	40,298	99.81	0.142	99.74	99.84	99.92
Scaled Raw Accruals	38,480	-0.056	0.076	-0.085	-0.047	-0.015
Earnings Guidance	40,766	0.495	0.500	0	0	1
Guidance Bundle	40,766	0.367	0.482	0	0	1
Post	40,766	0.507	0.5	0	1	1
Treated	40,766	0.908	0.29	1	1	1
Timing: Treatment to First EA (in days)	12,436	241.0	193.3	88	165	386

Panel B: Triennially-Inspected Auditors

Variable	N	Mean	Std. Dev	P25	Median	P75
CAR	2,147	-0.005	0.076	-0.036	-0.002	0.026
UE	2,147	-0.009	0.03	-0.006	0	0.001
Loss	2,147	0.252	0.434	0	0	1
Size	2,147	4.875	0.925	4.349	4.887	5.468
Market-to-Book	2,147	3.113	7.959	1.374	1.927	3.065
Leverage	2,147	5.203	5.618	0.439	2.820	9.795
Persistence	2,147	0.681	0.675	0.158	0.529	1.123
Beta	2,147	0.294	1.386	0	0.328	0.654
Post	2,147	0.457	0.498	0	0	1
Timing: Treatment to First EA (in days)	599	305.5	245.7	132	243	375

Table 2 presents descriptive statistics for the variables used in the limited, full, and triennial inspection analyses. We provide detailed variable definitions in Appendix B. We include observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample), so the same firm enters multiple times (see Table 1). We truncate all continuous variables, except *UE*, at 1% and 99% by fiscal year. *UE* is truncated at 2.5% and 97.5% by fiscal year. Panel A presents descriptive statistics for firms with annually-inspected auditors. The sample includes 40,766 firm-year observations from the treatment (i.e., firms with domestic Big-Four or Tier-Two auditors) and control samples (i.e., U.S. cross-listed firms with non-U.S. Big-Four or non-U.S. Grant Thornton auditors). Panel B presents descriptive statistics for firms with triennially-inspected auditors. The sample includes 2,147 firm-year observations. The last row in each panel provides the average number of days from the respective cutoff date (end of fieldwork or inspection report release) to the (treated) firm's first earnings announcement.

Table 3: Changes in Reporting Credibility around the Introduction of the PCAOB Inspection Regime

Panel A: Analysis combining Limited and Full Inspections and all Measurement Cutoff Dates

Dependent Variable: *CAR*	**(1)** **OLS**	**(2)** **OLS**	**(3)** **Robust**	**(4)** **Perc. OLS**	**(5)** **Robust**
UE×Post×Treated		**0.581***	**0.677*****	**0.018****	**0.788*****
		(1.698)	**(2.974)**	**(2.148)**	**(3.478)**
UE	1.377***				
	(3.780)				
UE×Loss	-1.043***	-1.072***	-0.806***	-0.027***	-0.760***
	(-5.541)	(-5.321)	(-6.604)	(-5.160)	(-6.225)
UE×Size	-0.064	-0.019	0.007	-0.002	-0.023
	(-1.291)	(-0.321)	(0.168)	(-1.474)	(-0.609)
UE×M2B	0.078**	0.067**	0.056**	0.001*	0.054**
	(2.353)	(1.992)	(2.525)	(1.675)	(2.394)
UE×Leverage	-0.040***	-0.044***	-0.031***	-0.001***	-0.024**
	(-2.819)	(-3.001)	(-2.953)	(-2.867)	(-2.295)
UE×Persistence	0.032	0.004	0.024	0.001	-0.034
	(0.233)	(0.031)	(0.277)	(0.316)	(-0.406)
UE×Beta	0.438***	0.358**	0.420***	0.016***	0.301***
	(2.967)	(2.270)	(4.150)	(4.218)	(2.954)
UE×Post		-0.439	-0.415**	-0.021***	
		(-1.466)	(-2.033)	(-2.645)	
Firm Characteristics (Controls)	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Auditor & Country	Auditor & Country	Auditor & Country	Auditor & Country & Year-Quarter
Treatment Indicators (*Post*, *Treated*)	No	Yes	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes	Yes
UE×Fixed Effects	No	Yes	Yes	Yes	Yes
UE×Treatment Indicators (*Post*, *Treated*)	No	Yes	Yes	Yes	Yes
Observations	40,766	40,766	40,766	41,882	40,766
Zero-weighted observations			194		193
R-squared	0.022	0.028	0.046	0.059	0.055

Table 3 reports an analysis of changes in reporting credibility around the introduction of annual PCAOB inspections. Panel A reports results for the combined sample, which stacks the limited and full inspections analyses for each cutoff date (end of fieldwork and report release). Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). In Columns (2), (3), (4), and (5), we do not report the coefficients for fixed effects, for *UE* interactions with the fixed effects, or for variables that pertain only to the benchmark category (e.g., omitted country fixed effects). We suppress the *UE* main effect because we interact *UE* with country-fixed effects and hence the coefficient on *UE* captures the average ERC for the country whose indicator is omitted from the regression. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (defined at the global network level) and the auditor's country of domicile. In Column (1), we use Ordinary Least Squares (OLS) and exclude the PCAOB regime treatment indicators to provide a benchmark ERC regression. In Column (2), we introduce the regime change indicators and use OLS estimation. In Column (3), we estimate a robust regression (based on Stata's "rreg" command). When using the rreg command, Stata assigns observations with a Cook's Distance greater than one a weight of zero. We report the number of zero-weighted observations in the table footer. In Column (4), we use OLS and the percentile rank (on a scale of 0 to 1) of *UE*. In this regression, the sample size is larger because we truncate *UE* only at the 1% and 99% levels (like all other variables) prior to applying percentile rank. In Column (5), we estimate a robust regression including fixed effects for the year-quarter of the respective fiscal year-end (and its interactions with *UE*). We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Table 3 (continued)

Panel B: Separate Analyses for Limited and Full Inspections and each Cutoff Date

	(1)	(2)	(3)	(4)	(5)
	Stacked	**Limited Inspections**		**Full Inspections**	
Dependent Variable: *CAR*		**Fieldwork**	**Reports**	**Fieldwork**	**Reports**
Panel B(1) Main design:					
UE×Post×Treated	0.788***	0.336	0.566*	1.600***	1.149**
	(3.478)	(1.094)	(1.881)	(4.978)	(2.141)
Observations	40,766	9,308	9,799	11,833	9,826
Panel B(2) Dropped observations design:					
UE×Post×Treated	0.874***	0.414	0.513*	1.620***	2.145***
	(3.543)	(1.310)	(1.691)	(4.965)	(4.940)
Observations	38,511	8,775	9,191	11,017	9,528
Firm Characteristics	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter
Treatment Indicators	Yes	Yes	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes	Yes
UE×Fixed Effects	Yes	Yes	Yes	Yes	Yes
UE×Treatment Indicators	Yes	Yes	Yes	Yes	Yes

Panel B presents separate analyses for each inspection event (limited and full) and each measurement cutoff date (end of fieldwork and report release). Panel B(1) reports results for our analysis using the "Main design" as described in Figure 1. Panel B(2) reports results for our analysis using the "Dropped observations" design as described in Figure 1. Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). For brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or the interactions among these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression (based on Stata's "rreg" command). When using the rreg command, Stata assigns observations with a Cook's Distance greater than one a weight of zero. In Column (1), for ease of comparison, we report the combined analysis (i.e., Panel B(1) is the same as Panel A Column (5)). Panel B(2) Column (1) presents an analogous coefficient based on the "Dropped observations" design. In Column (2), we examine the changes in ERCs following fieldwork completion for limited inspections. In Column (3), we examine the changes in ERCs following inspection report releases for limited Inspections. In Column (4), we examine the changes in ERCs following fieldwork completion for full inspections. In Column (5), we examine the changes in ERCs following inspection report releases for full inspections. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Table 3 (continued)

Panel C: Combined Analysis Allowing for Nonlinearities in ERCs

Dependent Variable: *CAR*	(1)	(2)	(3)	(4)
UE×Post×Treated		**0.941*****	**2.012*****	**2.177*****
		(3.589)	**(4.728)**	**(5.094)**
UE	2.303***			
	(9.244)			
Nonlinear	-24.46***			
	(-11.861)			
UE×Loss	-0.491***			
	(-4.158)			
UE×Loss×Post×Treated		-0.801		-0.901
		(-1.518)		(-1.638)
Nonlinear×Post×Treated			-78.97***	-73.22***
			(-4.554)	(-4.020)
Firm Characteristics	Yes	Yes	Yes	Yes
Fixed Effects	No	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter
Treatment Indicators	No	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes
UE×Fixed Effects	No	Yes	Yes	Yes
UE×Treatment Indicators	No	Yes	Yes	Yes
Loss & *UE×Loss* interacted with Treatment Indicators	No	Yes	No	Yes
Nonlinear interacted with Treatment Indicators	No	No	Yes	Yes
Observations	40,766	40,766	40,766	40,766
Zero-weighted observations	187	200	191	194
R-squared	0.041	0.056	0.073	0.074

Panel C reports results for the combined analysis allowing for nonlinearities in the estimation of the ERC. We add *Nonlinear* (i.e., *UE*×|*UE*|) and its interactions to Eq. (1), which amounts to introducing a cubic term. We regress *CAR* on *UE*, indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). As in Panels A and B, we report only the coefficients of interest. Controls include *Nonlinear*, *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Detailed variable definitions are in Appendix B. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and the year-quarter of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In all columns, we estimate robust regressions using Stata's "rreg" command and report the number of zero-weighted observations in the table footer. In Column (1), we exclude the regime change indicators to provide a benchmark ERC regression including *Nonlinear*. In Column (2), we include interactions between the treatment indicators and *Loss* and *UE×Loss* to separate the treatment effects for loss firms. In Column (3), we interact *Nonlinear* with the treatment indicators to allow nonlinearities to change as a result of the new regime. In Column (4), we include interactions between the treatment indicators and *Loss* and *UE×Loss* as well as *Nonlinear*, in essence combining the models in Columns (2) and (3). We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors as described in Panels A and B of this table.

