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

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Abstract

This paper studies the impact of public audit oversight on financial reporting credibility. We analyze changes in market responses to earnings news after public audit oversight is introduced, exploiting that the regime onset depends on fiscal year-ends, auditors, and the rollout of auditor inspections. We find that investors respond more strongly to earnings news following public audit oversight. Corroborating these findings, we find an increase in volume responses to 10-K filings after the new regime. Our results show that public audit oversight can enhance reporting credibility and that this credibility is priced in capital markets.

Keywords: Regulation, Enforcement, Public oversight, Auditing, Earnings response coefficients

JEL Classifications: G14, G38, K22, M41, M42, M48

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Introduction

This paper studies the impact of regulatory oversight of auditing on financial reporting credibility in capital markets. Theory suggests that credibility strengthens the response of investors to a signal (e.g., Holthausen and Verrecchia 1988; Kim and Verrecchia 1991). Moreover, credible financial reporting could generate significant cost of capital benefits (e.g., Diamond and Verrecchia 1991). Thus, credible reporting is often viewed as a cornerstone of well-functioning capital markets (Summers 1999). However, what assures reporting credibility? High-profile accounting scandals illustrate that reporting credibility can quickly vanish, often triggering regulatory responses to restore it (Hail, Tahoun, and Wang 2018). Thus, examining whether regulatory oversight enhances reporting credibility is important. We analyze this question and (more broadly) the role of reporting credibility in capital markets by exploiting a change in audit oversight. The focus on audit oversight maps well into our broader research question because auditors' assurance of firms' financial reporting is essentially a credence good for outside investors.

In 2002, the U.S. Congress passed the Sarbanes-Oxley Act ("SOX") in an effort to restore reporting credibility after several scandals in the early 2000s. To achieve this objective, SOX focused on the process by which financial reports are prepared and audited. One of its core provisions was the creation of the Public Company Accounting Oversight Board ("PCAOB"), which was tasked with overseeing and inspecting all audit firms ("auditors") of SEC-registered public companies ("firms" or "issuers"). This new regime represents a major shift from self-regulation to public oversight of the audit profession. It did not come with new financial disclosures, but rather was intended to increase the credibility of independent auditing and firms' audited financials.

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In light of the numerous agency problems in auditing (e.g., Watts and Zimmerman 1983; Duflo et al. 2013), stricter oversight could, in principle, increase audit quality and in turn raise the credibility of financial reporting. However, it is not clear whether public oversight necessarily improves upon peer review. Auditing faces economic trade-offs between expertise, incentives, and independence (e.g., Hilary and Lennox 2005; DeFond 2010; DeFond and Zhang 2014). Similarly, regulatory economics points to potential problems with public sector regulators, such as resource constraints, inefficient bureaucracies, regulatory capture, and political pressure (e.g., Demsetz 1968; Stigler 1971; La Porta, Lopez-De-Silanes, and Shleifer 2006). Thus, the effect of the new oversight regime on reporting credibility is not obvious; widespread skepticism remains as to whether the PCAOB regime has reassured investors (e.g., Coates and Srinivasan 2014).[1]

We analyze marketwide changes in reporting credibility after the introduction of the PCAOB to learn about the role of reporting credibility in capital markets and to study whether public oversight can enhance or restore credibility after a major shock. The hypothesized mechanism for such an effect is that the new oversight regime spurs improvements in auditing relative to the peer review regime (e.g., because of larger penalties, better enforcement, or because PCAOB inspections identify more audit deficiencies) and that investors learn about these improvements and adjust their assessments of firms' reporting credibility accordingly.[2]

Our identification strategy exploits the staggered rollout of the PCAOB regime, which affects firms at different points in time depending on their auditors, fiscal year-ends, and the timing of the new auditor inspections. The PCAOB regime was rolled out in three phases: (1) one-time

[1] See, for example, *Wall Street Journal* (2010) and Hilzenrath (2010). Consistent with these concerns, 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," and Glover, Prawitt, and Taylor (2009) characterize the PCAOB's inspection model as "inefficient and dysfunctional."

[2] Our analysis is a joint test of the hypotheses that (a) audit oversight has an effect and (b) investors have reasonably accurate assessments of changes in audit oversight and auditing. Thus, in our analysis, a no result could have several explanations and could occur even if public audit oversight is effective.

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limited-scope inspections for the U.S. Big Four auditors (Deloitte & Touche, Ernst & Young, KPMG, and PricewaterhouseCoopers) in 2003; (2) annual full inspections (starting in 2004) for all auditors with more than 100 issuers (hereafter, "large auditors"); and (3) triennial, full inspections (starting in 2004) for auditors headquartered in the United States that issued a report for one to no more than 100 issuers (hereafter, "small auditors"). Because the first two phases are clustered in time, we use firms with non-U.S. auditors as a control group. For the third phase, we exploit the staggering of the 3-year rollout and use other triennially inspected auditors as a control group. For all phases, we use a generalized difference-in-differences design that compares earnings response coefficients (ERCs) before and after the respective inspections have taken place and firms' auditors have been treated.

Relying on prior theoretical work (e.g., Holthausen and Verrecchia 1988; Kim and Verrecchia 1991), we measure investors' assessments of reporting credibility based on how strongly they respond to a given amount of earnings news. Ceteris paribus, the market response to earnings news should increase (regardless of its sign) if investors believe the numbers are more credible. We operationalize reporting credibility by computing an ERC, which reflects short-window stock market reactions to (standardized) unexpected earnings news at the earnings announcement. Specifically, we measure ERCs based on the association between 3-day cumulative abnormal returns (CARs) centered on the earnings announcement date and unexpected earnings news (defined as the difference between a firm's actual, annual earnings per share (EPS), and the median analyst forecast). Prior research has used ERCs to assess the credibility of audit firms (e.g., Teoh and Wong 1993) and the credibility effects of earnings restatements (e.g., Wilson 2008; Chen, Cheng, and Lo 2014). We build on this prior literature and use ERCs to study the effects of

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public audit oversight on financial reporting credibility.[3]

From a research design perspective, ERCs are well suited for our identification strategy, which exploits the staggered introduction of the PCAOB regime as well as variation in auditors' and firms' fiscal year-ends. First, an ERC should not change in anticipation of an expected (future) improvement in credibility, but only after more credible earnings are released. Moreover, ERCs are measured over a short window around firms' earnings announcements, which are spread out in calendar time and depend on predetermined fiscal year-ends. These features are critical when using a staggered research design like ours. Despite its conceptual appeal, the use of ERCs requires assumptions and has drawbacks. First, ERCs require a measure of expected earnings in order to determine earnings news. We use consensus analyst forecasts, which are known to exhibit biases and to imperfectly reflect investors' expectations. Second, ERCs are not directly observable for a given announcement but must be estimated from a sample of announcements, which likely introduces noise and reduces the power of the analyses.

In our empirical analysis, we first examine changes in reporting credibility for U.S. firms with large auditors after limited and full inspections, relative to non-U.S. firms traded on U.S. exchanges (i.e., cross-listed firms) with large non-U.S. auditors. These cross-listed firms are subject to U.S. market events and other aspects of the U.S. regulatory regime, but their non-U.S. auditors are outside the scope of the PCAOB's initial inspections. Consistent with public oversight increasing investors' assessments of reporting credibility, we find that the ERCs of firms with auditors that are subject to the new PCAOB regime increase significantly compared to the ERCs of the control group. The effect becomes statistically significant after the PCAOB releases the reports from its 2003 limited inspections and strengthens even more after the PCAOB conducts

[3] We also validate ERCs as an approach to studying reporting credibility and its effects in markets, showing that ERCs decline when the quality of auditing is in question. See Internet Appendix §2.

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the full inspections in 2004. We also find that the ERC effects are concentrated in profitable firms, which is expected based on prior work showing that loss firms have minimal ERCs because of the transitory nature of losses (e.g., Hayn 1995). The estimated credibility effects are economically meaningful, suggesting an almost 20% increase in ERCs. They are also comparable in magnitude to ERC declines around major credibility shocks.

Our key empirical challenge is to isolate the effects of the PCAOB regime from other events, including (a) any other contemporaneous macroeconomic or capital market changes, (b) other SOX provisions unrelated to audit oversight, and (c) market responses to the accounting scandals. For instance, regulatory changes for firms' internal controls (stipulated by SOX Section 404[b]) could improve reporting credibility independent of public audit oversight. Similarly, after the Enron scandal, investors likely expected firms (especially former Arthur Andersen clients) to provide more assurance about their financial reporting, even in the absence of a regulatory response (e.g., Leuz and Schrand 2009). We perform a number of additional analyses to address this challenge and corroborate our inferences. First, we provide evidence that concurrent changes in firms' information environments are unlikely to drive our results.[4] Second, we show that the increase in ERCs after the regime change is *not* concentrated in former Arthur Andersen clients, where firm responses should be most pronounced. Third, our results are even stronger for firms that were exempt from Section 404[b] compliance, and are also robust to separately controlling for the onset of SOX Sections 302[a] and 404[b].

Our second set of analyses focuses on changes in reporting credibility for firms with small auditors, for which the new regime was phased in over 3 years. This staggered implementation

[4] We show that our findings cannot be explained by changes in (a) the magnitude of unexpected earnings, (b) the timing and relative amount of disclosure prior to the earnings announcement, (c) analysts' forecast bias, (d) the accrual component of reported earnings, (v) management earnings guidance, or (e) guidance bundling. We also check whether changes in the bundling of earnings announcements with 10-K releases drive our results.

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allows us to estimate ERC changes within small auditors using only the variation in the timing of initial treatment, eliminating the need to use cross-listed foreign firms as a control group. Importantly, for small auditors, there is very little overlap between the introduction of PCAOB inspections and the effective dates of other SOX provisions. Thus, this setting allows us to more cleanly disentangle the impact of the PCAOB regime from other SOX provisions as well as other concurrent events. Similar to our findings for large auditors, our results indicate a significant increase in ERCs as the new regime is rolled out; again, the effects are concentrated in profit firms.

As a final sensitivity check, we use abnormal trading volume around firms' 10-K filings as an alternative proxy for reporting credibility. While this proxy is conceptually less appealing than ERCs, it is still based on the idea that investors trade more in response to more credible financial reports (Kim and Verrecchia 1991). Consistent with this prediction (and with the ERC results), we find that abnormal volume responses to 10-K filings increase under the new regime.

Our paper contributes to the literature in several ways. First, we provide evidence on the pricing of reporting (or signal) credibility in capital markets, as measured by investors' responses to earnings news. Prior research provides evidence that the assurance by external auditors enhances the pricing of earnings news in the capital markets; this research often focuses on auditor reputation and similar attributes (e.g., Teoh and Wong 1993; Moreland 1995; Wilson 2008; Chen, Cheng, and Lo 2014; Marshall, Schroeder, and Yohn 2018). By exploiting a regime change in audit oversight that is arguably exogenous to any given auditor or firm, we provide novel evidence on credibility effects in capital markets and the extent to which public oversight can increase the credibility of audited financial reports. It is not a priori obvious that such a government intervention improves reporting credibility or that it restores confidence after a major shock like the accounting scandals of the early 2000s. As such, our study also adds to the literature on the merits of private

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versus public enforcement of regulation (La Porta, Lopez-De-Silanes, and Shleifer 2006; Jackson and Roe 2009), which generally focuses on the activities of securities regulators. For example, Duro, Heese, and Ormazabal (2018) examine the effects of disclosing SEC oversight activities on investor responses to earnings news. Prior auditing studies have focused on litigation and reputation as mechanisms that discipline or incentivize auditors (DeFond 2010 & 2012). In contrast, we provide evidence on auditors and on the public regulatory oversight of auditing.

Second, we contribute to the literature on the effects of SOX (see Coates and Srinivasan 2014 and Leuz and Wysocki 2016 for reviews). Many of these studies evaluate the effects of SOX as a whole (e.g., Chhaochharia and Grinstein 2007; Iliev 2010). For instance, Iliev (2010) examines specific costs (audit fees) and benefits (earnings quality) as well as the net effects of SOX on smaller firms' market value. In turn, we provide evidence on the capital market effects of the introduction of the PCAOB, an integral part of SOX. Although a large literature studies the PCAOB (see Abernathy, Barnes, and Stefaniak 2013 and DeFond and Zhang 2014 for reviews), it does not provide a capital market-based assessment of the new regime, which is our focus. Prior studies investigate differences in audit quality for auditors subject to PCAOB inspections using variation in inspections of non-U.S. auditors (e.g., Lamoreaux 2016; Fung, Raman, and Zhu 2014). Previous research also studies PCAOB inspection reports, including their content and effects on returns, client responses, and audit quality (e.g., Lennox and Pittman 2010; DeFond and Lennox 2011 and 2017; Gunny and Zhang 2013), as well as market reactions and client responses to the 2007 PCAOB sanctions against Deloitte & Touche (e.g., Dee, Lulseged, and Zhang 2011; Boone, Khurana, and Raman 2015). Among other things, this research suggests that the PCAOB regime led to changes in auditing and that capital markets and clients respond to PCAOB inspection reports. These findings help explain the mechanism for our results, but they do not imply that the

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new regime enhanced reporting credibility as perceived by investors. In fact, a frequent criticism of the PCAOB is that its inspections lead to costly process outcomes (e.g., more documentation and work for auditors) that do not significantly enhance audit or reporting quality. By providing evidence that the PCAOB regime increases investors' assessments of reporting credibility, we add to this work by showing that these changes in public oversight of auditing matter to investors. Of course, the new regime has costs and we do not examine net benefits.

A few studies examine the PCAOB regime change overall. Specifically, our paper complements Shroff (2019) and Aobdia and Shroff (2017), who focus on real- and audit-market effects. Perhaps closest to ours are Krishnan, Krishnan, and Song (2017), who examine the effects of first-time PCAOB inspections of foreign auditors of U.S. cross-listed firms on the value relevance of earnings and book value.[5] They find that the value relevance of accounting numbers increases in the post-inspection period for clients of inspected auditors. However, this effect is estimated relative to other clients of the *same* auditor and hence exists only for inspected-client engagements, and not the audit firm. Our analysis provides evidence of an increase in reporting credibility for all U.S. firms after their auditors are subject to the PCAOB regime.