Table 4: Tests for Other Concurrent Changes in the Information Environment around the Introduction of the PCAOB

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Dependent Variable:	***UE***	***Forecast***	***Timeliness***	***Relative Information***	***Scaled Raw Accruals***	***Earnings Guidance***	***Guidance Bundle***
Panel A: Profit & Loss Firms							
Post×Treated	-0.002***	0.000	0.007	0.016***	-0.002	0.003	0.002
	(-3.126)	(0.149)	(0.922)	(3.307)	(-0.548)	(0.175)	(0.145)
Panel B: Profit Firms only (*Loss* interacted with Treatment Indicators)							
Post×Treated	-0.001**	0.000	0.003	0.026***	0.003	-0.021	-0.010
	(-2.285)	(0.227)	(0.366)	(5.253)	(0.876)	(-1.234)	(-0.705)
Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter
Treatment Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	40,766	40,353	40,329	40,298	38,480	40,766	40,766

Table 4 presents tests for other concurrent changes in the information environment around the introduction of the PCAOB inspection regime. In Panel A, the indicators for PCAOB inspection (i.e., *Post* and *Treated*) estimate the effect for profit and loss firms. In Panel B, we estimate the treatment effects separately for profit and loss firms, and report the effects for profit firms. In Column (1) [(2), (3), (4), (5), (6), (7)], we regress *UE* [*Forecast*, *Timeliness*, *Relative Information*, *Scaled Raw Accruals*, *Earnings Guidance*, *Guidance Bundle*] on indicators for PCAOB inspection (i.e., *Post* and *Treated*), controls, and fixed effects. In all columns, for brevity, we do not report coefficients for the control variables, fixed effects, and treatment indicator main effects. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (at the global network level), the firm's country of domicile, and the year-quarter of the respective fiscal year-end. In all columns, we report OLS regressions. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Table 5: Are the Results Driven by Changes in Reporting Incentives or Other Provisions of the Sarbanes-Oxley Act?

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
	Reporting Incentives		**Sarbanes-Oxley Act**				
Dependent Variable: *CAR*	**Non-AA clients**	**Only AA clients**	**Non-Accelerated**	**Only Accelerated**	**Excluding 404[b]**	**Only 404[b]**	**Controlling for SOX**
Panel A: Profit & Loss Firms							
UE×Post×Treated	**0.854*****	**0.332+**	**1.094*****	**0.787*****	**0.791*****	**0.028++**	**0.690*****
	(3.600)	**(1.098)**	**(3.384)**	**(3.244)**	**(3.158)**	**(0.090)**	**(2.835)**
UE×SOX404b							0.307
							(1.536)
UE×SOX302a							-0.911***
							(-3.268)
Panel B: Profit Firms only (*Loss* interacted with Treatment Indicators)							
UE×Post×Treated	**1.030*****	**0.492**	**1.139****	**0.871*****	**0.923*****	**0.234**	
	(3.662)	**(1.415)**	**(2.570)**	**(3.102)**	**(3.153)**	**(0.632)**	
Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter
Treatment Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UE×Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UE×Treatment Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	34,736	9,795	9,684	34,847	24,867	19,664	40,766

Table 5 presents sensitivity analyses examining the role of changes in reporting incentives and other provisions of SOX. In Panel A, the indicators for PCAOB inspection (i.e., *Post* and *Treated*) and *UE* estimate the effect for profit and loss firms. In Panel B, we separately estimate the treatment effects for loss and profit firms, and report the effect for profit firms only. Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on *UE*, indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). In all columns, for brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or the interactions among these variables. Controls include *Loss, Size, M2B, Leverage, Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (at the global network level), the firm's country of domicile, and the year-quarter of the respective fiscal year-end. In all columns, we estimate a robust regression (based on Stata's "rreg" command). In Columns (1) & (2), we partition the treatment sample based on whether the firm was audited by Arthur Andersen in 2000 or 2001, as indicated by the column headings. In Columns (3) & (4), we partition the treatment sample based on whether a firm-year observation is classified as an accelerated filer in Audit Analytics, as indicated by the column headings. In Columns (5) & (6), we partition the treatment sample based on whether a firm-year has an auditor internal control opinion (effective, adverse, or disclaimer) in Compustat, as indicated by the column headings. In Column (7), we include the indicator variables *SOX404b* and *SOX302a* and their interactions with *UE* as additional controls. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. + and ++ indicate significance (two-sided) at the 10% and 5% levels, respectively, for tests of coefficient magnitudes relative to the adjacent column on the left. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Table 6: Changes in Reporting Credibility around the Introduction of PCAOB Triennial Inspections

Panel A: Combined Analysis for all Measurement Cutoff Dates

Dependent Variable: ***CAR***	**(1) OLS**	**(2) Robust**	**(3) OLS**	**(4) Robust**	**(5) OLS**	**(6) Robust**
UE×*Post*	0.357*	0.110	2.013***	0.877***	1.484**	0.716***
	(1.748)	(1.259)	(3.281)	(3.356)	(2.263)	(2.615)
Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Auditor & Year-Quarter	Auditor & Year-Quarter	Auditor & Year-Quarter	Auditor & Year-Quarter	Auditor & Year-Quarter	Auditor & Year-Quarter
Treatment Indicator (*Post*)	Yes	Yes	Yes	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
UE×Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Loss & *UE*×*Loss* interacted with Treatment Indicator	No	No	Yes	Yes	Yes	Yes
Nonlinear interacted with Treatment Indicator	No	No	No	No	Yes	Yes
Observations	2,147	2,147	2,147	2,147	2,147	2,147
Zero-weighted observations		125		123		127
R-squared	0.296	0.676	0.305	0.677	0.309	0.683

In Table 6, we report an analysis of changes in reporting credibility around the introduction of triennial PCAOB inspections. Panel A reports results for the combined analysis using both cutoff dates (see Appendix A, Panel B). Following Eq. (2), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), an indicator for PCAOB inspection (i.e., *Post*), control variables, fixed effects, the interactions of *UE* with the control variables, the fixed effects and the treatment indicator (as shown in the table footer). Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor and the year-quarter of the respective fiscal year-end. In Columns (1), (3), & (5), we use Ordinary Least Squares ("OLS"). In Columns (2), (4), (6), we estimate a robust regression (based on Stata's "rreg" command). When using the rreg command, Stata assigns observations with a Cook's Distance greater than one a weight of zero. We report the number of zero-weighted observations in the table footer. In Columns (1) & (2), we estimate the treatment effect for profit and loss firms. In Columns (3) & (4), we allow the treatment effect to vary across profit and loss firms (by including treatment indicator interactions for *Loss* as noted in the table footer). In Columns (5) & (6), we also allow for nonlinearities in the ERC estimation and for changes in these nonlinearities around the regime shift by including additional interactions for *Nonlinear* as noted in the table footer. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Table 6: Changes in Reporting Credibility around the Introduction of PCAOB Triennial Inspections

Panel B: Sensitivity Analyses

Dependent Variable: ***CAR***	**(1)** **Fieldwork**	**(2)** **Report**	**(3)** **Dropped Observations**	**(4)** **Controlling for SOX**
UE×Post	**0.835****	**1.767*****	**2.472*****	**0.857*****
	(2.073)	**(4.346)**	**(3.952)**	**(3.522)**
UE×SOX404b				0.330
				(1.487)
UE×SOX302a				-0.900**
				(-2.154)
Firm Characteristics	Yes	Yes	Yes	Yes
Fixed Effects	Auditor & Year-Quarter	Auditor & Year-Quarter	Auditor & Year-Quarter	Auditor & Year-Quarter
Treatment Indicator (*Post*)	Yes	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes
UE×Fixed Effects	Yes	Yes	Yes	Yes
Loss & *UE×Loss* interacted with Treatment Indicator	Yes	Yes	Yes	Yes
Observations	1,229	918	885	2,147
Zero-weighted observations	69	73	71	120
R-squared	0.640	0.760	0.794	0.675

Panel B presents several sensitivity analyses. Following Eq. (2), we regress cumulative abnormal returns (*CAR*) on *UE*, an indicator for PCAOB inspection (i.e., *Post*), control variables, fixed effects, the interactions of *UE* with the control variables, the fixed effects, and the treatment indicator (as shown in the table footer). Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Detailed variable definitions are in Appendix B. We include fixed effects for the auditor and the year-quarter of the respective fiscal year-end. We estimate robust regressions (based on Stata's "rreg" command) and report the number of zero-weighted observations in the table footer. In all columns, we estimate the treatment effect for profit and loss firms separately by including additional interactions as noted in the table footer. We report the coefficient of interest for profit firms only. In Column (1), we examine changes in ERCs using the fieldwork cutoff date (i.e., *Post* equals 1 if the firm's fiscal year-end is at least 30 days after the date of fieldwork completion—see Appendix A, Panel B). In Column (2), we examine changes in ERCs using the report release as the cutoff date (i.e., *Post* equals 1 if the firm's earnings announcement is after the report-release date). In Column (3), we exclude the interim period between the end of fieldwork and the release of the inspection report (i.e., *Post* indicates that the firm's earnings announcement is after the report-release date and that the pre-period is measured prior to the start of fieldwork). In Column (4), we re-estimate the main model (from Panel A, Column (4)) adding the indicator variables *SOX404b* and *SOX302a* and their interactions with *UE* as additional controls. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5% and, 1% levels, respectively. We calculate firm-level-clustered standard errors as described in Panel A.

Table 7: Falsification Tests Shifting the Timing of the Introduction of the PCAOB Inspection Regime

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Dependent Variable: ***CAR***	**Lag 2**	**Lag 1**	**Proper date**	**Plus 1**	**Plus 2**
Panel A: Full Inspection Fieldwork					
UE×*Post*×*Treated*	-0.579	0.322	1.600***	1.008**	0.342
	(-1.449)	(0.868)	(4.978)	(2.320)	(1.037)
Observations	10,631	11,196	11,833	12,051	12,041
Panel B: Full Inspection Report Release					
UE×*Post*×*Treated*	-0.347	0.980**	1.149**	-0.215	-1.183***
	(-0.633)	(2.047)	(2.141)	(-0.440)	(-2.978)
Observations	9,047	9,616	9,826	9,924	9,849
Firm Characteristics	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter
Treatment Indicators	Yes	Yes	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes	Yes
UE×Fixed Effects	Yes	Yes	Yes	Yes	Yes
UE×Treatment Indicators	Yes	Yes	Yes	Yes	Yes

Table 7 presents falsification tests (counterfactually) shifting the timing of the introduction of the PCAOB inspection regime. Panel A reports results for this analysis around the first full inspections using the fieldwork completion as the cutoff date. Panel B reports results for this analysis around the first full inspections using the report release as the cutoff date. Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). For brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or the interactions among these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (at the global network level), the firm's country of domicile, and the year-quarter of the respective fiscal year-end. In all columns, we estimate a robust regression (based on Stata's "rreg" command). In Column (1) for Panel A [Panel B], we (counterfactually) code the *Post* variable to equal one beginning two years prior to the actual fieldwork end date [report release date]. In Column (2) for Panel A [Panel B], we code the *Post* variable to equal one beginning one year prior to the actual fieldwork end date [report release date]. In Column (3) for Panel A [Panel B], we code the *Post* variable to equal one on the true fieldwork end date [report release date]. In Column (4) for Panel A [Panel B], we code the *Post* variable to equal one beginning one year after the actual fieldwork end date [report release date]. In Column (5) for Panel A [Panel B], we code the *Post* variable to equal one beginning two years after the actual fieldwork end date [report release date]. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Table 8: Changes in Abnormal Trading Volume around 10-K filings after the Introduction of the PCAOB Inspection Regime

Dependent Variable: *Abnormal 10-K Volume*	**(1)** **OLS**	**(2)** **Robust**	**(3)** **OLS**	**(4)** **OLS**
Post×Treated	**0.088****	**0.044*****	**0.097*****	**0.062***
	(2.552)	**(2.692)**	**(2.748)**	**(1.655)**
Size	0.016**	0.024***	-0.024	-0.023
	(2.476)	(7.480)	(-0.990)	(-0.967)
M2B	-0.009***	-0.009***	-0.002	-0.002
	(-3.457)	(-7.307)	(-0.359)	(-0.339)
Leverage	0.009***	0.007***	0.012**	0.012**
	(5.256)	(7.717)	(2.089)	(2.091)
Beta	0.076***	0.050***	0.077***	0.074***
	(5.460)	(7.202)	(3.679)	(3.503)
Loss	-0.075***	-0.047***	-0.061**	-0.061**
	(-4.126)	(-5.254)	(-2.219)	(-2.219)
Filing Delay after FYE	0.004***	0.002***	0.003***	0.003***
	(7.920)	(10.738)	(5.065)	(5.068)
Filing Delay after EA	-0.006***	-0.004***	-0.005***	-0.005***
	(-15.081)	(-19.193)	(-7.570)	(-7.509)
Analyst Following	-0.002*	-0.002***	0.000	-0.000
	(-1.862)	(-2.781)	(0.020)	(-0.110)
SOX404b				0.045
				(1.491)
SOX302a				0.115**
				(2.221)
Fixed Effects	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Firm & Year-Quarter	Firm & Year-Quarter
Treatment Indicators	Yes	Yes	Yes	Yes
Observations	68,830	68,830	68,830	68,830
Zero-weighted observations		2,261		
R-squared	0.039	0.059	0.317	0.317

Table 8 presents results for an analysis of changes in abnormal trading volume around 10-K filings after the introduction of the PCAOB inspection regime. Following Eq. (3), we regress *Abnormal 10-K Volume* on indicators for PCAOB inspections (i.e., *Post* and *Treated*), control variables, and fixed effects (as indicated in the table footer). We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (at global network level), the firm's country of domicile, the year-quarter of the respective fiscal year-end, and the firm (as indicated in the table footer). In Column (1), we use Ordinary Least Squares ("OLS"). In Column (2), we estimate a robust regression (based on Stata's "rreg" command). When using the rreg command, Stata assigns observations with a Cook's Distance greater than one a weight of zero. We report the number of zero-weighted observations in the table footer. In Column (3), we repeat Column (1), but substitute firm-fixed effects for auditor and country fixed effects. In Column (4), we use OLS and include the indicator variables *SOX404b* and *SOX302a*. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Internet Appendix:

**Public Audit Oversight and Reporting Credibility:
Evidence from the PCAOB Inspection Regime**

This appendix provides supplemental descriptive information that supports our manuscript "Public Audit Oversight and Reporting Credibility: Evidence from the PCAOB Inspection Regime."

Figure IA1: Scatterplot of Cumulative Abnormal Return on Unexpected Earnings

These figures provide a scatter plot of Cumulative Abnormal Returns (*CAR*) on Unexpected Earnings (*UE*) for both untrimmed *UE* and a variety of levels of trimming (1%, 2.5%, and 5% in each tail). These figures illustrate the extreme distribution of *UE*, especially for negative realizations. For instance, in the untrimmed scatter plot, some of the most extreme realizations of *UE* are between five and ten times as large as the firm's stock price. As we increase the level of trimming from 1% to 5% the distribution becomes significantly less skewed. In our primary analyses, we choose an intermediate trimming level of 2.5%.

Figure IA2: Fractional Polynomial Regression Plots for Loss Firms

As discussed in the paper, loss firms are expected to have fundamentally different ERCs due to the lower persistence of losses. Thus, we do not expect loss firms to exhibit the same treatment effect as profit firms. This figure illustrates that loss firms have a fundamentally different ERC shape. In Figure IA2, we plot fractional polynomial regressions of cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*) using Eq. (1) but including only non-profitable (loss) firms from the treatment group in the pre-inspection (the dashed line) and the post-inspection (the solid line) periods. As expected, the shape of the function for loss firms is generally flat, consistent with the low persistence of loss observations.

Figure IA3: Fractional Polynomial Regression Plots for Triennial Firms

In Figure IA3, we provide the analogue of Figure 3 in the paper. We plot fractional polynomial regressions of cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*) using Eq. (1) including only profitable, triennially-inspected firms in the pre-inspection (the dashed line) and the post-inspection (the solid line) periods. This figure illustrates a significant upward shift in the response to positive *UE*, similar to the plot for annually-inspected firms in Figure 3. The plot shows no increase in the response to negative values of *UE* (but it is also not benchmarked against control firms as our regression analysis).

Figure IA4: Historic Parallel Trends in ERCs

In Figure IA4, we graph historical trends in ERCs for treatment and control firms in calendar time. The purpose of this figure is to explore whether these trends have historically been parallel.