2. Empirical Approach, Institutional Setting, and Presumed Mechanism

Our empirical analysis connects key dates for the rollout of the PCAOB oversight regime to subsequent changes in the market's assessment of reporting credibility for U.S. firms overall. We deliberately take the approach of studying marketwide shifts in investors' assessments of reporting credibility. An alternative approach would be to examine specific audit process outcomes (e.g., inspection findings, audit hours, audit opinions). While studying such outcomes is clearly

[5] Although the value relevance approach also tests whether the market pricing of accounting numbers changes, the long-run nature of value relevance regressions makes it difficult to control for concurrent but unrelated economic shocks and does not allow for a research design that exploits the rollout of the new regime (as does our study).

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important, these outcomes do not indicate whether public audit oversight enhances reporting credibility in capital markets. It is conceivable that auditors spend more time on documentation in the new regime but that investors do not value this increase in audit hours. For this reason, we do not focus on inspection reports or specific audit outcomes, but instead examine investor responses to earnings news in capital markets.

Because we take a market-based approach, our analysis cannot separately evaluate the impact of specific regime elements, such as changes in audit standards, inspections, or penalties. Instead, our analysis assesses the effects of the new public oversight regime *overall* relative to the prior regime. Because this approach does not specify the mechanism through which public oversight affects reporting credibility, it is important to spell out a potential mechanism that links the change in audit oversight to changes in investors' credibility assessments. Specifically, we expect a credibility effect in markets if (1) the new public regime represents a meaningful change in audit oversight relative to the peer review regime; (2) PCAOB inspections identify meaningful deficiencies in the way audits are conducted, and stricter oversight spurs improvements in auditing that extend beyond a single engagement;[6] and (3) investors learn about these changes and adjust their assessments of reporting credibility for U.S. firms accordingly.

To gauge the plausibility of this mechanism, we conduct an extensive search for descriptive, institutional, and academic evidence on each of the three elements of this presumed mechanism. Section 1 of the Internet Appendix ("IA§1") presents this evidence. We briefly summarize it here.

[6] We view the PCAOB as primarily playing an oversight and enforcement role. Although auditors are likely aware of changes they could make that would improve audit quality, such changes are costly, and it is not obvious that firms are willing to pay for them. The observed level of audit quality is not necessarily optimal for the market as a whole because individual auditors and firms do not internalize the full costs of a marketwide decline in the credibility of the audit profession or corporate reporting. In this situation, stricter public oversight could force auditors to undertake costly changes that they likely would not have made in the absence of oversight, which includes inspections and potential penalties.

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First, we examine whether the shift from (private) peer review to (public) PCAOB oversight represents a meaningful change in audit regulation (see IA§1 Part 1). The peer review regime was funded by the profession itself, and auditors were inspected by other auditors. Even for large auditors, peer reviews were relatively infrequent—occurring only once every 3 years. A perceived lack of independence and weak enforcement were frequently raised concerns about the peer review regime (e.g., Fogarty 1996; Hilary and Lennox 2005; Glover, Prawitt, and Taylor 2009; Doty 2011). In contrast, the PCAOB is a quasi-public agency established by SOX, funded largely by issuers, and overseen by the SEC. Section 104 of SOX tasks the PCAOB with the responsibility of inspecting registered accounting firms (i.e., auditors) and their audits of public issuers. PCAOB inspections extend to the (issuer-specific) engagement level. For large auditors (i.e., those that issue audit reports for more than 100 issuers), the PCAOB conducts annual inspections. All other auditors are subject to triennial inspections.

A PCAOB inspection assesses an auditor's compliance with: SOX, the rules and standards of the PCAOB, SEC rules, and professional audit standards (PCAOB 2004a). A full inspection includes (1) reviews of selected audits, (2) an evaluation of the sufficiency, documentation, and communication of the quality control systems, and (3) other testing of audit procedures as deemed necessary. The PCAOB has substantial enforcement authority and a wide array of penalties (see IA§1 Part 2).

To corroborate the mechanism's second element, we provide descriptive evidence that (a) PCAOB inspections identify meaningful weaknesses and deficiencies in auditing procedures (see IA§1 Part 3) and (b) that these findings lead to subsequent changes in auditing and financial reporting that last beyond the respective engagement (see IA§1 Part 4). Conceptually, lasting improvements in audit procedures beyond a single engagement are critical for the market to

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increase its overall assessment of credibility, as the mere revelation of previously unidentified deficiencies will likely lower investors' credibility assessments. Although stricter oversight could initially reveal more audit failures (to which investors presumably respond negatively), a stricter regime should ultimately enhance credibility if it leads to broader improvements in audit quality that spillover to other engagements and auditors.[7] Toward this end, the PCAOB regime not only identifies deficiencies but also requires *subsequent* changes in audit procedures (known as "remediation"). If, during their fieldwork, inspectors identify potential deficiencies in one or multiple engagements, the PCAOB 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." While the inspection report does not reveal which engagements were inspected or had Part I findings, auditors are *required* under PCAOB rules to remediate Part I findings, both contemporaneously, by performing additional audit work to validate the issued audit opinion, and prospectively, in future audit engagements. Consistent with this, DeFond and Lennox (2017) provide large sample evidence that PCAOB inspections improve internal control audits.

The PCAOB also evaluates auditors' firmwide quality control systems. If the auditor addresses any quality control criticisms successfully within a 12-month remediation period, the findings remain confidential. Otherwise, the PCAOB publicly releases these criticisms as "Part II findings." In 2004, when the new regime was phased-in, all Big Four auditors had quality control criticisms in their initial limited inspections (PCAOB Release 104-2006-078). The existence of these Part II findings indicates that inspections led to audit- and firmwide quality control changes. Such evidence is particularly important for our analysis because quality control criticisms by

[7] Note that although the PCAOB does not reveal which engagements were inspected, it does produce publicly observable outcomes (specifically, auditor-level inspection reports) that allow investors to update credibility assessments.

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definition are broader, extending beyond a single engagement.

The mechanism's third element is public information about the new oversight regime and the resulting changes in auditing practices that enable investors to update their credibility assessments. In IA§1 Part 5, we provide examples of numerous public sources that allowed investors to learn about the scope and effectiveness of the new regime. These examples illustrate that investors had meaningful information from several sources—including (1) the legislation creating the PCAOB as well as the initial authoritative pronouncements issued by the PCAOB; (2) PCAOB inspection reports and auditor responses to these reports; and (3) the news media—upon which they could rely to form assessments of the regime's effects on reporting credibility.

In sum, the institutional facts and descriptive evidence presented above support each of the three elements of the mechanism by which the new oversight regime could create greater reporting credibility for U.S. firms. Whether such regulatory oversight improves investors' credibility assessments of audited financial statements is an empirical question.

3. Empirical Measures, Research Design, and Sample Selection

3.1 Defining and measuring reporting credibility

Theory suggests that credibility strengthens the response of investors to signals. More specifically, investors should respond more strongly to a given level of earnings surprise (relative to expectations) if they believe that reported earnings accurately reflect economic performance. Holthausen and Verrecchia (1988) formalize this prediction using a noisy-rational expectations model with two consecutive information releases: an analyst forecast and an earnings announcement. Under fairly general conditions, they show that the variance of the price reaction to the earnings announcement (i.e., the second release) is unambiguously nondecreasing in the signal-to-noise ratio of the earnings surprise (see their proposition 1). An increase in credibility is

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tantamount to an increase in the signal-to-noise ratio and, more specifically, to an increase in investors' assessments of the precision of the earnings news (see also Kim and Verrecchia 1991). Thus, we use the strength (or sensitivity) of short-term market responses to earnings surprises as a proxy for credibility. This construct is commonly referred to as the earnings response coefficient (ERC) and is estimated by regressing abnormal returns at the earnings announcement on the respective earnings news (see Section 4.1 for notation and more details).

In addition to its sound theoretical underpinning, there is also a substantial empirical precedent for using ERCs as a proxy for investors' assessments of reporting credibility (see Kothari 2001; Dechow, Ge, and Schrand 2010 for reviews).[8] Even more relevant to us, many empirical studies use ERCs in audit-specific settings as a proxy for reporting credibility and to assess the capital market effects of changes in audit quality (e.g., Teoh and Wong 1993; Hackenbrack and Hogan 2002; Francis and Ke 2006; Wilson 2008; Chen, Cheng, and Lo 2014; Marshall, Schroeder, and Yohn 2018).

3.2 Research design and earnings response coefficients

From a research design perspective, ERCs are also well suited for our identification strategy, which exploits the staggered introduction of the PCAOB regime and variation in auditors' and firms' fiscal year-ends.[9] ERCs should not change in anticipation of future improvements in audit quality and reporting credibility. That is, ERCs reflect the credibility of reported earnings at a specific point in time (i.e., the earnings announcement). This feature is critical when using a staggered design. In contrast, firms' stock returns, market values, or cost of capital would likely

[8] In IA§2, we provide additional evidence to support our use of ERCs to capture audit-related differences in reporting credibility; this includes an examination of the ERCs and F-scores of Big Four auditor clients and an examination of ERC changes following the PCAOB's enforcement action against Deloitte & Touche in December 2007.
[9] In IA§3, we present a stylized time line for the introduction of the PCAOB regime and the related changes in reporting credibility and ERCs. This time line provides the conceptual underpinnings for our research design.

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reflect the PCAOB regime's impact much earlier, and hence would not allow us to exploit the staggered introduction. In addition, ERCs are measured over short windows around firms' earnings announcements, which are spread out in calendar time and depend on predetermined fiscal year-ends. These are desirable features from an identification perspective.

However, there are also some assumptions and drawbacks. First, ERCs require a measure of expected earnings in order to determine earnings news. We use consensus analyst forecasts, which are known to exhibit biases and imperfectly reflect investors' expectations. Second, ERCs are not directly observable for a given announcement but must be estimated from a sample of announcements. This likely introduces noise and reduces the power of the analyses. We use several approaches to deal with this noise and also consider abnormal volume reactions around the release of firms' 10-Ks as an alternative measure of reporting credibility (see Section 4.4). Third, ERCs change for reasons other than reporting credibility. We directly control for several known ERC determinants (see Collins and Kothari 1989) and employ a difference-in-differences design that strips out time-invariant biases in the ERC estimation in order to isolate credibility effects.

Nonetheless, we recognize that stricter audit oversight could have other effects beyond changes in reporting credibility that indirectly affect firms' disclosure and reporting. Thus, we acknowledge that our analysis does not capture all the reporting effects of public audit oversight; instead, it focuses on credibility changes as measured by ERCs. We also note that the potential effects of audit oversight on disclosure and reporting could potentially confound an ERC analysis. Given this concern, we gauge the extent to which changes in ERC components and/or disclosure and reporting affect our inferences (see Section 4.2 for details).

3.3 Timing of the regime change and control firms

If public audit oversight is effective in increasing reporting credibility, we expect ERCs to

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increase after auditors and firms are treated under the new regime. Thus, it is critical to isolate the timing of the treatment and to determine what counterfactual to use.

In June 2003, the PCAOB began limited inspections of U.S. Big Four auditors. The PCAOB conducted fieldwork and released inspection reports at approximately the same time for all limited inspections (see Table A1 in Appendix A). In 2004, the PCAOB conducted full inspections of large U.S. auditors and the first round of triennial inspections of small U.S. auditors. We examine the effects of the new regime on reporting credibility for each of the three distinct phases of the PCAOB regime introduction (i.e., limited, full, and triennial inspections). For each phase, we use a difference-in-differences analysis. It is unclear ex ante when investors adjust their credibility assessment and hence which phase is most relevant. The initial 2003 inspections were limited in scope.[10] Additionally, it takes time for auditors to adjust their audit procedures in meaningful ways and for investors to learn about these changes. Thus, the first full inspections are more likely to be the relevant treatment for large auditors.

Furthermore, limited and initial full inspections are clustered in time. Therefore, the first set of analyses relies on non-U.S. firms that are cross-listed on U.S. exchanges. This control group has several desirable features as well as potential drawbacks. The first benefit is that cross-listed control firms are audited by *non-U.S.* Big Four and Grant Thornton affiliates that (in 2003 or 2004) are not subject to PCAOB inspections, but are still required to comply with other SOX provisions at the same time as U.S. issuers (with one exception discussed later).[11] This feature helps us separate the PCAOB regime and other SOX provisions. Second, these issuers are exposed to the

[10] Limited inspections involved all components of full inspections, but were scaled down in extent (e.g., a lower number of individual audit engagements inspected), because, at that time, the PCAOB was still in the process of staffing up and building out its inspection regime (PCAOB 2004b). In the United States, the Big Four voluntarily agreed to participate in the limited inspections as the official PCAOB registration process had not yet begun.
[11] In IA§1 Table IA6A, we provide details on the timing of the adoption of other SOX provisions, broken down by U.S. versus foreign firms and accelerated filer status.

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U.S. market conditions and information environment, which makes it more likely that the treatment and control groups would have similar ERC trends in the absence of the PCAOB regime. However, we acknowledge that foreign, cross-listed issuers could be differentially affected by shocks in the United States (e.g., Bailey, Karolyi, and Salva 2006), which could bias our analysis. For this reason, we carefully examine the validity of the parallel trends assumption in our setting (see Section 4.1 and IA§4). Third, foreign cross-listed issuers could experience similar treatments at home if these countries implement audit oversight reforms similar to those in the United States.[12] Moreover, it is possible that non-U.S. auditors change their procedures because the PCAOB inspects their U.S. affiliates. Both spillover effects would lead us to underestimate the impact of the U.S. audit regime. Given these possibilities, it is useful to have a set of analyses that does not rely on foreign control firms.

In the triennial inspection analyses, we exploit the fact that the PCAOB phased-in small-auditor inspections over 3 years, which allows us to use (only) U.S. firms whose auditors have not yet been inspected by the PCAOB as the control group. Thus, we can identify the effects of the new oversight regime based solely on differences in inspection timing. This within-group design greatly mitigates concerns about the parallel trends assumption.[13] The 3-year rollout helps control for unrelated macroeconomic shocks and concurrent regulatory changes (including SOX). The primary drawbacks of this analysis are (a) the relatively small sample of U.S. issuers with triennially inspected, small auditors and (b) the possibility that auditors in later inspection cohorts could make anticipatory adjustments based on the results from earlier inspection cohorts. This

[12] In IA§1 Table IA6B, we provide details on the adoption timing of audit oversight regulation in other countries and discuss our basis for concluding that these regulations likely have little impact on our analyses.

[13] In IA§1 Table IA6D, we explicitly compare the timing of the initial PCAOB triennial inspections and the implementation of SOX provisions 404[b] and 302[a] and find that the overlap is very small (around 10% or less). To assess the similarity of clients of triennially inspected auditors, we compare firm characteristics across the years in which the auditors were initially inspected (or the years in which their inspection reports were released). We find no systematic differences across firms inspected in different years (untabulated).

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concern about preemptive auditor adjustments also arises in our large-auditor analysis, though to a lesser extent.

Although we examine three different events related to the rollout of the PCAOB regime, it is important to note that that these events are not independent. Moreover, there is the possibility that our effects could be biased downward if audit firms make changes ahead of their inspections and the market anticipates these changes (particularly for later triennial inspections). However, several aspects of our research design and credibility measure suggest that such anticipation effects are likely to be small. First, as noted before, ERCs should change only after the new regime is in place and hence do not reflect expected future improvements in credibility. Second, given the credibility issues auditors faced in the wake of the accounting scandals, any voluntary (or pre-regime) improvements were likely to be viewed with skepticism by the market. Third, the large number of Part I findings in PCAOB inspection reports (even in later stages of the regime rollout) provides little indication of anticipatory improvements by the auditors. Thus, it is reasonable to expect that investor responses to earnings surprises do not change until auditors have been treated by the new regime and investors learn about these changes.

Based on this logic, the earliest possible date that the ERC could reflect an increase in credibility is after the completion of the PCAOB's inspection fieldwork for a given auditor. The latest date for an ERC response is the public release of the inspection report. As it is not obvious exactly when the market updates its assessment (and hence when ERCs respond), we use both dates as alternative cutoffs and estimate treatment effects based on ERCs at the first earnings announcement after each alternative date.

Using the fieldwork end date as the cutoff, we define an issuer as treated when its fiscal

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year-end occurs in or after the month that inspection fieldwork ends for its auditor.[14] By that time, the auditor can use information from its PCAOB inspection to improve other audits that have not advanced from the planning stage.[15] If the inspection leads to lasting improvements in audit quality beyond the inspected engagements, and investors learn about these improvements (or expect them to have taken place), reporting credibility could increase shortly after fieldwork is completed. However, many fiscal year-ends occur well after the completion of fieldwork and there is a lag from a firm's fiscal year-end until its earnings announcement. Thus, there is generally a considerable amount of time between the completion of the fieldwork and our measurement of ERCs, giving auditors time to adjust their procedures and for the market to become aware of these changes. If we use the release date of the PCAOB inspection report as an alternative cutoff, there is an even longer period during which the auditor can adjust procedures and investors can learn about these changes. When we use the report release date as the cutoff, we define an issuer as treated when it first announces earnings after the PCAOB posts the inspection report for the firm's auditor online.

Importantly, while inspection reports do not reveal which audit engagements were inspected, they provide investors with information about the changes in audit procedures that arise from the inspections. Thus, the reports allow investors to update their assessments about the strictness of audit oversight (e.g., relative to their expectations at the enactment of SOX). In principle, this adjustment could go either direction. For instance, it is conceivable that the inspection reports reveal that the oversight regime is less strict than expected, so credibility goes

[14] For the Big Four, fieldwork typically lasts between 5 to 7 months. For small auditors, inspections are shorter, so we add 30 days to fieldwork completion when defining the cutoff date to allow small auditors to adjust audit procedures. See Table A1 and Figures A1 and A2 in Appendix A for more details on timing and an illustration of our research design for annually inspected auditors. See the Internet Appendix for an illustration for triennially inspected auditors.
[15] Aobdia (2018) notes that PCAOB inspectors normally share feedback on auditing deficiencies during on-site inspections. For example, the substance of the inspection comment forms, which are a precursor to Part I findings, is shared on site during fieldwork (Riley et al. 2008).

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down. For this reason, we do not compute incremental changes in the ERC from the end of fieldwork to the report release. Rather, we estimate long-run changes in (short-window) ERCs *relative* to the pre-PCAOB-regime period and test whether ERCs have increased.

3.4 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 Web site. All data are publicly available. For the analyses of annually inspected auditors, we define a roughly 4-year window around treatment such that we typically have 2 fiscal year-ends *before* and *after* the respective cutoff date. For the limited inspections, using the fieldwork (inspection report) cutoff date, the sample period is from December 2001 to November 2005 (June 2002 to May 2006) and the sample includes firms whose fiscal year-ends that fall into this window. For the limited inspections, we include the full sample of cross-listed control firms because (at that time) there were no formal cooperative agreements between the PCAOB and the home-country regulators of non-U.S. firms to conduct inspections in non-U.S. jurisdictions. For the full inspections of Big Four and Tier-Two auditors, using the fieldwork (inspection-report) cutoff date, the sample period is from June 2002 to December 2006 (July 2003 to November 2007) and the sample includes firms whose fiscal year-ends that fall into this window. For the full inspection control sample, we exclude cross-listed firms from countries with an inspection agreement with the PCAOB during or before our analysis window.[16] The control group also includes firms from countries unavailable

[16] The PCAOB commenced full inspections on some non-U.S. Big Four affiliates in 2005. In April 2005, KPMG Canada was the first inspected. Australia signed an agreement with the PCAOB on July 16, 2007. We exclude Australian control firms when there is an 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. See http://pcaobus.org/International/Pages/RegulatoryCooperation.aspx for details.

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for inspection.[17]

Panel A of Table 1 provides details about the sample composition for the limited and full inspection analyses for the treatment and control groups by auditor, inspection type, and treatment date. For the limited inspections, the number of treatment firms is similar across auditors. For the full inspections, among the Big Four, there are a similar number of treatment firms, whereas other large (Tier-Two) auditors have fewer firms than the Big Four. Combining inspections and respective groups, our treatment sample includes 4,289 unique domestically audited firms over 37,001 firm-years, while our control sample includes 579 unique non-U.S. firms over 3,765 firm-years.[18] In IA§5, we provide a breakdown of the treatment and control samples by auditor location.

Panel B of Table 1 provides details about the sample for the triennially inspected auditor analyses. The sample size is 1,338 firm-year observations. As expected, there is significant variation in inspection timing because of the triennial cycle. To avoid overlap with the 2008 financial crisis, we exclude fiscal years ending after Q2 of 2008 from our analysis.

3.5 Descriptive statistics

Panels A–D of Table 2 present descriptive statistics for domestic issuers with large annually inspected auditors (i.e., the treatment group in the limited and full inspection analyses), cross-listed firms with non-U.S. auditors (i.e., the control group in the limited and full inspection analyses), a comparison of means between these subsamples, and issuers with triennially inspected auditors, respectively. The first two variables are the key inputs to estimate the ERC: the *CAR* at the earnings announcement and the earnings surprise or unexpected earnings (*UE*). Our primary control variables are *Loss*, *Size*, *Market-to-book*, *Leverage*, *Persistence*, and *Beta*. The other

[17] http://pcaobus.org/International/Inspections/Pages/IssuerClientsWithoutAccess.aspx (accessed January 2015).
[18] 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 for these auditors. We do not include Grant Thornton in the control group for the limited inspections in order to provide a clean within-Big-Four comparison.

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variables in Table 2 are used in additional sensitivity tests. We also count the number of days between the respective cutoff date for the auditor's initial treatment (i.e., either the end of fieldwork or the report release) and the firm's earnings announcement at which the first post-treatment ERC is measured (*Timing: Treatment to first EA (in days*)). The variable indicates that our design allows for a substantial time lag during which auditors could adjust their procedures and where investors could learn and price the effects of the regime change.

In panel C, we see that the control sample is similar to the treatment sample along most dimensions, including mean *Loss*, *Market-to-book*, *Leverage*, and *Persistence*. However, the two groups differ in terms of *Size* and *Beta*, which is unsurprising given that exchange-traded, cross-listed firms tend to be quite large. Thus, it is important to include these variables as controls (interacted with *UE*). In addition, we run analyses in which we explicitly match firms based on these two characteristics. The final two columns of Table 2 show that *Size* and *Beta* are no longer statistically significantly different across the treatment and control groups when matched.

Panel D reports the descriptive statistics for the control variables for the firms with triennially inspected auditors. As expected, these firms are smaller and more highly levered.

4. Empirical Results

4.1 Analysis of large, annually inspected auditors

Our first set of analyses examines changes in reporting credibility for firms whose auditors were subject to the 2003 limited inspections as well as the initial full inspections in 2004. We estimate the following equation:

$$\begin{aligned} CAR_{it} = {} & \alpha + \beta_1 UE_{it} + \beta_2 Post_t + \beta_3 Treated_i + \lambda_n Controls_{it} + \gamma_n Fixed\ effects + \\ & + \beta_4 UE \times Post_{it} + \beta_5 UE \times Treated_{it} + \beta_n UE \times Controls_{it} + \beta_n UE \times Fixed\ effects + \\ & \beta_6 Post \times Treated_{it} + \beta_7 UE \times Post \times Treated_{it} + \varepsilon_{it} \end{aligned} \quad (1)$$

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CAR is the 3-day (t-1, t=0, and t+1) cumulative abnormal return for firm i, centered on the earnings announcement date and market-adjusted by the CRSP value-weighted index. *UE* is the difference between the actual, annual EPS and the median analyst forecast for annual EPS, both from I/B/E/S. *Treated* is an indicator 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 that equals one after the treatment by the new regime, and zero otherwise. As discussed in Section 3.3, we use two alternative cutoffs: the fieldwork end date and inspection report release date (see Appendix A for details). For analyses using the fieldwork end date, *Post* equals one if a firm's fiscal year-ends in the same month as the fieldwork or later. For analyses using the inspection report date, *Post* equals one if a firm's fourth quarter earnings announcement falls on or after the release of the inspection report. While auditors' fieldwork end and inspection report release dates are clustered in time, the *Post* variable is coded based on clients' fiscal year-ends or earnings-announcement dates, respectively, which provides more variation. In Equation (1), our primary coefficient of interest is β_7, which measures the incremental change in the ERC for firms whose auditors have been treated by the PCAOB regime. A positive coefficient indicates an increased response to earnings news under the new regime, which we interpret as an increase in reporting credibility.

We include controls for a variety of firm characteristics shown by prior research to be important determinants of a firm's ERC. First, we include *Loss*, an indicator variable that equals one if a firm reports negative earnings, and zero otherwise; we also include *UE*×*Loss*. As losses are less persistent than profits, the response to negative earnings is likely lower than the response to positive earnings (Hayn 1995). Second, we include *Size*, *Market-to-book*, *Leverage*, *Persistence*, and *Beta* as well as the interaction of these variables with *UE*, given that prior work shows that ERCs are a function of the riskiness, growth, and persistence in earnings (e.g., Collins and Kothari

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1989; Easton and Zmijewski 1989; Dhaliwal, Lee, and Fargher 1991).

We include *Fixed effects* for the auditor's global network, 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 first two sets of fixed effects control for cross-sectional ERC differences across auditors and countries. The year-quarter fixed effects flexibly account for ERC changes over time, for instance, due to changes in market sentiment or macroeconomic cycles (e.g., a recession). We truncate all continuous variables, with the exception of *UE*, at the 1% and 99% level. Unexpected earnings are known to have large outliers, especially in the left tail (e.g., Beaver, Lambert, and Morse 1980; Collins and Kothari 1989; Kothari 2001). Hence, we truncate *UE* at the 2.5% and 97.5% level. As a further control for extreme observations, we estimate a weighted least squares ("robust") regression that places less weight on estimates with large absolute residuals.[19] We rely on the robust regression as our primary specification because we view it as an effective and nondiscretionary way to reduce the influence of outliers.[20] Because the treatment coefficient of interest includes *UE* and hence varies by firm, in all tests we cluster standard errors by firm.[21] Table B1 in Appendix B defines each variable.

Table 3, panel A, presents the robust regression results of estimating Equation (1) using each of the four alternative dates for the onset of the PCAOB regime: limited inspection fieldwork

[19] We perform robust regressions using Stata's "*rreg*" procedure, which eliminates any observations with a Cook's distance greater than one and creates weights for the remaining observations based on the absolute residuals.

[20] Prior studies use a variety of approaches to deal with extreme *UE* observations, including deleting *UE* observations that exceed 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, Cheng, and Lo 2014). In IA§6, we present scatterplots for untrimmed and truncated data across a variety of truncation levels and provide several additional analyses to assess the sensitivity of our results to extreme *UE* observations. These analyses suggest that the method for handling extreme *UE* observations can affect the strength of our inferences.

[21] We calculate robust, firm-level clustered standard errors using a WLS regression with weights generated by the robust regression. In our setting, clustering (i.e., by auditor or year) is problematic given few large auditors and the short time series (e.g., Petersen 2009; Conley, Goncalves, and Hansen 2018). However, our inferences are very similar when we double cluster by firm (or industry) and earnings announcement month (untabulated).