One potential concern with our analysis is the possibility that our treatment and control firms differ systematically, and would not have had similar trends in their ERCs in the absence of the PCAOB inspection regime (i.e., the parallel-trends assumption is violated). Our analysis is conducted in event time using a relatively short pre-period only. We therefore provide evidence on the historical trends of the ERCs for treatment and control firms for a longer time. We plot the pre-period trends using a ten-year calendar-time period from the early 1990s to the early 2000s. For comparison, we normalize the starting point of the figure to reflect the magnitude of the baseline ERC in Table 3 Panel A Column (1), 1.377. Throughout the entire period, the ERCs of the U.S. and non-U.S. firms change in a similar fashion, suggesting that the parallel-trends assumption is likely to be valid for our sample.

Figure IA5: Mapping Out of the Treatment Effect in Event-Time

In Figure IA5, we map out the estimated treatment effect in event time to graphically illustrate that the observed ERC increase occurs around the introduction of the PCAOB inspection regime. The figure illustrates that the difference in the ERC between treatment and control firms is insignificantly different from zero in the period leading up to the introduction of the inspection regime and then increases significantly in the period after the inspection regime (red dots indicate that the incremental coefficient is statistically significant). The timing of the response in this figure provides further comfort that the parallel-trends assumption is likely to be valid in our analysis.

Table IA1: Details on the Timing of the Adoption of other SOX Provisions (Sections 302 and 404)

Table IA1 presents details on the timing of the adoption of other SOX provisions relative to the timing of the fiscal year in which our sample of triennial firms were first treated by the PCAOB inspection regime. Panel A presents the timing of SOX 302 adoption relative to the fiscal year of the first PCAOB inspection using the *fieldwork* cutoff date. Gauging the overlap in the timing of the PCAOB triennial inspections and the implementation of SOX provision 302, we find that the timing of the first SOX 302 opinion coincides with the initial inspection year for only 2.5% of our sample. Panel B presents the timing of SOX 302 adoption relative to the fiscal year of the first PCAOB inspection using the *inspection report release* cutoff date. The timing of the first SOX 302 opinion coincides with the initial inspection year for only 2.7% of our sample.

Panel C presents the timing of SOX 404 adoption relative to the fiscal year of the first PCAOB inspection using the *fieldwork* cutoff date. The timing of the first SOX 404 opinion coincides with the initial inspection year for only 10.5% of our sample. Panel D presents the timing of SOX 404 adoption relative to the fiscal year of the first PCAOB inspection using the *inspection report release* cutoff date. The timing of the first SOX 404 opinion coincides with the initial inspection year for only 6.8% of our sample.

Overall, the tables show that there is minimal overlap between other SOX provisions and the treatment dates used for the PCAOB inspection regime, suggesting that the triennial inspection analysis is unlikely to be confounded by the concurrent adoption of other SOX provisions.

Table IA2: Descriptive Statistics for Abnormal Trading Volume Sample

Table IA2 presents descriptive statistics for the analysis of changes in abnormal trading volume around 10-K filings after the introduction of the PCAOB inspection regime (Manuscript Table 8). The sample size is much larger than for the ERC analysis (68,830) because we do not require analyst coverage. However, as before, the majority of the sample observations (89%) are from the treatment group. On average, *Abnormal 10-K Volume* is positive. At the median, a firm files its 10-K 83 days after the fiscal year-end and 36 days after the earnings announcement. Four analysts follow the median firm. Across the other control variables, the sample is comparable to the main ERC tests.

Internet Appendix: Figure 1 - Scatterplot of Cumulative Abnormal Return on Unexpected Earnings

Panel A: Scatterplot of Cumulative Abnormal Return on Untrimmed Unexpected Earnings

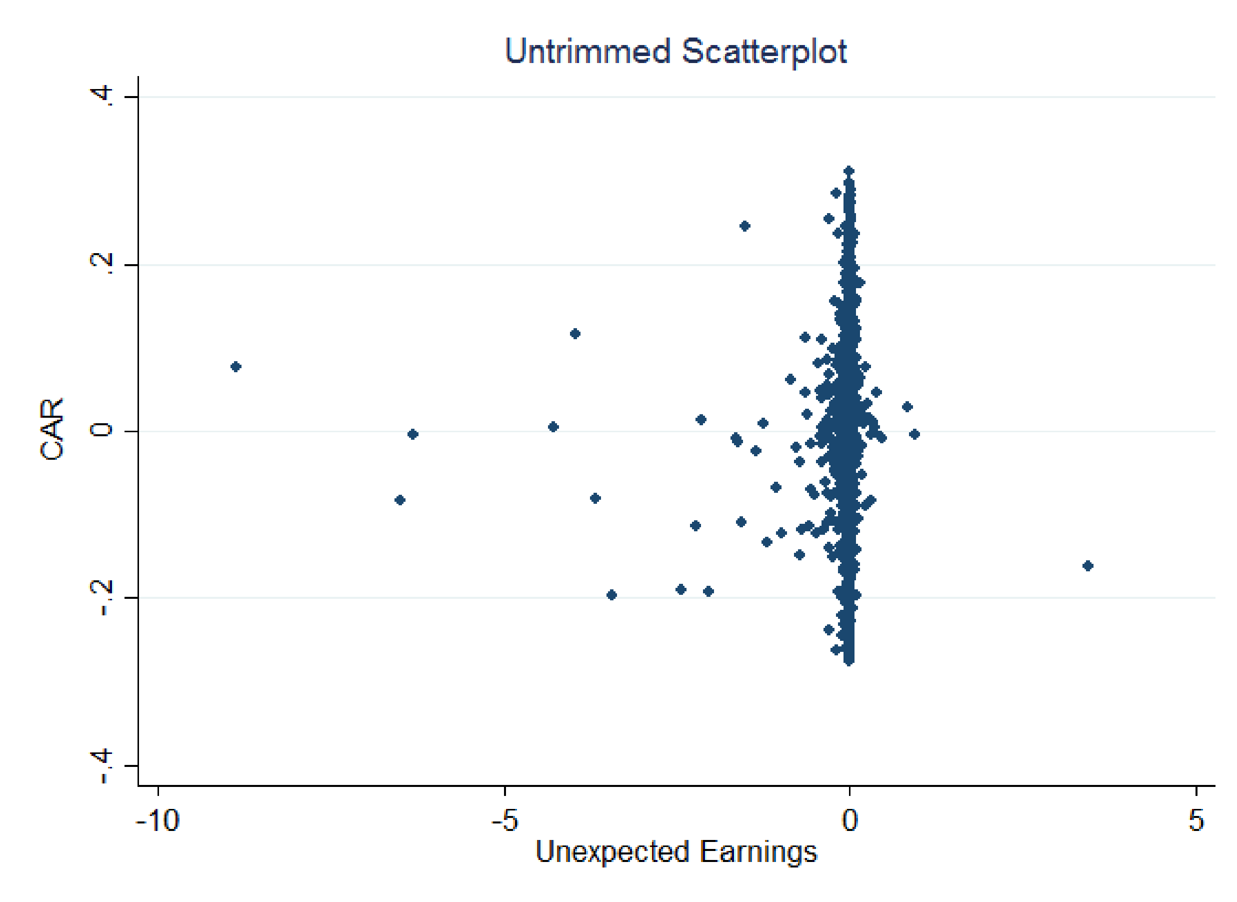
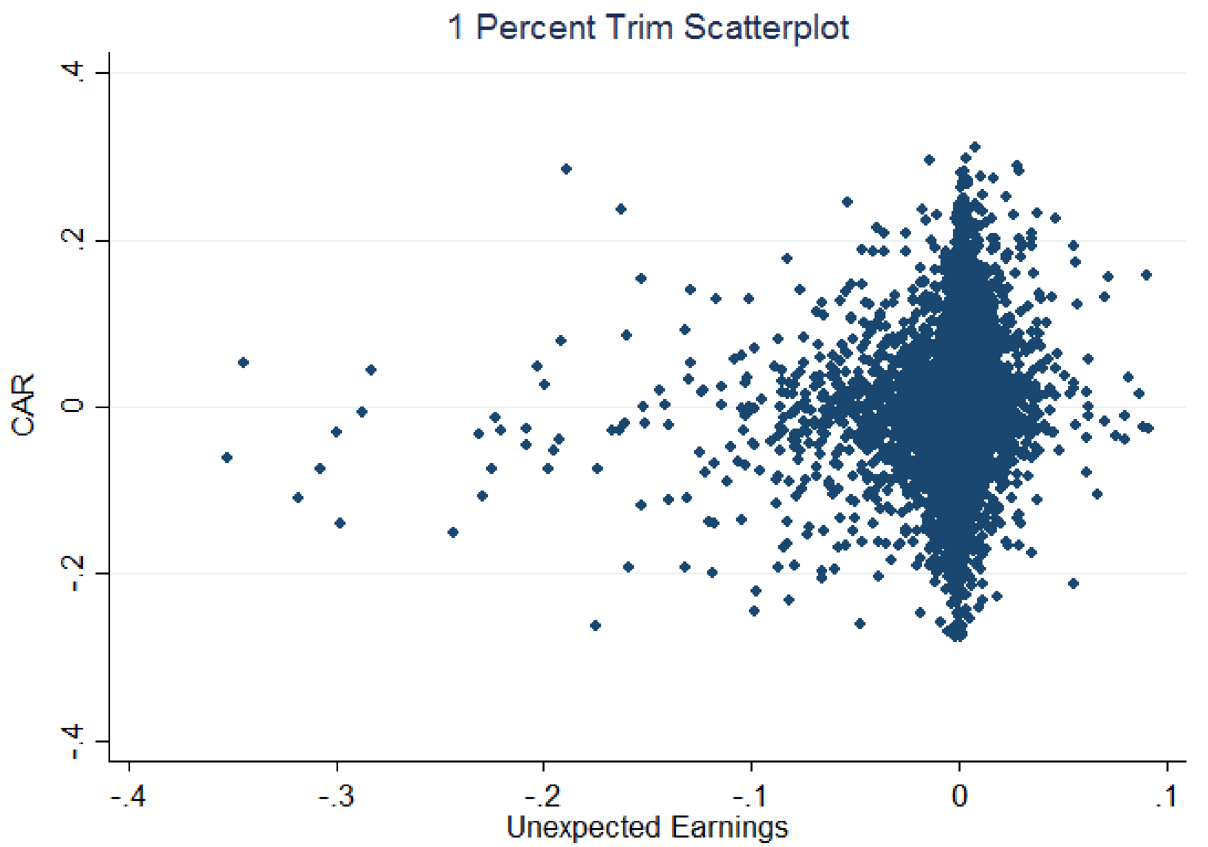