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(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 sample windows, the estimated effects for each date cannot be interpreted cumulatively (or incrementally); they simply provide alternative estimates for the effect of the regime change. In Column 1, which uses the end of the limited inspection fieldwork, *UE*×*Post*×*Treated* is positive but statistically insignificant. In Column 2, the treatment effect based on the limited inspection report release is significant at the 10% level. In Columns 3 and 4, where *Post* is based on the full inspection fieldwork end and the full inspection report release, respectively, *UE*×*Post*×*Treated* is positive and significant at the 5% level (at least) and ranges in magnitude from 1.149 to 1.600. Overall, these results indicate that ERCs increase significantly after the release of the limited inspection reports and become even more pronounced after the first full inspections.

In Table 3, panel B, we present results from an alternative design that reduces potential contamination effects from the overlap in the pre- and post-periods when using alternative cutoff dates (e.g., in the primary design, the pre-period for the report release overlaps with the post-period for fieldwork). In this design, we exclude pre-period fiscal year-ends that occur during fieldwork and prior to the release of the inspection report. To reduce sample attrition, we extend the sample window to maintain pre and post periods that are of similar length to the main analyses. Figures A1 and A2 in Appendix A illustrate the limited and full inspection designs without this overlap (which we call the "dropped observations" design). Results with this design are stronger, particularly in Column 4, which is where the contamination effects from overlap are likely the most severe. As expected, the described overlap biases against our results. To be conservative, we use the design without the dropped observations as our primary specification.

Table 4, panel A, presents results when stacking the samples for the limited and full

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inspections and the fieldwork end and inspection report release dates (hereafter, the "combined" sample), which effectively provides the average change in ERC across the four alternative sample windows. This presentation is parsimonious without favoring a particular date, and it also exploits the variation in firms' fiscal year-ends more effectively, which is why we use it for the subsequent analyses. In this specification, *UE*×*Post*×*Treated* is positive and significant at the 1% level (Column 1).[22] This specification should deliver a conservative estimate of the treatment effect because it pools the relatively small response to the limited inspections with the larger response to the full inspections. We report results for the combined sample using the dropped observation design in Column 2; the coefficient magnitude is similar and is also significant at the 1% level.

The key assumption underlying our identification strategy is that the treatment and control firms would have had similar trends in their ERCs absent the introduction of public audit oversight (i.e., the parallel trends assumption). To assess the reasonableness of this assumption, in Figure 1, we replace the single *Post*×*Treated*×*UE* interaction term with separate interactions for each of the years in our sample, except for the year immediately before the introduction of the PCAOB regime, and map out the treatment effect in event time. In the pre-period, the coefficients for the incremental ERC are small and statistically insignificant, which supports the parallel trends assumption.[23] The treatment effect is positive but statistically insignificant in period *T* and becomes economically and statistically significant in periods *T*+1 and *T*+2, consistent with the coefficient pattern in Table 3 that shows stronger results after the first full inspections.

Although the evidence regarding the parallel trends assumption is reassuring, recall that our treatment and control firms differ along two observable dimensions, *Size* and *Beta*. For this

[22] For completeness, in IA§7, we report a full tabulation of all the coefficient estimates, excluding the coefficient estimates for the fixed effects and fixed effects interacted with *UE*.
[23] In IA§4, we examine past trends in ERCs for our treatment and control firms over an extended period and again find no evidence that calls into question the validity of the parallel trends assumption.

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reason, we also conduct an analysis using coarsened exact matching (CEM) (see Blackwell et al. 2009) based on both these firm characteristics. CEM relies on covariate weighting to construct a synthetic control sample, allowing us to preserve sample size. We coarsen our sample into 20 CEM bins (per matching variable), which reflects a trade-off between preserving observations and the ex post similarity of the matching variables for the treatment and control groups. We then use the weights from this coarsening in estimations of Equation (1). After applying the CEM weights, average *Size* and *Beta* are very similar for the treatment and the control samples (see Table 2, panel C). Table 4, Column 3, presents the regression results with the CEM weights. They are consistent with the results in Column 1, which do not use the CEM weights.

Next, we introduce a cross-sectional split which further tightens our analysis in two ways. First, we exploit the fact that the market response to the earnings surprises of loss firms (i.e., firms with negative earnings, as distinct from negative surprises) is muted because of the transitory nature of losses (Hayn 1995).[24] Given the low ERCs for loss firms, the treatment effect of the new regime is expected to be concentrated in profitable firms. Thus, this differential prediction provides a way to gauge whether the treatment effects behave sensibly. Second, while the inclusion of *Loss* and its interaction with *UE* already accounts for the differential response to losses, it is possible that the proportion of firms with losses happens to change around the introduction of the new regime, which could affect our estimates. By separately estimating the effects of the regime change for profit and loss firms, we control for composition changes through time and further insulate our analysis from macroeconomic changes. We include the interactions of *Loss* with the treatment indicators in all subsequent analyses.

In Table 4, Column 4, when we separately estimate the treatment effect for profit and loss

[24] In untabulated results, we confirm that the ERC for firms with losses (i.e., negative earnings) is close to zero.

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firms, we find that the results are stronger and that the credibility effects of the new regime are concentrated in profitable firms, which is consistent with our expectations and corroborates our interpretation (i.e., the *UE*×*Post*×*Treated* coefficient is 0.942).[25]

To assess the economic magnitude of the observed effects, we estimate the ERC for the treatment group in the pre-period (using our full baseline model to calculate the pre-period average *UE* for the treatment group). Next, we interpret the magnitude of the estimated treatment effects in terms of the pre-Enron "baseline" ERC (see IA§10 for details). For treated firms in the pre-period, an earnings surprise of 1% leads to a price change of 3.7%. The estimated treatment effect in our most conservative specification (i.e., our primary design with coarsened exact matching in Column 3 of Table 4) implies that ERCs increase by 0.719 (or roughly 19.5%). Thus, after the introduction of public audit oversight, the total ERC for treated firms equals 4.4, which implies that in the post-period, an earnings surprise of 1% translates into a price change of about 4.4%, as compared to 3.7% in the pre-period. This effect is sensible and economically meaningful.

Next, we present several additional tests to gauge the influence of firm-level heterogeneity on our results. To address this concern, it is common to include firm fixed effects in a generalized difference-in-differences estimation. However, this approach poses several issues in our analyses. First, our main analysis is based on a short 4-year window, centered on the onset of the regime. This means that we typically have only two firm-year observations before and after an inspection, which makes the estimation of firm fixed effects quite demanding and noisy. Second, including firm fixed effects in an ERC regression is different from a standard difference-in-differences setting where the left-hand-side variable is the outcome of interest. In an ERC regression, the

[25] Similar arguments apply to extreme realizations of *UE*, which essentially make the ERC nonlinear. We therefore perform several tests in IA§8 to gauge the influence of extreme observations and the effect on our results of allowing for nonlinearities in the relationship between *CAR* and *UE*.

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outcome of interest is an interaction on the right-hand side of the model. Therefore, including firm fixed effects (without interacting them with *UE*) does not control for firm-level differences in the ERC. It controls for a firm's average CAR at the earnings announcement (and hence can help with sample composition changes over time of firms with different CARs). But in order to have a within-firm ERC analysis, it is necessary to interact *UE* with firm fixed effects. However, such a fully interacted model is not feasible with only four observations per firm. Given these issues, we use three alternative approaches to assess the impact of firm-level heterogeneity on our results.

First, in Table 4, panel B, Column 1, we introduce firm fixed effects as main effects in our primary (4-year) design, and in Column 3 use the same specification for a sample period of 6 years. Despite the limited number of observations per firm, the results are very similar in magnitude and statistical significance to those in our primary specification (panel A).[26] Second, in Table 4, panel B, Columns 2 and 4, we present results using "firm-group fixed effects," which are included as main effects and interacted with *UE* using a 4- and 6-year sample period, respectively. Specifically, we create firm groups by forming 100 portfolios (10-by-10) matched on *Size* and *Beta* in the first year that a firm enters the sample; we then introduce fixed effects for these firm groups. We choose *Size* and *Beta* to form firm groups because the treatment and control firms exhibit statistically significant differences along these two dimensions (see Table 2, panel C), which are important determinants of firms' ERCs. These results are similar to those in our primary analyses.[27]

Third, instead of estimating the ERC in a regression, we define it as *CAR* divided by *UE*

[26] In additional untabulated analyses, we assess the impact of including firm fixed effects as main effects for each of the specifications in Table 4, panel A. In some of these specifications, the treatment effect is insignificant and/or substantially attenuated. However, this appears to be driven by a reduction in the number of observations that contribute to the estimation of the treatment effect. Once we extend the sample period to include up to 6 years per firm, the results are again positive and statistically significant, and the coefficients of interest are similar in magnitude to those in our primary analyses without firm fixed effects.

[27] In IA§9, we present results for additional analyses that confirm our results are robust to the inclusion of fixed effects for the Fama and French 12 industries as well as alternative portfolio sizes for the firm-group fixed effects analyses.

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and then use this proxy as the dependent variable. With this specification, we can control for time-invariant firm-level heterogeneity with firm fixed effects without using interactions. However, our sample period is still short, so the downside of this approach is that the "ERC" proxy is quite noisy. Nevertheless, using this approach, we obtain similar results and draw essentially the same inferences as in our primary analyses (see IA§9). Thus, based on all three approaches, we conclude that firm-level heterogeneity is unlikely to drive our results.

4.2 Sensitivity analyses: Changes in information environment and concurrent events

In this section, we conduct four sets of sensitivity analyses. First, we explore whether other contemporaneous changes in firms' information environments or in the properties of reported earnings affect our prior results. For instance, it is conceivable that the new regime itself affects elements used in the construction of ERCs (e.g., analysts' forecasts). To investigate this possibility, we examine changes in seven separate proxies for changes in firms' information environments or earnings properties after the introduction of the PCAOB, including (1) unexpected earnings (*UE*); (2) analysts' earnings forecasts (*Forecast*); (3) the timeliness with which information is incorporated into prices (*Timeliness*); (4) the relative amount of information that firms disclose prior to the earnings announcement as a proportion of the total information released during the year, including the earnings announcement (*Relative information*); (5) accruals (*Scaled raw accruals*); (6) the presence of management earnings guidance (*Earnings guidance*); and (7) the bundling of the earnings announcement with management guidance (*Guidance bundle*). Table B1 in Appendix B describes each of these measures in detail. We present descriptive statistics for each of the proxies in panels A and B of Table 2 separately for our treatment and control firms.

To examine whether there are systematic (and potentially confounding) changes in these proxies around the regime change, we use the same difference-in-differences design as in our

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primary analyses, successively replacing *CAR* in Equation (1) with each proxy. The coefficient on *Post×Treated* indicates whether there is a change in the proxy after the onset of the PCAOB regime relative to the control group. In each specification, we include the same set of control variables and auditor-, country-, and year-quarter fixed effects. Table 5 presents the regression results. Across all seven information environment proxies, the coefficient on *Post×Treated* is economically small, suggesting that these proxies are not much affected.[28] The effects are not significant, except for *UE* in Column 1 and *Relative information* in Column 4. The documented decrease in *UE* suggests that analyst forecast bias slightly decreases for treated firms in the post-period. However, in addition to being small in magnitude, any potential impact of this change is mitigated, because we control for *UE* in Equation (1) when estimating the ERC for a given level of earnings surprise.

The observed increase in *Relative information* suggests that in the new regime, treated firms release more of the year's total information before the earnings announcement. The increase of 0.026 is about 18% of 1 standard deviation (0.144). Again, this effect is relatively small. More importantly, if firms release more of the year's total information before the earnings announcement, this should decrease the relative importance of the earnings announcement. Thus, the change in *Relative information* likely works against finding an increase in the ERC. In an untabulated test, we confirm that our results are essentially unchanged when we include *Relative information* as an additional control variable (interacted with *UE*). Consistent with a decrease in *Relative information* biasing against our results, the treatment effect for *UE×Post×Treated* increases slightly (0.860) and is still significant at the 1% level. We also confirm that results do not change materially if we simultaneously include all seven information environment proxies as

[28] Each of these results is similar when including firm fixed effects (untabulated).

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controls (untabulated). Overall, there is no evidence that our findings are explained by significant changes in pre-earnings-announcement disclosures, management guidance, earnings' properties, and/or analyst forecast behavior.[29]

In our second set of sensitivity analyses, we address the possibility that the observed ERC change is attributable to firms' voluntary efforts to improve their financial disclosures in response to the 2001–2002 accounting scandals. Although our use of cross-listed, non-U.S. firms as a control group mitigates this concern, it is possible that U.S. firms respond more strongly to these scandals, affecting our analysis. To test this, we separately examine firms audited by Arthur Andersen ("AA") in 2000 and 2001. Leuz and Schrand (2009) show that former AA clients responded more strongly (i.e., with a larger increase in disclosure) to the revelations at Enron than did firms with other auditors. Thus, if our results are driven by these market responses rather than the PCAOB regime, we would expect to see larger ERC changes for former AA clients. Columns 1 and 2 of Table 6 present the results. Excluding former AA clients, the treatment effect is still positive, significant, and larger in magnitude than the treatment coefficient for former AA clients. While the coefficients are not statistically different from each other, the relative magnitudes suggest smaller effects for former AA clients, which is inconsistent with the explanation that a scandal-induced shift in reporting incentives drives our findings.

In the third set of analyses, we address the possibility that the observed ERC increase could reflect other SOX provisions. Three provisions stand out as possibilities: (1) rules regarding audit committee independence, (2) Section 302 rules regarding executive certification of financial

[29] In untabulated analyses, we also examine the possibility that our results could be explained by the increase in external monitoring that accompanies the raising of additional external capital following PCAOB inspections (as documented in Shroff 2017). We find no evidence that our treatment firms experience an increase in capital raising or capital expenditures around the onset of the PCAOB regime. Rather, it seems more plausible that the increase in credibility we document is the mechanism that leads to subsequent increases in capital raising.

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statements, and (3) Section 404[b] rules regarding the assessment of internal controls.[30] Rules on audit committee independence became effective on April 25, 2003, for both domestic and foreign issuers, thus affecting our treatment and control groups simultaneously (SEC Release Nos. 33-8220; 34-47654). Similarly, Section 302 came into effect on August 29, 2002, for all domestic and foreign issuers (SEC Release No. 33-8124).

In contrast, the adoption of Section 404[b] was staggered based on issuer size and domicile. For U.S. accelerated filers (i.e., firms with market capitalizations greater than $75 million), Section 404[b] became effective for fiscal year-ends on or after November 15, 2004. The SEC deferred implementation for nonaccelerated filers because of cost concerns. In 2010, the Dodd-Frank Act made this exemption permanent. Foreign 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 the market responds negatively to the disclosure of 404[b] internal control weaknesses (e.g., Hammersley, Myers, and Shakespeare 2008). Thus, if firms improve their internal controls, and if better controls lead to more credible reporting, it is possible that the effects documented in Table 4 are attributable to the implementation of SOX 404[b] rather than the new public oversight regime for auditors.

We conduct two analyses to separate the PCAOB regime from other SOX provisions. First, in an approach similar to Iliev (2010), we separately examine ERC changes for accelerated and nonaccelerated filers. If the documented increase in credibility is attributable to the new oversight regime (instead of 404[b]), we expect similar effects for accelerated and nonaccelerated filers. Results in Columns 3 and 4 of Table 6 are consistent with this prediction. The treatment effect for nonaccelerated filers is 1.139 as compared to 0.871 for accelerated filers. These coefficients are

[30] In addition, the PCAOB adopted several new auditing standards. However, we consider these changes part of the PCAOB regime (and not confounds). In IA§1 Table IA6C, we provide details on the adoption timing of the new PCAOB auditing standards. Given their timing, it is unlikely that they affect our analysis.

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not significantly different and, if anything, indicate a larger ERC change for nonaccelerated filers—a result that goes against the alternative explanation.

Second, we separately examine ERC changes within treatment firms based on whether or not a firm has an internal control opinion from its auditor—be it an effective, adverse, or disclaimer opinion (i.e., we estimate our effects for firms outside and within the SOX 404[b] regime). If the internal control opinions required under SOX 404[b] made earnings more credible, we would expect a larger treatment effect for firms with such opinions. The results, presented in Columns 5 and 6 of Table 6, do not support this conjecture. The estimated treatment effect for firms *without* a SOX 404[b] internal control opinion (0.923) is larger than for firms with an opinion (0.234); the difference in the coefficients is statistically significant at the 10% level, suggesting that the effects we document are distinct from the potential impact of SOX 404[b]. To be sure, in Column 7, we simultaneously include both indicators in our model to control for the effects of SOX 404[b] and SOX 302[a]. The estimated treatment effect is similar to that in Table 4, which again provides assurance that the documented increase in reporting credibility is not attributable to other key SOX provisions.[31] The next section provides further support for this conclusion.

4.3 Analysis of small, triennially inspected auditors

Next, we examine the initial triennial inspections of U.S.-registered, small auditors, beginning in 2004. We use generalized difference-in-differences tests to measure the effect of triennial inspections, estimating the following equation:

$$\begin{aligned} CAR_{i,t} = \alpha + \beta_1 UE_{i,t} + \beta_2 Post_t + \beta_3 UE \times Post_{i,t} + \lambda_n Controls_{i,t} + \gamma_n Fixed\ effects + \\ \beta_n UE \times Controls_{i,t} + \beta_n UE \times Fixed\ effects_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{2}$$

[31] The negative coefficient for SOX 302[a] should be cautiously interpreted, because SOX 302[a] was effective for all filers for fiscal years ending after August 29, 2002, which is early relative to the relevant PCAOB regime dates. Hence, the indicator equals one for most (about 84%) firm-year observations.

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CAR, *Post*, and *UE* are calculated as previously discussed.[32] We include controls as indicated in the table. We also include auditor- and year-quarter *Fixed effects* as well as the interactions of these fixed effects with *UE*.[33] With this fixed effect structure, the identification of the treatment effect, *UE*×*Post*, comes solely from variation in the timing of inspections among triennially inspected auditors. We include all available firm-year observations for firms with small auditors from 2001 through 2007. We exclude fiscal year-ends after Q2 of 2008 to mitigate potential confounding effects from the financial crisis. As in Table 3, we separately examine two alternative cutoff dates: the completion of fieldwork and the inspection report release.[34]

Table 7 presents results for this analysis. In Column 1, we estimate a robust WLS regression of Equation (2), where *Post* is based on the fieldwork end date. The estimated treatment effect of 0.789 is positive and significant at the 5% level. In Column 2, *Post* is based on the report release date. *UE*×*Post* is positive (1.063) and statistically significant at the 5% level. The larger coefficient magnitude for the inspection report release is consistent with less publicized fieldwork dates for triennial firms. In Column 3, we include additional controls for SOX 404[b] and 302[a] and (using the report release date) find similar results, which indicates that the increase in reporting credibility is not attributable to other SOX provisions. Column 4 reports results for the "dropped observations" design, which excludes the post-fieldwork period from the sample to avoid overlap and contamination. The treatment effect (1.022) is similar to the other specifications. Again,

[32] There are two exceptions. First, for triennially inspected auditors, fieldwork is shorter, and it is less clear that the market is aware of its timing. Thus, we code the *Post* variable as 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). Second, because small firms have less analyst coverage, we extend our window for measuring the median analyst forecast (from which *UE* is computed) from 95 to 360 days.

[33] As in the large auditor analysis, the degrees of freedom limit the number of fixed effects we can include and preclude the use of firm fixed effects. However, in IA§9, we confirm that results are robust to the consideration of pseudo-firm fixed effects based on firm characteristics and industry groupings.

[34] In IA§3, we provide specific examples of how we code the *Post* indicator for a variety of fiscal year-ends and inspection years.

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assuming a pre-period benchmark return response for a 1% earnings surprise of 3.7%, in the post-PCAOB regime period, an earnings surprise of the same magnitude leads to a price change of about 4.7%, an increase of about 28% for clients of triennially inspected auditors.

In Table 7, Column 5, we report results including firm fixed effects as main effects in Equation (2). The treatment effect is positive but statistically insignificant. One potential explanation is that firm fixed effects reduce the number of firms that contribute to the identification of the treatment effect. We have relatively few observations for triennially inspected auditors to begin with (1,338 firm years), but even fewer (581) with at least one observation in both the pre- and post-period. In Column 6, we include firm-group fixed effects as main effects and interacted with *UE*, which allows us to control for firm-group-level heterogeneity in the estimated ERC. These results are positive, statistically significant, of a very similar magnitude to those in Columns 1 and 2, and consistent with the result in Column 5 reflecting sample attrition.

Overall, the results for small auditors are consistent with our earlier findings for large auditors, which indicate a significant increase in reporting credibility following the introduction of public audit oversight.

4.4 Abnormal trading volume around 10-K filings as an alternative credibility proxy

In this section, we examine abnormal trading volume around the SEC filing of firms' annual financial statements (10-Ks) as an alternative measure of reporting credibility. While prior empirical studies generally interpret abnormal trading volume as a measure of the information content of firm disclosures (e.g., Asthana and Balsam 2001; Asthana, Balsam, and Sankaraguruswamy 2004; Leuz and Schrand 2009), it likely also reflects the credibility of the information released. Kim and Verrecchia (1991) model the relation for abnormal trading volume and show that the results for price reactions in Holthausen and Verrecchia (1988), on which the

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ERCs rely, extend to trading volume even when investors are diversely informed. Thus, the conceptual underpinnings discussed in Section 3.1 still apply. If the new audit oversight regime increases reporting credibility, we predict a stronger volume reaction.

The abnormal trading volume proxy also has some empirically desirable properties. Like ERCs, abnormal trading volume around an information event is not anticipatory in nature and can be measured over short intervals. Unlike the ERC, however, it does not have to be estimated from an interaction and can be simply observed at the firm-year level, making it less noisy and allowing us to introduce firm fixed effects. The drawback of this measure is that we cannot compute the news component (or surprise) for the 10-K filing to standardize reactions, as we do for the ERC.

Following prior literature (e.g., Asthana, Balsam, and Sankaraguruswamy 2004; Leuz and Schrand 2009), we calculate abnormal volume, *Abnormal 10-K Volume*, using trading volume in a window beginning 1 trading day prior to the filing and ending 3 trading days after. We normalize raw trading volume by subtracting the mean trading volume in the 45 trading days beginning 5 trading days prior to the 10-K release and dividing by the standard deviation of trading volume (calculated over the same window). We exclude any days in the 3-day earnings announcement window and define *Abnormal 10-K Volume* as the mean of the normalized trading volume in the 5-day (from *t*-1 to *t*+3) window surrounding the 10-K.

We conduct a difference-in-differences analysis of changes in *Abnormal 10-K Volume* around the introduction of the PCAOB regime by estimating the following equation:

$$\begin{aligned} Abnormal\ 10\text{-}K\ Volume_{i,t} = \alpha + \beta_1 Post_t + \beta_2 Treated_i + \beta_3 Post \times Treated_{i,t} + \\ \lambda_n Controls_{i,t} + \gamma_n Fixed\ effects + \varepsilon_{i,t} \end{aligned} \tag{3}$$

We combine the two alternative cutoff dates (completion of fieldwork and release of the inspection report) and pool data across limited and full inspections in a single analysis. We use the

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same treatment and control samples as in our primary analyses for large auditors. 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 between a firm's fiscal year-end and the 10-K release (*Filing delay after FYE*) and between the earnings announcement and the 10-K release (*Filing delay after EA*), following Asthana, Balsam, and Sankaraguruswamy (2004). We also include *Analyst following*, since some sample firms do not have analyst coverage.

We present descriptive statistics for the variables in IA§11. While the sample size is much larger than for the ERC analysis (because we do not require analyst forecasts for these tests), the majority of the sample observations (89%) are from the treatment group. On average, *Abnormal 10-K Volume* is positive, as expected. The median firm files its 10-K 83 days after the fiscal year-end and 36 days after the earnings announcement.

Table 8 presents the regression results. In Column 1, we estimate Equation (3) using ordinary least squares (OLS) and include auditor-, country-, and year-quarter fixed effects. In Column 2, we also include firm fixed effects. In both columns, the treatment effect, *Post*×*Treated*, is positive and significant (at the 5% level or greater). In Column 3, we use a similar approach as in Table 4 and employ CEM based on *Size*, *Beta*, and *Loss*.[35] In Column 3, the coefficient of interest has a magnitude of 0.136, which translates into a 14.6% increase in abnormal trading volume around the release of a firm's 10-K.

In Column 4, following Loughran and McDonald (2014), we include the log of the 10-K file size (*log*(*10-K file size*)) as an additional control for information found in the 10-K and to isolate credibility effects. In Column 5, we include additional controls for SOX provisions 404[b]

[35] We also match on *Loss* in this analysis because (in unreported analyses) we find that the proportion of loss firms are significantly different between the treatment and control samples. In additional (untabulated) analyses, we confirm that matching on *Size* and *Beta* alone does not affect our inferences.

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and 302[a]. Although the magnitudes and standard errors differ slightly across specifications, the results and inferences are robust and similar to those in Column 3.

Overall, our results indicate that the abnormal trading volume around 10-K filings increases after firms' auditors are subject to PCAOB inspections. This result is consistent with an increase in the reporting credibility of audited 10-Ks and corroborates our ERC-based analyses.

5. Conclusion

This paper examines the effects of financial reporting credibility in capital markets. To this end, we analyze whether an increase in audit oversight by a quasi-public regulator increases capital market responses to firms' earnings surprises, as theory would suggest if the new oversight regime enhances the credibility of reported earnings. We use a generalized difference-in-differences research design that exploits the staggered introduction of the PCAOB regime that was established by SOX to replace the prior self-regulatory regime. The introduction of the PCAOB regime affects firms at different times depending on their fiscal year-ends, auditors, and the timing of PCAOB inspections. Consistent with an increase in reporting credibility after the introduction of public audit oversight, we find that capital market responses to earnings surprises increase significantly. The effects are present for firms with Big Four auditors, other annually inspected auditors, and triennially inspected auditors. SOX provisions unrelated to audit oversight do not appear to drive the findings. Corroborating these results, we find that abnormal trading volume reactions to 10-K filings increase after the introduction of the new oversight regime. Overall, our study provides evidence on the capital market effects of the PCAOB regime and 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 reporting credibility is priced by investors in capital markets.

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Despite many sensitivity analyses, our 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 2 years, ERCs are based on investor perceptions and can change as more information about the oversight regime (and reporting and audit quality) becomes publicly available. Second, attributing the credibility effect to public audit oversight depends critically on our ability to control for other concurrent changes in regulation and markets with our difference-in-differences analyses. Third, because ERCs are noisy and difficult to measure, the magnitude of our estimates should be interpreted carefully. Fourth, while we provide evidence that other SOX provisions do not appear to drive our results, it is difficult to rule out the possibility that our results reflect the joint effect of these provisions and public audit oversight. Fifth, our results are relative to the prior peer review regime and do not rule out the possibility that a substantially reformed peer review system could also have increased reporting credibility. Sixth, our study focuses on the capital market benefits of public audit oversight, but does not examine the costs of the new regime. Thus, we do not show *net* benefits. Seventh and finally, our analysis is limited to equity investors. Given the role of auditing in debt contracting, it is conceivable that public audit oversight also provides benefits to (and has costs for) other stakeholders (e.g., Costello and Wittenberg-Moerman 2011; Minnis 2011). We leave this question to future research.

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Appendix A. Details on the Timing of the Introduction of the PCAOB Regime and Identification Strategy

This appendix provides details on the timing of the introduction of the PCAOB audit oversight regime.