Figure IA1 includes scatterplots across various trimming thresholds for *Unexpected Earnings* (*UE*) along the x-axis and *Cumulative Abnormal Returns* (*CAR*) along the y-axis. Panel A presents a scatterplot of *CAR* on untrimmed *UE*. The sample comprises 42,544 observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample).

Internet Appendix: Figure 1 - Scatterplot of Cumulative Abnormal Return on Unexpected Earnings

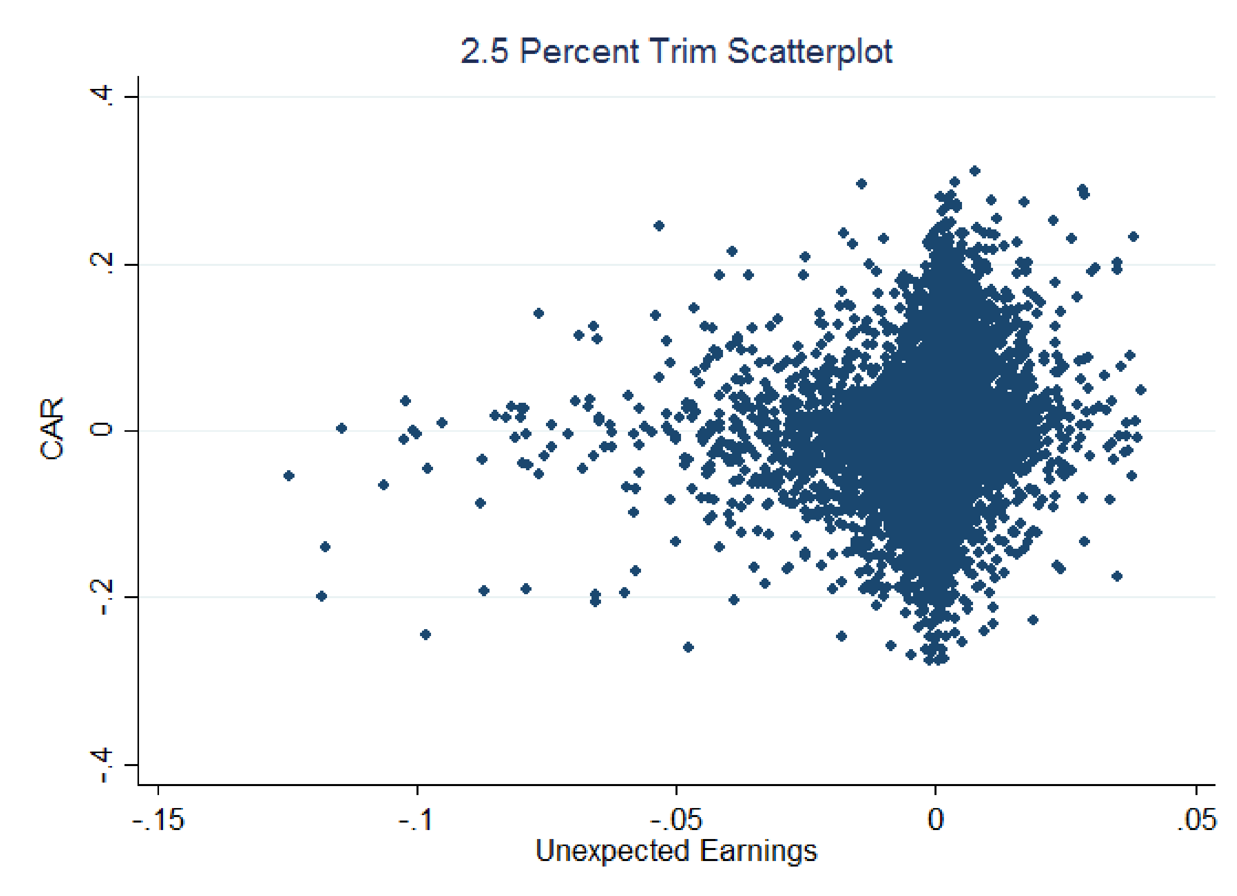
Panel B: Scatterplot of Cumulative Abnormal Return on 1% Trimmed Unexpected Earnings



Panel B presents a scatterplot of *CAR* on 1% and 99% trimmed *UE*. The sample comprises 41,882 observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample).

Internet Appendix: Figure 1 - Scatterplot of Cumulative Abnormal Return on Unexpected Earnings

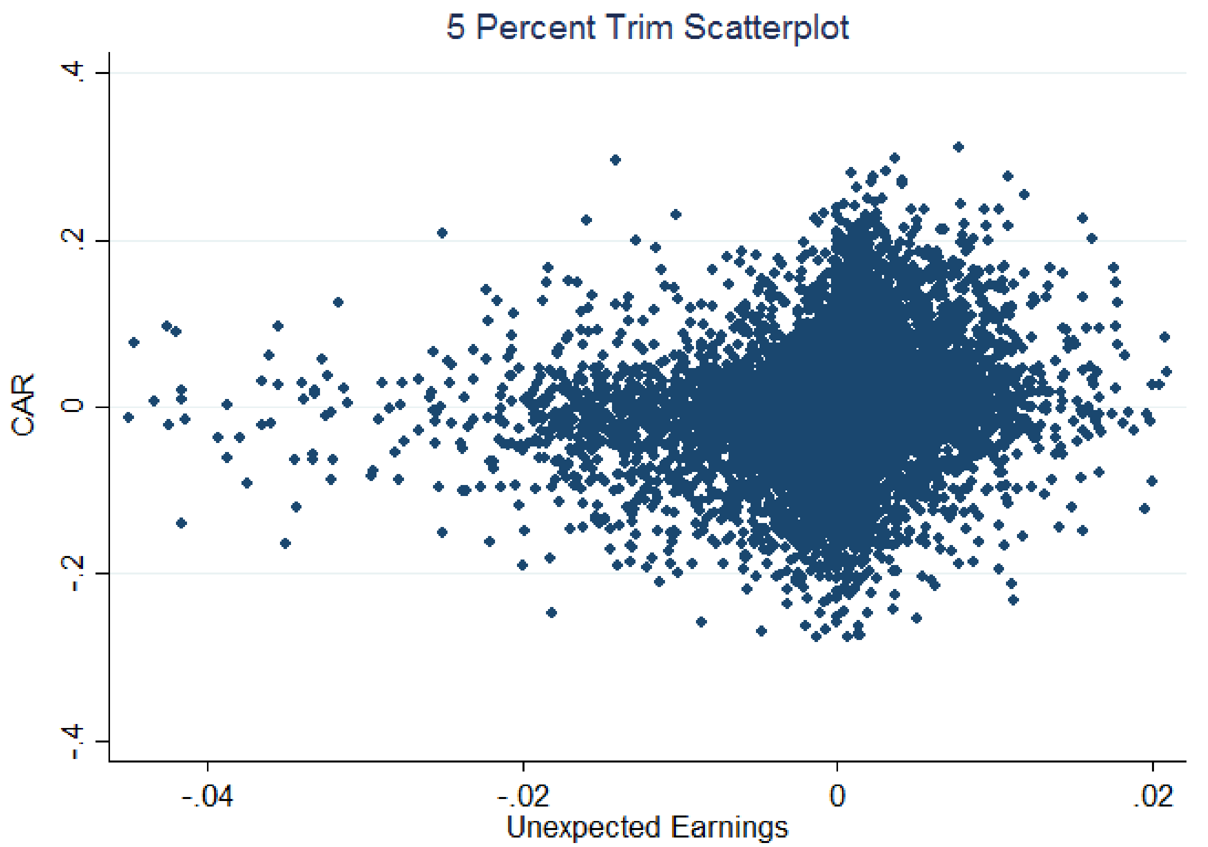
Panel C: Scatterplot of Cumulative Abnormal Return on 2.5% Trimmed Unexpected Earnings



Panel C presents a scatterplot of *CAR* on 2.5% and 97.5% trimmed *UE*. The sample comprises 40,766 observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample).