Table A1. 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	August 26, 2004
Ernst & Young	June 2003	December 2003	August 26, 2004
KPMG	June 2003	December 2003	August 26, 2004
PricewaterhouseCoopers	June 2003	January 2004	August 26, 2004
Full inspections			
Big Four auditors			
Deloitte & Touche	May 2004	November 2004	October 06, 2005
Ernst & Young	July 2004	December 2004	November 17, 2005
KPMG	June 2004	October 2004	September 29, 2005
PricewaterhouseCoopers	May 2004	January 2005	November 17, 2005
Tier-Two auditors			
BDO	May 2004	July 2004	November 17, 2005
Crowe Chizek	November 2004	December 2004	January 19, 2006
Grant Thornton	May 2004	March 2005	January 19, 2006
McGladrey & Pullen	October 2004	December 2004	November 30, 2005

This table provides the beginning and end dates for PCAOB fieldwork and the inspection report release dates for both limited and full inspections by auditor. We use these dates to define the sample window for each of the respective analyses. Whether a firm's fiscal year-end falls within a given sample window determines which earnings announcement observations are included. However, in setting the pre- and post-period windows, we also consider the earnings announcement timeline of a typical firm in determining which fiscal year-end months to include. We do not use the timing of earnings announcements to determine the sample window because the timing is not fixed, and thus a firm could enter or exit the sample for endogenous reasons related to that timing. Instead, we use fiscal year-end dates, which are fixed. See also Section 3.3 and Figures A1 and A2 for further details on the sample windows.

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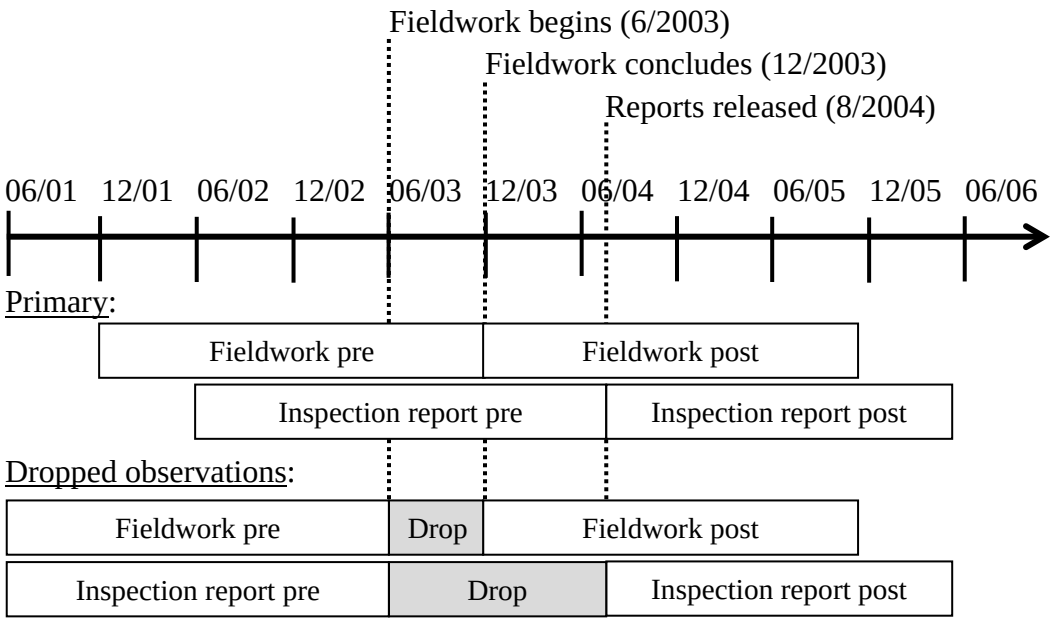
Figure A1. Limited inspections: Treatment timing (specific dates from Deloitte & Touche are presented as an example)



This figure describes the coding of the *Post* variable around the limited inspections. We use two different designs. In the primary design, we use the end of fieldwork across audit firms and the release of the inspection report as alternative cutoff dates to define the sample window. For the limited inspections, using the fieldwork (inspection report) cutoff date, the sample window is defined between December 2001 and November 2005 (June 2002 and May 2006). The sample includes firms whose fiscal year-ends fall into this window. For analyses using the end of fieldwork date, *Post* equals one if a firm's fiscal year ends in the final month of the fieldwork or later. For analyses using the inspection report date, *Post* equals one if a firm's fourth-quarter earnings announcement falls on or after the release of the inspection report. Typically, there are two firm-year observations before and after the relevant cutoff date. In the dropped observations design, to avoid overlapping pre and post periods, we exclude fiscal year-ends occurring during PCAOB fieldwork when the fieldwork end is the cutoff date and exclude fiscal year-ends occurring between the start of fieldwork and the release of the inspection report when the inspection report release is the cutoff date. To reduce sample attrition, we extend the sample window to maintain pre and post periods that are of similar length to the main analyses. Time line dates are presented in the following format: MM/YY.

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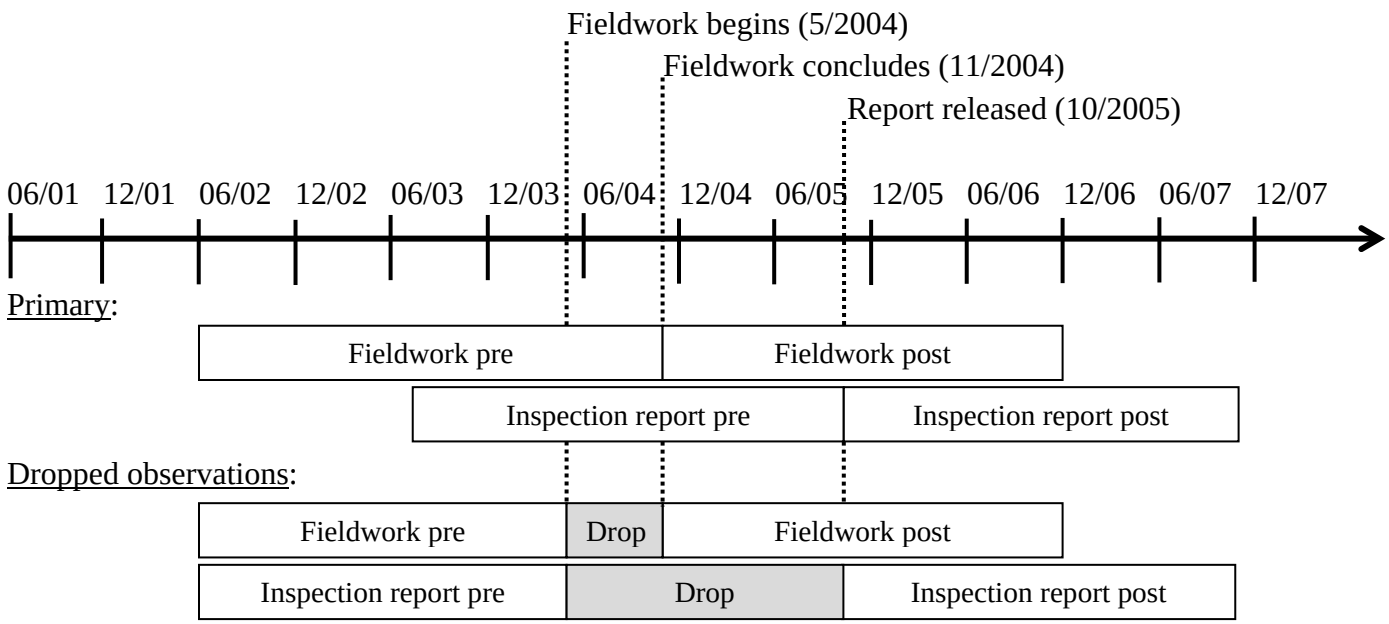
Figure A2. Full inspections: Treatment timing (specific dates from Deloitte & Touche are presented as an example)



This figure describes the coding of the *Post* variable around the full inspections. We use two different designs. In the primary design, we use the end of fieldwork across audit firms and the release of the inspection report as alternative cutoff dates to define the sample window. For the full inspections, using the fieldwork (inspection-report) cutoff date, the sample window is defined between June 2002 and December 2006 (July 2003 and November 2007). The sample includes firms whose fiscal year-ends fall into this window. For analyses using the end of fieldwork date, *Post* equals one if a firm's fiscal year ends in the final month of the fieldwork or later. For analyses using the inspection report date, *Post* equals one if a firm's fourth-quarter earnings announcement falls on or after the release of the inspection report. Typically, there are two firm-year observations before and after the relevant cutoff date. In the dropped observations design, to avoid overlapping pre and post periods, we exclude fiscal year-ends occurring during PCAOB fieldwork when the fieldwork end is the cutoff date and exclude fiscal year-ends occurring between the start of fieldwork and the release of the inspection report when the inspection report release is the cutoff date. To reduce sample attrition, we extend the sample window to maintain pre and post periods that are of similar length to the main analyses. Time line dates are presented in the following format: MM/YY.

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Appendix B

Table B1. Variable definitions

Variables used in calculating earnings response coefficients

$CAR_{i,t}$	A firm's 3 trading day return, centered on the earnings announcement date, less the CRSP market return over the same period. The earnings announcement date is the earliest date available on Compustat or I/B/E/S
$UE_{i,t}$	The difference between the I/B/E/S annual EPS and the median I/B/E/S forecast of annual EPS from each analyst's most recent forecast in the window starting 95 calendar days before and ending 3 days before the earnings announcement, scaled by the CRSP price 2 days before the earnings announcement. We supplement the triennially inspected auditor analysis 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 the window starting 360 calendar days before and ending 3 days before 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 after 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-ends in the same month as the final month of fieldwork (as indicated in Table A1 in Appendix A) or later, and zero otherwise; (2) triennially inspected auditor full inspection fieldwork: *Post* equals one if a firm's fiscal year-ends after the auditor-specific fieldwork end date plus 30 days, and zero otherwise; and (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 Table A1 in Appendix 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

Control variables

Analyst following$_{i,t}$	The number of unique analysts who issue at least one forecast on I/B/E/S in a window starting 360 calendar days before and ending 3 days before 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 Kenneth French's data library
Filing delay after $EA_{i,t}$	The number of days between the earnings announcement date (the earliest date available on Compustat or I/B/E/S) and the filing date of the 10-K (the earliest date reported by Audit Analytics or WRDS SEC Analytics). We use many variations of 10-K filings (e.g., 10-K405, 10-KSB, 20-F)
Filing delay after $FYE_{i,t}$	The number of days between the firm's fiscal year-end (from Compustat) and the filing of the 10-K (based on the earliest date reported by Audit Analytics or WRDS SEC Analytics). We use many variations of 10-K filings (e.g., 10-K405, 10-KSB, 20-F)
$Leverage_{i,t}$	The ratio of total liabilities to total equity, measured at the fiscal year-end, from Compustat
log(10-K file size)	The natural log of the file size of the firm's 10-K SEC filing, from WRDS SEC Analytics. We use many variations of 10-K filings (e.g., 10-K405, 10-KSB, 20-F)

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Loss$_{i,t}$	An indicator variable coded as one when basic earnings per share excluding extraordinary items from 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
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 natural log of the 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 coded 1 only 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 in a window starting 1 day before the 10-K filing date and ending 3 days after. Abnormal trading volume is defined as raw volume less mean daily volume over a window starting 49 days before and ending 5 days before the annual financial statement 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, using each analyst's most recent forecast in a window beginning 95 days before and ending 3 days before the earnings announcement, scaled by the CRSP price from 2 days before 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 calendar 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 of information reflected in equity prices over a firm's fiscal year, calculated as the sum of the absolute value of daily market-adjusted CRSP returns starting 345 calendar days before and ending the day before the earnings announcement window, divided by the same value 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 $$100 \cdot \frac{\sum_{d=-345}^{0} \left\lvert r_{i,d} - r_{M,d} \right\rvert}{\left\lvert \widehat{\alpha_{lag2}} + UE_i \cdot \widehat{ERC_{lag2}} + Loss_i \cdot \widehat{\beta_{lag2}^{Loss}} + UE_i \cdot Loss_i \cdot \widehat{\beta_{lag2}^{LossERC}} \right\rvert + \sum_{d=-345}^{0} \left\lvert r_{i,d} - r_{M,d} \right\rvert}.$$ Returns are from CRSP, and *d* represents the number of calendar days relative to 2 trading days prior to the earnings announcement. To increase the precision of the measurement, we allow separate ERC coefficients for profits and losses estimated from cross-sectional regressions 2 years prior to *t*
Scaled raw accruals$_{i,t}$	The difference between net income and cash flow from operations scaled by average total assets, from Compustat

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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) and given by the equation: $$-1 \cdot \frac{\sum_{d=-345}^{0} \left\lvert \log(p_{d=0}) - \log(p_d) \right\rvert}{\sum_{d=-345}^{0} 1_d}.$$ We multiply by -1 so that the measure is increasing in timeliness. Prices are from CRSP, and *d* represents the number of calendar days relative to 2 trading days before 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.