Internet Appendix: Figure 1 - Scatterplot of Cumulative Abnormal Return on Unexpected Earnings

Panel D: Scatterplot of Cumulative Abnormal Return on 5% Trimmed Unexpected Earnings



Panel D presents a scatterplot of *CAR* on 5% and 95% trimmed *UE*. The sample comprises 38,766 observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample).

Internet Appendix: Figure 2: Fractional Polynomial Regression Plots for Loss Firms

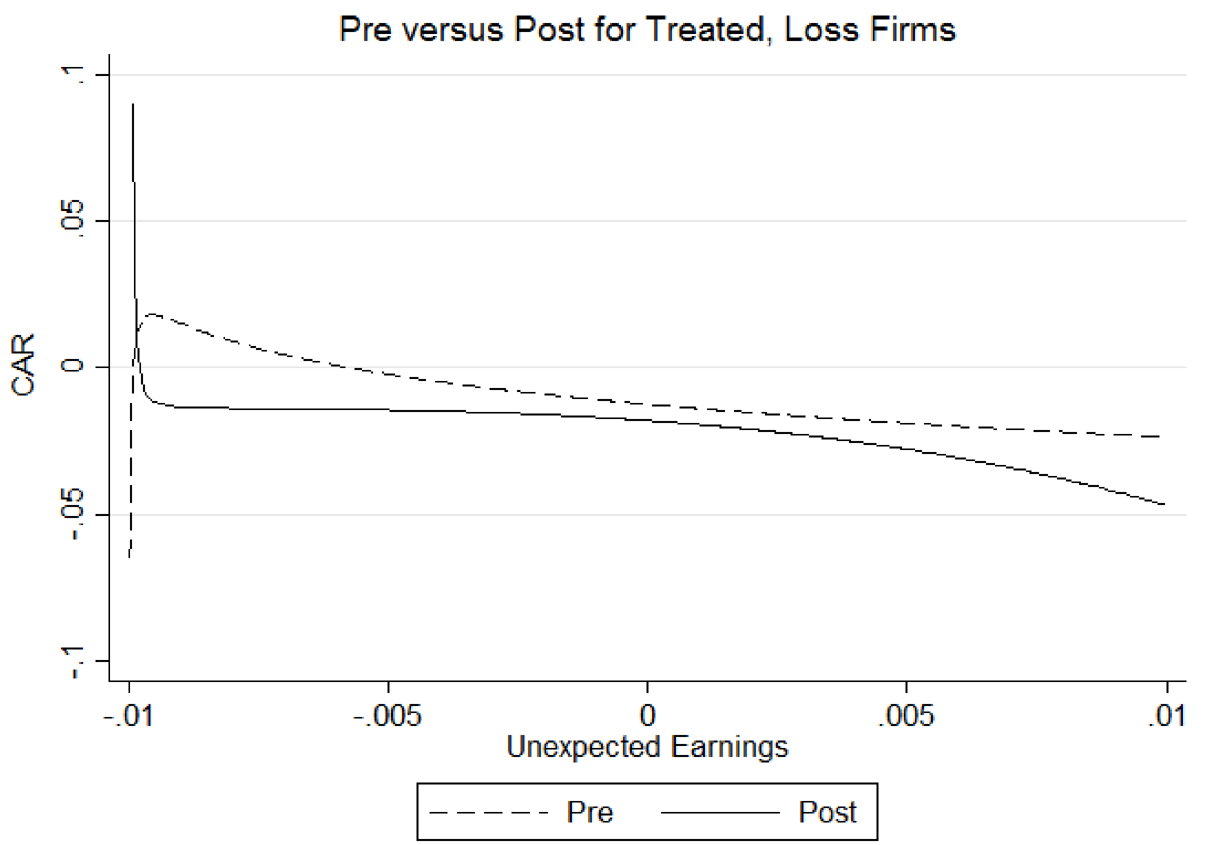


Figure IA2 presents plots of fractional polynomial regressions of cumulative abnormal returns (*CAR*) and unexpected earnings (*UE*) using Eq. (1). Fractional polynomial regressions provide flexible parameterization for continuous variables without predetermining the shape. The procedure searches over a set of possible polynomial functions for the model that best fits the data. We use Stata's 'fp' function, which by default allows for the following non-integer powers (-2, -1, -0.5, ln(x), 0.5, 1, 2, 3) along with repeated powers multiplied by ln(x). We include powers for unexpected earnings only, but include the full set of control variables. The sample comprises only treated loss firms (i.e., *Loss* = 1) from the combined analyses, provided unexpected earnings are within ±1% of price. The pre-inspection period is represented by the dashed line and the post-inspection period by the solid line. This sample comprises 5,226 observations, or 12.8% of the sample. We provide detailed variable definitions in Appendix B of the manuscript.

Internet Appendix: Figure 3: Fractional Polynomial Regression Plots for Triennial Firms

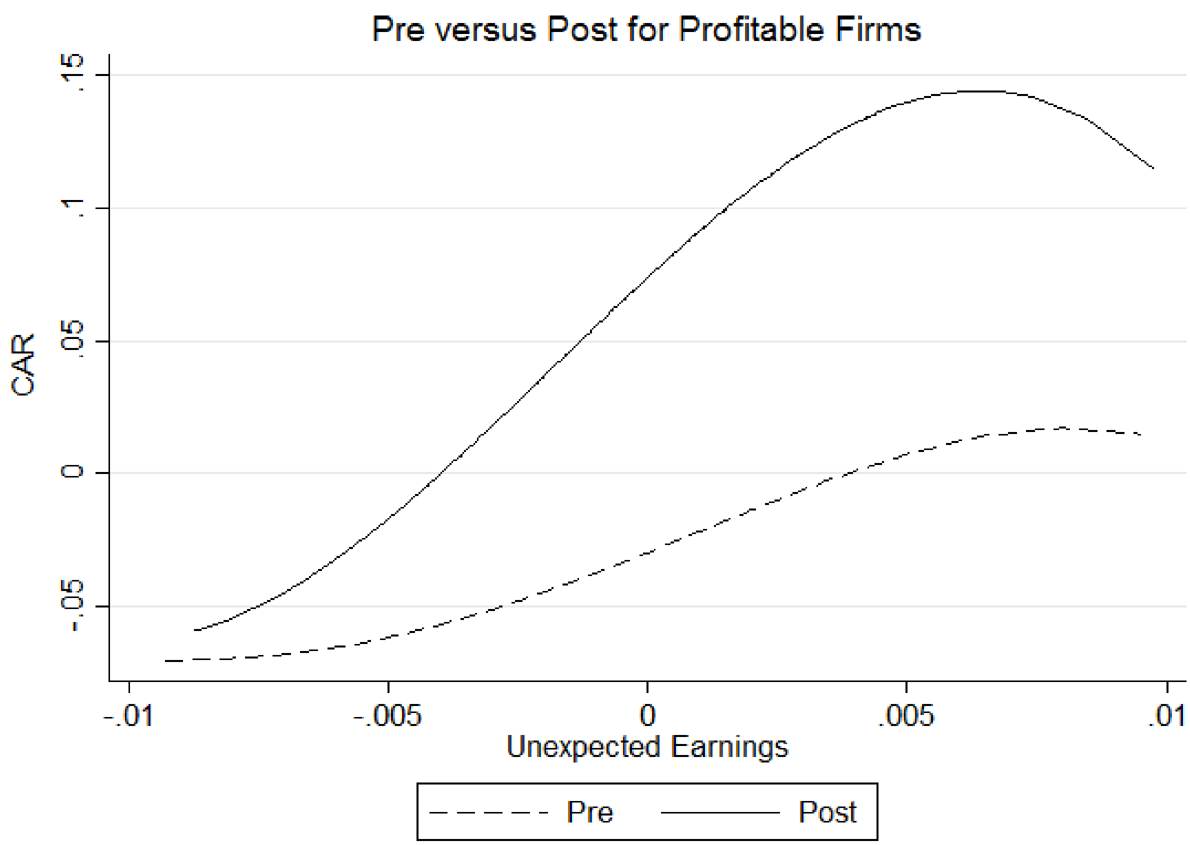


Figure IA3 presents plots of fractional polynomial regressions of cumulative abnormal returns (*CAR*) and unexpected earnings (*UE*) using Eq. (1). Fractional polynomial regressions provide flexible parameterization for continuous variables without predetermining the shape. The procedure searches over a set of possible polynomial functions for the model that best fits the data. We use Stata's 'fp' function, which by default allows for the following non-integer powers (-2, -1, -0.5, ln(x), 0.5, 1, 2, 3) along with repeated powers multiplied by ln(x). We include powers for unexpected earnings only, but include the full set of control variables. The sample comprises only treated profitable firms (i.e., *Loss* = 0) from the triennial inspection analyses, provided unexpected earnings are within ±1% of price. The pre-inspection period is represented by the dashed line and the post-inspection period by the solid line. This sample comprises 1,242 observations, or 57.8% of the sample. We provide detailed variable definitions in Appendix B of the manuscript.