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Figure 1. Mapping the estimated treatment effect by event time

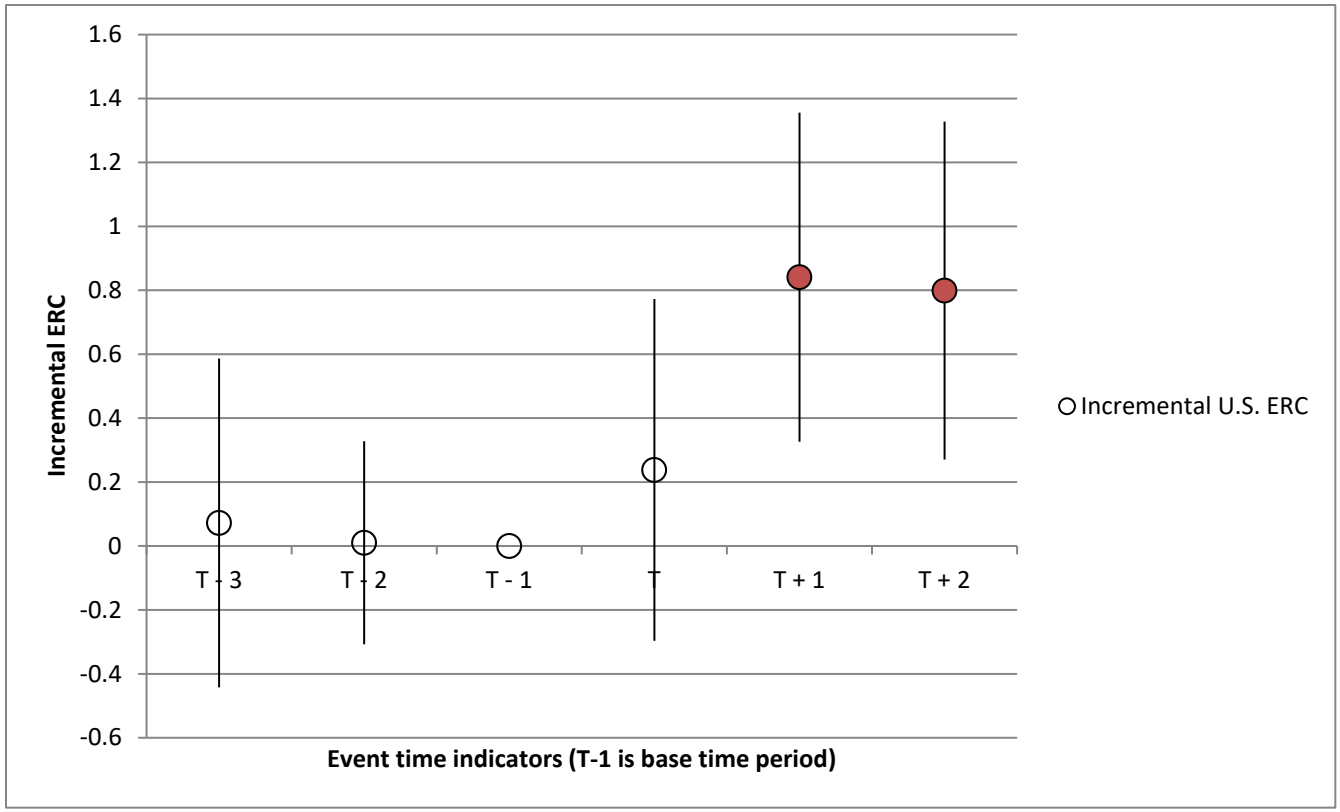

Figure 1 presents simultaneously estimated trends in earnings response coefficients (ERCs) for the combined sample using profit firms only (i.e., *Loss* = 0), which stacks the limited and full inspections analyses for each cutoff date (i.e., the end of fieldwork and report release) using the dropped observation design (see Figures A1 and A2 in Appendix A). We use the sample from our primary analyses (i.e., Table 3, panel A of the manuscript) plus years "T-3" and "T+2" to better map out the pretreatment period and treatment response. Each unshaded (shaded) dot on the graph represents an insignificant (significantly positive) regression coefficient for U.S. firms in event time (i.e., *UE*× *Treated* interacted with event time dummies) from a robust regression estimation of Equation (1). We include auditor and country fixed effects interacted with *UE*. Each line bar represents 2 standard errors on either side of the coefficient. We calculate robust, firm-level clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression. Table B1 in Appendix B defines each variable in detail.

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Table 1. Sample composition

A. Number of unique issuers by auditor, inspection type, and sample window cutoff date

	Unique firms					Firm-years
	Limited inspections		Full inspections		Combined	
	(1)	(2)	(3)	(4)	(5)	(6)
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 sample window cutoff date. Columns 1 through 4 report the count of unique firms with available data for each of the four separate sample windows (limited inspection fieldwork end, limited inspection report release, full inspection fieldwork end, and full inspection report release). We provide the dates (month and year) of these four events for annually inspected auditors in Table A1 in Appendix A. In Column 5, we report the number of unique firms in the combined analysis in which we stack all inspections and sample windows. Thus, the combined analysis includes the same firm up to 4 times. In Column 6, we report the number of firm-years for the combined analysis. We include any firm fiscal year-end that falls into a sample window defined for the respective cutoff date (see Section 3.4 and Figures A1 and A2 in Appendix A for details). We require that a firm have available data on Audit Analytics, Compustat, CRSP, and I/B/E/S.

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Table 1. Sample composition (continued)

B. Number of newly treated, triennially inspected auditors and firm-years

	Fieldwork		Inspection reports	
Calendar year	**Newly inspected auditors (1)**	**Unique firms (2)**	**Newly reported-on auditors (3)**	**Unique firms (4)**
2004	24	98	–	–
2005	54	98	36	68
2006	73	297	44	131
2007	14	32	56	179
Other	4	4	32	150
Total	**169**	**529**	**169**	**529**
Total firm-years		**1,338**		**1,338**

Panel B 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) the auditor has registered with the PCAOB. At the end of the sample period, all but 4 auditors in our sample have been inspected; 32 had not yet had an inspection report released. Column 1 (2) reports the number of newly treated auditors by calendar year, using the inspection fieldwork (report release) as the cutoff date. Column 3 (4) reports the number of unique client firms associated with the newly inspected auditors. In the last row, we report the number of firm-years contributed by these firms.

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Table 2. Descriptive statistics for the limited, full, and triennial inspection samples

A. Annually inspected U.S. auditors (treatment group)

Variable	N	Mean	SD	P25	Median	P75
CAR	37,001	0.002	0.064	-0.030	0.001	0.034
UE	37,001	0	0.008	-0.001	0	0.002
Loss	37,001	0.182	0.386	0	0	0
Size	37,001	7.018	1.518	5.948	6.926	7.990
Market-to-book	37,001	2.973	2.639	1.594	2.254	3.487
Leverage	37,001	2.654	3.991	0.506	1.153	2.643
Persistence	37,001	0.282	0.446	0	0.285	0.553
Beta	37,001	1.092	0.548	0.708	1.033	1.436
Forecast	36,659	0.032	0.074	0.025	0.046	0.063
Timeliness	36,596	-0.201	0.157	-0.256	-0.155	-0.093
Relative information	36,586	99.81	0.144	99.74	99.84	99.92
Scaled raw accruals	34,855	-0.055	0.076	-0.084	-0.046	-0.014
Earnings guidance	37,001	0.530	0.499	0	1	1
Guidance bundle	37,001	0.394	0.489	0	0	1
Post	37,001	0.504	0.500	0	1	1
Timing: Treatment to first EA (in days)	12,436	241.0	193.3	88	165	386

Table 2 presents descriptive statistics for the variables used in the limited, full, and triennial inspection analyses. Table B1 in Appendix B defines each variable in detail. We include observations from limited 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 U.S. annually inspected Big Four or Tier-Two auditors (i.e., the treatment group in the limited and full inspection analyses). The sample includes 37,001 firm-year observations. The last row in the 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.

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Table 2. Descriptive statistics for the limited, full, and triennial inspection samples (continued)

B. Non-U.S. auditors of U.S. cross-listed firms (control group)

Variable	N	Mean	SD	P25	Median	P75
CAR	3,765	-0.001	0.058	-0.031	-0.001	0.030
UE	3,765	-0.001	0.012	-0.003	0	0.003
Loss	3,765	0.171	0.376	0	0	0
Size	3,765	8.102	1.848	6.733	8.243	9.580
Market-to-book	3,765	2.914	2.414	1.510	2.299	3.597
Leverage	3,765	2.711	5.045	0.470	1.118	2.198
Persistence	3,765	0.257	0.529	-0.001	0.260	0.544
Beta	3,765	0.939	0.566	0.529	0.833	1.292
Forecast	3,694	0.042	0.064	0.026	0.050	0.074
Timeliness	3,733	-0.218	0.160	-0.279	-0.177	-0.106
Relative information	3,714	99.83	0.125	99.75	99.85	99.92
Scaled raw accruals	3,625	-0.063	0.078	-0.097	-0.055	-0.020
Earnings guidance	3,765	0.148	0.355	0	0	0
Guidance bundle	3,765	0.098	0.297	0	0	0
Post	3,765	0.538	0.499	0	1	1

Panel B presents descriptive statistics for U.S. cross-listed firms with non-U.S. Big Four or Grant Thornton auditors that have annually inspected global network affiliates (i.e., the control group in the limited and full inspection analyses). The sample includes 3,765 firm-year observations.

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Table 2. Descriptive statistics for the limited, full, and triennial inspection samples (continued)

C. U.S. and Non-U.S. auditors control variable balance

	U.S.	**Non-U.S.**	**Unweighted**		**CEM-weighted** *Size, Beta*	
N=	37,001	3,765	40,766		39,843	
Variable	**Mean**	**Mean**	**Diff.**	***t*-stat**	**Diff.**	***t*-stat**
Loss	0.182	0.171	0.011	0.76	–	
Size	7.018	8.102	-1.084	-11.50	-0.014	-0.18
Market-to-book	2.973	2.914	0.060	0.55	–	
Leverage	2.654	2.711	-0.056	-0.22	–	
Persistence	0.282	0.257	0.025	1.13	–	
Beta	1.092	0.939	0.154	6.11	-0.002	-0.07
Dropped obs. from						
U.S. sample			0		895	
Non-U.S. sample			0		28	

Panel C presents differences in means for firms with U.S. versus non-U.S. auditors (i.e., the treatment versus control group in the limited and full inspection analyses). We show unweighted and, where the unweighted differences are statistically different from zero, weighted differences. Weights are calculated using coarsened exact matching (CEM), with 20 bins for the control variables listed in the table header. The matching procedure drops observations when the bins only contain U.S. or non-U.S. subsamples. All *t*-statistics are based on standard errors clustered at the firm level. **p* < .1; ***p* < .05; ****p* < .01 (two sided).

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Table 2. Descriptive statistics for the limited, full, and triennial inspection samples (continued)

D. Triennially inspected auditors

Variable	N	Mean	SD	P25	Median	P75
CAR	1,338	-0.005	0.070	-0.036	-0.003	0.027
UE	1,338	-0.009	0.033	-0.006	0	0.001
Loss	1,338	0.254	0.436	0	0	1
Size	1,338	4.800	0.890	4.309	4.831	5.405
Market-to-book	1,338	2.862	4.167	1.374	1.890	3.046
Leverage	1,338	4.983	5.133	0.431	2.167	9.576
Persistence	1,338	0.648	0.605	0.156	0.491	1.083
Beta	1,338	0.316	0.587	0	0.328	0.643
Fieldwork timing:						
Post	1,338	0.528	0.499	0	1	1
Timing: Treatment to first EA (in days)	706	543.3	343.8	245	505.5	677
Report release timing:						
Post	1,338	0.297	0.457	0	0	1
Timing: Treatment to first EA (in days)	397	387.1	288.2	130	335	581
Dropped observation timing:						
Post	1,013	0.392	0.488	0	0	1

Panel D presents descriptive statistics for firms with triennially inspected auditors. The sample includes 1,338 firm-year observations. We provide descriptive information about the timing assigned to *Post* for the fieldwork, report release, and dropped observation designs. In the dropped observation design, we lose 325 earnings announcements that are between the beginning of fieldwork and the report release (24.3% of the sample). *Timing: Treatment to first EA (in days)* is the average number of days from the respective cutoff date (the end of fieldwork date plus 30 days or inspection report release) to the (treated) firm's first earnings announcement.

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Table 3. Changes in reporting credibility around the introduction of the PCAOB regime for limited and full inspections

A. Analyses using the primary design

	(1)	(2)	(3)	(4)
	Limited inspections		**Full inspections**	
Dependent variable: *CAR*	**Fieldwork**	**Reports**	**Fieldwork**	**Reports**
UE×Post×Treated	**0.336**	**0.566***	**1.600*****	**1.149****
	(1.094)	**(1.881)**	**(4.978)**	**(2.141)**
UE×Loss	-0.369***	-0.627***	-0.899***	-1.745***
	(-2.617)	(-4.645)	(-6.547)	(-8.008)
UE×Size	-0.008	-0.008	0.012	-0.230***
	(-0.188)	(-0.200)	(0.272)	(-3.202)
UE×Market-to-book	0.031	0.064**	0.047**	0.085**
	(1.136)	(2.447)	(2.051)	(2.561)
UE×Leverage	-0.011	-0.029**	-0.026**	-0.022
	(-0.972)	(-2.559)	(-2.435)	(-1.101)
UE×Persistence	-0.038	-0.045	-0.081	-0.019
	(-0.330)	(-0.560)	(-0.724)	(-0.092)
UE×Beta	0.289**	0.334***	0.224**	0.227
	(2.211)	(2.711)	(2.055)	(1.426)
Firm characteristics	Yes	Yes	Yes	Yes
Treatment indicators	Yes	Yes	Yes	Yes
UE×Treatment indicators	Yes	Yes	Yes	Yes
Fixed effects	Auditor, country, & year-quarter	Auditor, country, & year-quarter	Auditor, country, & year-quarter	Auditor, country, & year-quarter
UE×Fixed effects	Yes	Yes	Yes	Yes
Observations	9,308	9,799	11,833	9,826
Adjusted *R*-squared	.048	.037	.041	.067

Table 3 presents separate analyses for each inspection event (limited and full) and each sample window cutoff date (end of fieldwork and report release). Panel A reports results for our analysis using the primary design as described in Figures A1 and A2 in Appendix A. Following Equation (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 most of the interactions among these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Table B1 in Appendix B defines each variable in detail. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and the respective fiscal year-end, as well as the interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Column 1, we examine the changes in ERCs following fieldwork completion for limited inspections. In Column 2, we examine the changes in ERCs following inspection report releases for limited inspections. In Column 3, we examine the changes in ERCs following fieldwork completion for full inspections. In Column 4, we examine the changes in ERCs following inspection report releases for full inspections. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. For all robust regressions, we calculate firm-level clustered standard errors using a weighted least squares regression based on the weights (and coefficients) generated by the rreg command. *$p < .1$; **$p < .05$; ***$p < .01$ (two sided).