Internet Appendix: Figure 4 - Historic Parallel Trends in ERCs

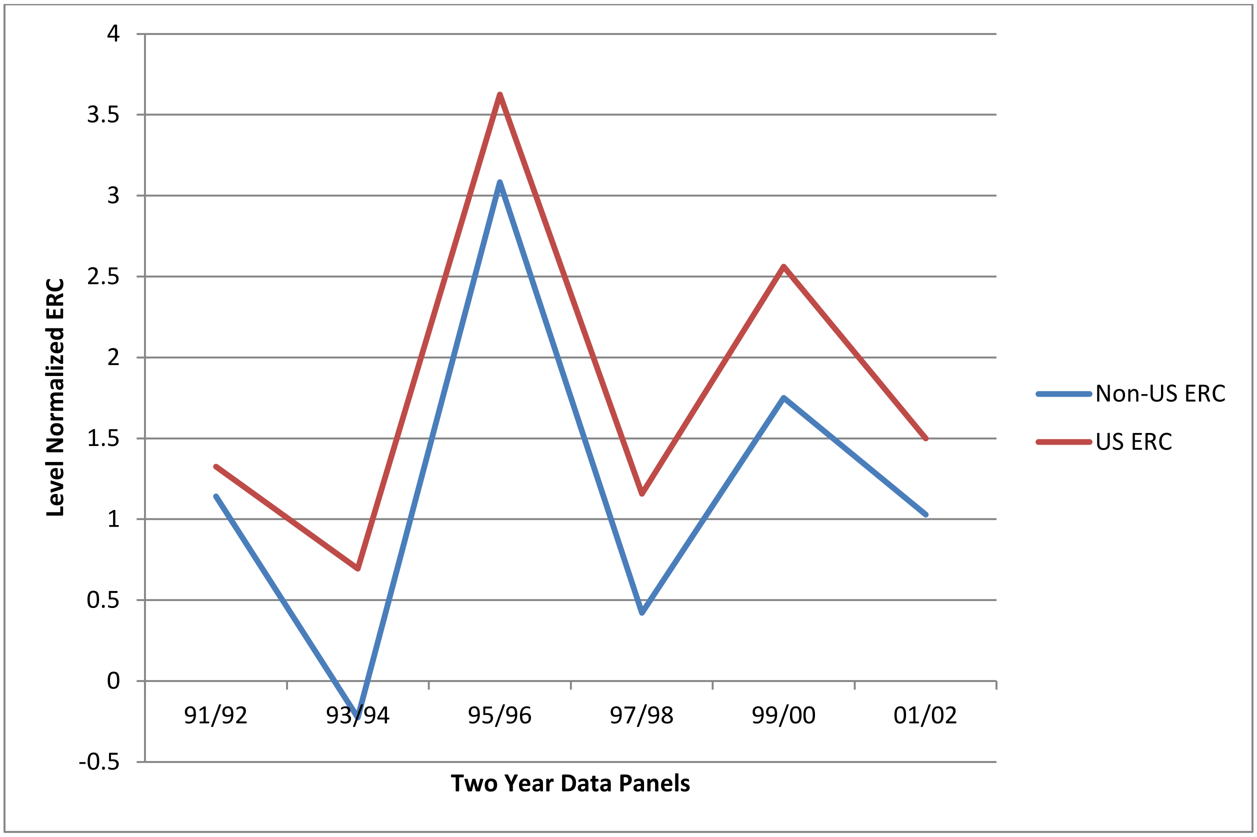


Figure IA4 presents historical trends in ERCs for U.S. firms audited by Big-Four and Tier-Two U.S. auditors and for non-U.S. firms audited by the Big Four and Grant Thornton from 1991-2002, estimating ERCs in two-year intervals in calendar time based on Eq. (1). The sample includes firms used in our primary analyses (i.e., Table 3 Panel A). Each red point on the graph represents the ERC regression coefficient for U.S. firms (i.e. *UE +UE× Treated*) from a robust regression (based on Stata's "rreg" command). Each blue point on the graph represents the ERC regression coefficient for non-U.S. firms (i.e. *UE*) from the same regression. To facilitate the interpretation of the graph and the comparison with our regressions reported in Table 3, we normalize the level of the ERCs so that the average of the non-U.S. ERC is equal to our baseline estimation in Table 3, Panel A. This normalization has no effect on the relative trends in ERCs and relative magnitudes of the estimates for non-U.S. and U.S. firms. We provide detailed variable definitions in Appendix B of the manuscript.

Internet Appendix: Figure 5 - Mapping Out of the Treatment Effect in Event-Time

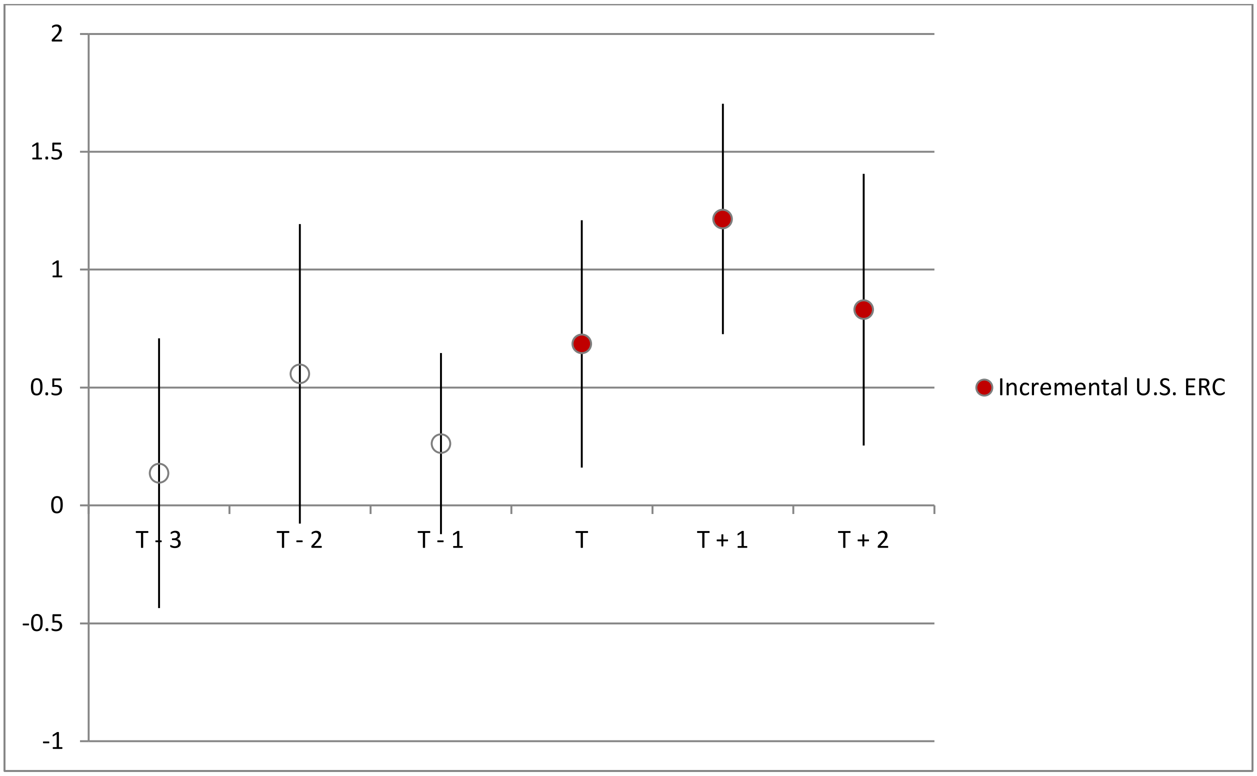


Figure IA5 presents trends in earnings response coefficients (ERCs), in event time, for firms audited by Big-Four and Tier-Two U.S. auditors and for cross-listed firms audited by the non-U.S. Big Four and Grant Thornton. The figure presents the incremental U.S. ERC for the combined sample, which stacks the limited and full inspection analyses for each cutoff date (i.e., the end of fieldwork and report release), using the dropped observation design and profitable firms only (i.e. *Loss* = 0). Each unshaded [red] dot on the graph represents an insignificant [significantly positive] ERC regression coefficient for U.S. firms (i.e. *UE*× *Treated*) from a robust regression (based on Stata's "rreg" command) estimation of Eq. (1). Each line bar spans two standard errors on either side of the coefficient. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression. We provide detailed variable definitions in Appendix B of the manuscript.

Internet Appendix: Table 1 – Details on the Timing of the Adoption of other SOX Provisions (Sections 302 and 404)

Panel A: Timing of SOX 302 Adoption Relative to the Fiscal Year of Treatment for Small Auditors using the Fieldwork Cutoff Date

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Fiscal Year of Treatment	**Fiscal Year of Treatment minus Fiscal Year of First SOX 302 Opinion**									**No 302 Obs**	**Total**
	-3	**-2**	**-1**	**0**	**1**	**2**	**3**	**4**	**5**		
2004	1	3	2	1	4	22				17	50
2005		-	2	2	3	4	18			20	49
2006			9	7	**3**	4	16	108		159	306
2007				1	-	-	2	-	14	16	33
Total	1	3	13	11	10	30	36	108	14	212	438

SOX Adoption Year	Firm Count
2002	162
2003	24
2004	10
2005	7
2006	12
2007	11
No Obs	212
Total	438

Table IA1 compares the timing of the PCAOB inspection for triennially inspected auditor client firms with the issuer's adoption of other SOX provisions. Panel A compares the timing of the fiscal year when *Post* = 1 for the first time relative to the timing of the firm's first observed SOX 302 opinion using the end of the inspection fieldwork as the cutoff date (i.e., *Post* equals one if a firm's fiscal year-end is after the auditor-specific fieldwork end date plus 30 days, and zero otherwise). Each row separates the firms by fiscal year—indicated in Column (1)—where the treatment indicator, *Post*, switches from 0 to 1. Each column separates the firms by timing distance—indicated in Columns (2) through (10)—to the fiscal year of the firm's first SOX 302 opinion. Each cell counts the number of unique firms for which the firm's initial PCAOB inspection overlaps with the firm's first SOX Section 302 opinion. For instance, the emphasized cell shows that three firms had *Post* coded as 1 for the first time in 2006, but issued their first SOX 302 opinion one year earlier in 2005. Column (11) enumerates the number of firms with no SOX 302 opinion in our sample window. We indicate the SOX adoption year by color and include a reconciliation to the total. Note in Column (5), 11 firms of 438 total firms, about 2.5%, adopt SOX 302 at the same time as the initial PCAOB inspection.

Internet Appendix: Table 1 – Details on the Timing of the Adoption of other SOX Provisions (Sections 302 and 404)

Panel B: Timing of SOX 302 Adoption Relative to the Fiscal Year of Treatment for Small Auditors using the Inspection Report Release as the Cutoff Date

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fiscal Year of Treatment	**Fiscal Year of Treatment minus Fiscal Year of First SOX 302 Opinion**							**No 302 Obs**	**Total**
	-1	**0**	**1**	**2**	**3**	**4**	**5**		
2005	1	-	3	4	11			26	45
2006	2	6	**3**	2	8	48		46	115
2007		3	1	3	2	8	64	78	159
2008			1	-	-	-	-	16	17
Total	3	9	8	9	21	56	64	166	336

SOX Adoption Year	**Firm Count**
2002	123
2003	20
2004	7
2005	6
2006	8
2007	6
No Obs	166
Total	336

Panel B compares the timing of the fiscal year when *Post* = 1 for the first time relative to the timing of the firm's first observed SOX 302 opinion using the inspection report release as the cutoff date (i.e., *Post* equals one if a firm's fourth quarter earnings announcement falls on or after the release date of the inspection report, and zero otherwise). Each row separates the firms by fiscal year—indicated in Column (1)—where the treatment indicator, *Post*, switches from 0 to 1. Each column separates the firms by timing distance—indicated in Columns (2) through (8)—to the fiscal year of the firm's first SOX 302 opinion. Each cell counts the number of unique firms for which the firm's initial PCAOB inspection overlaps with the firm's first SOX Section 302 opinion. For instance, the emphasized cell shows that three firms had *Post* coded as 1 for the first time in 2006, but issued their first SOX 302 opinion one year earlier in 2005. Column (9) enumerates the number of firms with no SOX 302 opinion in our sample window. We indicate the SOX adoption year by color and include a reconciliation to the total. Note in Column (3), 9 firms of 336 total firms, about 2.7%, adopt SOX 302 at the same time as the initial PCAOB inspection.

Internet Appendix: Table 1 – Details on the Timing of the Adoption of other SOX Provisions (Sections 302 and 404)

Panel C: Timing of SOX 404 Adoption Relative to the Fiscal Year of Treatment for Small Auditors using the Fieldwork Cutoff Date

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fiscal Year of Treatment	**Fiscal Year of Treatment minus Fiscal Year of First SOX 404 Opinion**							**No 404 Obs**	**Total**
	-3	**-2**	**-1**	**0**	**1**	**2**	**3**		
2004	8	5	9	9				19	50
2005		2	5	1	12			29	49
2006			21	33	**49**	73		130	306
2007				3	2	1	9	18	33
Total	8	7	35	46	63	74	9	196	438

SOX Adoption Year	Firm Count
2004	103
2005	60
2006	45
2007	34
No Obs	196
Total	438

Panel C compares the timing of the fiscal year when *Post* = 1 for the first time relative to the timing of the firm's first observed SOX 404 opinion using the end of the inspection fieldwork as the cutoff date (i.e., *Post* equals one if a firm's fiscal year-end is after the auditor-specific fieldwork end date plus 30 days, and zero otherwise). Each row separates the firms by fiscal year—indicated in Column (1)—where the treatment indicator, *Post*, switches from 0 to 1. Each column separates the firms by timing distance—indicated in Columns (2) through (8)—to the fiscal year of the firm's first SOX 404 opinion. Each cell counts the number of unique firms for which the firm's initial PCAOB inspection overlaps with the firm's first SOX Section 404 opinion. For instance, the emphasized cell shows that 49 firms had *Post* coded as 1 for the first time in 2006, but issued their first SOX 404 opinion one year earlier in 2005. Column (9) enumerates the number of firms with no SOX 404 opinion in our sample window. We indicate the SOX adoption year by color and include a reconciliation to the total. Note in Column (5), 46 firms of 438 total firms, about 10.5%, adopt SOX 404 at the same time as the initial PCAOB inspection.

Internet Appendix: Table 1 – Details on the Timing of the Adoption of other SOX Provisions (Sections 302 and 404)

Panel D: Timing of SOX 404 Adoption Relative to the Fiscal Year of Treatment for Small Auditors using the Inspection Report Release as the Cutoff Date

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Fiscal Year of Treatment	**Fiscal Year of Treatment minus Fiscal Year of First SOX 404 Opinion**						**No 404 Obs**	**Total**
	-2	**-1**	**0**	**1**	**2**	**3**		
2005	5	5	4	7			24	45
2006		11	9	**16**	29		50	115
2007			10	17	16	40	76	159
2008				1	1	4	11	17
Total	5	16	23	41	46	44	161	336

SOX Adoption Year	Firm Count
2004	76
2005	40
2006	32
2007	27
No Obs	161
Total	336

Panel D compares the timing of the fiscal year when *Post* = 1 for the first time relative to the timing of the firm's first observed SOX 404 opinion using the inspection report release as the cutoff date (i.e., *Post* equals one if a firm's fourth quarter earnings announcement falls on or after the release date of the inspection report, and zero otherwise). Each row separates the firms by fiscal year—indicated in Column (1)—where the treatment indicator, *Post*, switches from 0 to 1. Each column separates the firms by timing distance—indicated in Columns (2) through (7)—to the fiscal year of the firm's first SOX 404 opinion. Each cell counts the number of unique firms for which the firm's initial PCAOB inspection overlaps with the firm's first SOX Section 404 opinion. For instance, the emphasized cell shows that 16 firms had *Post* coded as 1 for the first time in 2006, but issued their first SOX 404 opinion one year earlier in 2005. Column (8) enumerates the number of firms with no SOX 404 opinion in our sample window. We indicate the SOX adoption year by color and include a reconciliation to the total. Note in Column (4), 23 firms of 336 total firms, about 6.8%, adopt SOX 404 at the same time as the initial PCAOB inspection.

Internet Appendix: Table 2 - Descriptive Statistics for Abnormal Trading Volume Sample

Variable	N	Mean	Std. Dev	P25	Median	P75
Abnormal 10-K Volume	68,830	0.253	1.047	-0.379	-0.054	0.534
Size	68,830	6.218	1.911	4.893	6.18	7.467
Market-to-Book	68,830	2.713	2.742	1.332	2.016	3.243
Leverage	68,830	2.447	4.088	0.414	1.044	2.421
Beta	68,830	0.928	0.603	0.476	0.875	1.313
Loss	68,830	0.280	0.449	0	0	1
Filing Delay after FYE	68,830	83.16	32.03	70	75	89
Filing Delay after EA	68,830	35.69	31.09	16	33	47
Analyst Following	68,830	7.125	8.130	1	4	10
Post	68,830	0.471	0.499	0	0	1
Treated	68,830	0.892	0.310	1	1	1

Table 2 presents descriptive statistics for the variables used in the abnormal trading volume analysis (i.e., Table 8 in the manuscript). We provide detailed variable definitions in Appendix B of the manuscript. We include observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample), so the same firm enter multiple times (see Table 1 in the manuscript). We truncate all continuous variables at 1% and 99% by fiscal year. The sample comprises 68,830 firm-year observations from the treatment (i.e., firms with domestic Big-Four or Tier-Two auditors) and control samples (i.e., U.S. cross-listed firms with non-U.S. Big-Four or non-U.S. Grant Thornton auditors).