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Table 3. Changes in reporting credibility around the introduction of the PCAOB regime for limited and full inspections (continued)

B. Analyses for the dropped observations design

	(1)	(2)	(3)	(4)
	Limited inspections		Full inspections	
Dependent variable: *CAR*	**Fieldwork**	**Reports**	**Fieldwork**	**Reports**
UE×Post×Treated	**0.414**	**0.513***	**1.620*****	**2.145*****
	(1.310)	**(1.691)**	**(4.965)**	**(4.940)**
UE×Loss	-0.541***	-0.544***	-0.809***	-1.076***
	(-4.033)	(-4.285)	(-5.760)	(-5.881)
UE×Size	0.012	0.039	0.010	0.019
	(0.321)	(1.055)	(0.229)	(0.303)
UE×Market-to-book	0.030	0.060**	0.048**	0.076**
	(1.184)	(2.270)	(2.080)	(2.400)
UE×Leverage	-0.014	-0.028**	-0.025**	-0.035**
	(-1.163)	(-2.457)	(-2.355)	(-2.348)
UE×Persistence	0.006	0.026	-0.109	-0.038
	(0.078)	(0.337)	(-0.968)	(-0.233)
UE×Beta	0.363***	0.255**	0.227**	0.290*
	(3.108)	(2.352)	(2.031)	(1.675)
Firm characteristics	Yes	Yes	Yes	Yes
Treatment indicators	Yes	Yes	Yes	Yes
UE×Treatment indicators	Yes	Yes	Yes	Yes
Fixed effects	Auditor, country, & year-quarter	Auditor, country, & year-quarter	Auditor, country, & year-quarter	Auditor, country, & year-quarter
UE×Fixed effects	Yes	Yes	Yes	Yes
Observations	8,775	9,191	11,017	9,528
Adjusted *R*-squared	.041	.034	.043	.060

Panel B reports results for our analysis using the dropped observations design as described in Figures A1 and A2 in Appendix A. Following Equation (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 most of the interactions among these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Table B1 in Appendix B defines each variable in detail. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and the respective fiscal year-end, as well as the interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Column 1, we examine the changes in ERCs following fieldwork completion for limited inspections. In Column 2, we examine the changes in ERCs following inspection report releases for limited inspections. In Column 3, we examine the changes in ERCs following fieldwork completion for full inspections. In Column 4, we examine the changes in ERCs following inspection report releases for full inspections. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. 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. *$p < .1$; **$p < .05$; ***$p < .01$ (two sided).

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Table 4. Changes in reporting credibility around the introduction of the PCAOB regime in the combined analyses

A. Main results combining inspection events and cutoff dates

Dependent variable: *CAR*	**(1) Primary design**	**(2) Dropped obs. design**	**(3) Primary design CEM**	**(4) Primary design loss interactions**
UE×Post×Treated	**0.788*** **	**0.874*** **	**0.719** **	**0.942*** **
	(3.473)	**(3.543)**	**(2.230)**	**(3.589)**
UE×Loss	-0.761***	-0.699***	-0.755***	-0.093
	(-6.226)	(-5.857)	(-5.787)	(-0.378)
UE×Size	-0.023	0.016	-0.033	-0.022
	(-0.610)	(0.441)	(-0.777)	(-0.604)
UE×Market-to-Book	0.054**	0.050**	0.061**	0.064***
	(2.384)	(2.335)	(2.557)	(2.845)
UE×Leverage	-0.024**	-0.025**	-0.032***	-0.024**
	(-2.294)	(-2.371)	(-3.028)	(-2.271)
UE×Persistence	-0.034	-0.009	-0.090	-0.043
	(-0.405)	(-0.124)	(-1.028)	(-0.501)
UE×Beta	0.301***	0.284***	0.378***	0.247**
	(2.955)	(2.901)	(3.243)	(2.396)
UE×Loss×Post×Treated	–	–	–	-0.803
				(-1.520)
Firm characteristics	Yes	Yes	Yes	Yes
Treatment indicators	Yes	Yes	Yes	Yes
UE×Treatment indicators	Yes	Yes	Yes	Yes
Fixed effects	Auditor, country, & year-quarter	Auditor, country, & year-quarter	Auditor, country, & year-quarter	Auditor, country, & year-quarter
UE×Fixed effects	Yes	Yes	Yes	Yes
Loss & *UE×Loss* interacted with treatment indicators	No	No	No	Yes
Observations	40,766	38,511	39,843	40,766
Adjusted *R*-squared	.051	.049	.058	.052

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Table 4. Changes in reporting credibility around the introduction of the PCAOB regime in the combined analyses

B. Main results, including additional firm-level fixed effects

Dependent variable: *CAR*	**(1)**	**(2)**	**(3)**	**(4)**
	Primary design		**6-year design**	
	Firm effects	**Interacted firm-group effects**	**Firm effects**	**Interacted firm-group effects**
UE×Post×Treated	**0.674****	**0.784*****	**0.783*****	**0.791*****
	(2.278)	**(2.959)**	**(2.760)**	**(3.101)**
UE×Loss×Post×Treated	-2.162***	-0.136	-0.950*	-0.105
	(-3.237)	(-0.249)	(-1.672)	(-0.234)
Firm characteristics	Yes	Yes	Yes	Yes
UE×Firm characteristics	Yes	Yes	Yes	Yes
Treatment indicators	Yes	Yes	Yes	Yes
UE×Treatment indicators	Yes	Yes	Yes	Yes
Firm-level main effects only	Firm	No	Firm	No
Interacted (and main) effects	Auditor, country, & year-quarter	Firm-group, auditor, country, & year-quarter	Auditor, country, & year-quarter	Firm-group, auditor, country, & year-quarter
UE×Interacted Effects	Yes	Yes	Yes	Yes
Loss & *UE×Loss* interacted with treatment indicators	Yes	Yes	Yes	Yes
Observations	40,766	40,766	58,554	58,554
Adjusted *R*-squared	.272	.084	.236	.077
Within *R*-squared	.013	–	.010	–

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Table 4 presents analyses combining inspection events (limited and full) and the cutoff dates (end of fieldwork and report release). Following Equation (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 most of the interactions among these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Table B1 in Appendix B defines each variable in detail. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and the respective fiscal year-end, as well as the interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. 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. In panel A of Column 1, we estimate the primary design using the combined sample. In Column 2, we estimate the dropped observations design using the combined sample. We describe these designs in Figures A1 and A2 in Appendix A. In Column 3, we estimate the primary design using a CEM sample with 20 bins for the control variables *Size* and *Beta*; unmatched bins result in 923 fewer observations. In Column 4, we allow for heterogeneous treatment among profit and loss firms by interacting the treatment indicators with the *Loss* control variable. In all other ways, Column 4 is consistent with Column 1. In panel B of Columns 1 and 2, we use our primary design and combined sample (i.e., up to four firm-year observations around each of the inspection events and cutoff dates). In Columns 3 and 4, we use the combined sample, but add 1 year before and after the respective event date, resulting in a 6-year series (as opposed to a 4-year series). Columns 1 and 3 otherwise follow the design of panel A, Column 4, except for the inclusion of firm fixed effects that are introduced as main effects only. That is, firm-level main effects are not interacted with *UE*. In Columns 2 and 4, we follow the design of panel A, Column 4, and include firm-group fixed effects, which are generated by forming 100 portfolios (10-by-10) using the controls *Size* and *Beta* from the first year that a firm enters the sample (see Internet Appendix §9 for more details on groups). The firm-group fixed effects are included in the model as main effects and with respective interactions, including *UE*. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ (two sided).

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Table 5. Tests for other concurrent changes in the information environment around the introduction of the PCAOB

Dependent variable:	**(1)** ***UE***	**(2)** ***Forecast***	**(3)** ***Timeliness***	**(4)** ***Relative information***	**(5)** ***Scaled raw accruals***	**(6)** ***Earnings guidance***	**(7)** ***Guidance bundle***
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
Treatment indicators	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
Loss interacted with treatment indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	40,766	40,353	40,329	40,298	38,480	40,766	40,766
Adjusted *R*-squared	0.032	0.410	0.249	0.494	0.147	0.137	0.114

Table 5 presents tests for other concurrent changes in the information environment around the introduction of the PCAOB inspection regime. We estimate the treatment effects separately for profit and loss firms, but only report the effects for profit firms. In Columns 1 (2, 3, 4, 5, 6, 7), we regress *UE* (*Forecast*, *Timeliness*, *Relative information*, *Scaled raw accruals*, *Earnings guidance*, and *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*. Table B1 in Appendix B defines each variable in detail. 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. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. *$p < .1$; **$p < .05$; ***$p < .01$ (two sided).

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Table 6. Are results driven by changes in reporting incentives or by 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**	**Nonaccelerated**	**Only accelerated**	**Excluding 404[b]**	**Only 404[b]**	**Controlling for SOX**
UE×Post×Treated	**1.030*** **	**0.492**	**1.139** **	**0.871*** **	**0.923*** **	**0.234+**	**0.921*** **
	(3.662)	**(1.415)**	**(2.570)**	**(3.102)**	**(3.153)**	**(0.632)**	**(3.306)**
UE×SOX404b	–	–	–	–	–	–	0.275
							(1.375)
UE×SOX302a	–	–	–	–	–	–	-0.900***
							(-3.276)
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UE×Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UE×Treatment indicators	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
UE×Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loss & *UE×Loss* interacted with Treatment indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	34,736	9,795	9,684	34,847	24,867	19,664	40,766
Adjusted *R*-squared	.054	.075	.091	.050	.057	.062	.053

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Table 6 presents sensitivity analyses that examine the role of changes in market-based reporting incentives and other provisions of SOX. We separately estimate the treatment effects for loss and profit firms, though we only report the effects for profit firms. Following Equation (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*. Table B1 in Appendix B defines each variable in detail. 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. In Columns 1 and 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 and 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 and 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). As additional controls, in Column 7, we include the indicator variables *SOX404b* and *SOX302a* and their interactions with *UE*. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. + indicates significance (two-sided) at the 10% level for tests of the 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. *$p < .1$; **$p < .05$; ***$p < .01$ (two sided).

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Table 7. Changes in reporting credibility around the introduction of PCAOB triennial inspections

Dependent variable: *CAR*	**(1) Fieldwork**	**(2) Report release**	**(3) Report release-SOX**	**(4) Dropped observation**	**(5) Firm effects**	**(6) Interacted firm-groups**
UE×Post	**0.789****	**1.063****	**0.915****	**1.022****	**0.531**	**1.077*****
	(2.125)	**(2.387)**	**(2.231)**	**(2.247)**	**(0.566)**	**(2.380)**
UE×SOX404b	–	–	-0.566	–	–	–
			(-1.595)			
UE×SOX302a	–	–	-0.120	–	–	–
			(-0.604)			
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes
UE×Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Treatment indicator (*Post*)	Yes	Yes	Yes	Yes	Yes	Yes
Firm-level main effects only	No	No	No	No	Firm	No
Interacted (and main) effects	Auditor & year-quarter	Auditor & year-quarter	Auditor & year-quarter	Auditor & year-quarter	Auditor & year-quarter	Firm-group, auditor, & year-quarter
UE×Interacted effects	Yes	Yes	Yes	Yes	Yes	Yes
Loss & *UE×Loss* interacted with treatment indicator	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,338	1,338	1,338	1,013	1,338	1,338
Adjusted *R*-squared	.682	.681	.681	.823	.699	.694

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Table 7 presents results for the analysis of the change in reporting credibility around the introduction of triennial PCAOB inspections. Following Equation (2), we regress cumulative abnormal returns (*CAR*) on *UE*, an indicator for PCAOB inspections (i.e., *Post*), control variables, fixed effects, the interactions of *UE* with the control variables, the interacted fixed effects, and the treatment indicator (as shown in the table footer). Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Table B1 in Appendix B defines each variable in detail. In all columns, we include fixed effects for the auditor and the year-quarter of the respective fiscal year-end. We estimate robust regressions. We separately estimate the treatment effect for profit and loss firms by including additional interactions. 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 fieldwork completion). 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 Columns 3 through 6, we perform tests similar to the specifications for annually inspected auditors. In Columns 3, 5, and 6, we use the report release design from Column 2. As additional controls, in Column 3, we add the indicator variables *SOX404b* and *SOX302a* and their interactions with *UE*. In Column 4, we examine the dropped observation design for triennially inspected auditors. In Column 5, we include firm-level main effects that are not interacted with *UE*. In Column 6, we include firm-group fixed effects interacted with *UE*; these are generated by forming 16 portfolios (4-by-4) using *Size* and *Beta* from the first year that a firm enters the sample (see Internet Appendix §9 for more details on groups). The firm-group fixed effects are included in the model as main effects and with their respective interactions, including *UE*. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. We calculate firm-level clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ (two sided).

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Table 8. Changes in abnormal trading volume around 10-K filings after the introduction of the PCAOB regime

Dependent variable: ***Abnormal 10-K volume***	**(1)** **OLS**	**(2)** **OLS**	**(3)** **CEM: WLS**	**(4)** **CEM: WLS**	**(5)** **CEM: WLS**
Post×Treated	**0.088****	**0.097*****	**0.136*****	**0.126****	**0.100****
	(2.552)	**(2.748)**	**(2.973)**	**(2.551)**	**(2.146)**
Size	0.016**	-0.024	-0.018	-0.016	-0.017
	(2.476)	(-0.990)	(-0.710)	(-0.638)	(-0.685)
M2B	-0.009***	-0.002	0.000	-0.000	0.000
	(-3.457)	(-0.359)	(0.013)	(-0.016)	(0.034)
Leverage	0.009***	0.012**	0.009	0.009	0.009
	(5.256)	(2.089)	(1.575)	(1.525)	(1.592)
Beta	0.076***	0.077***	0.058**	0.057**	0.054**
	(5.460)	(3.679)	(2.534)	(2.463)	(2.347)
Loss	-0.075***	-0.061**	-0.051*	-0.057**	-0.051*
	(-4.126)	(-2.219)	(-1.783)	(-1.961)	(-1.780)
Filing delay after FYE	0.004***	0.003***	0.004***	0.003***	0.004***
	(7.920)	(5.065)	(5.076)	(3.888)	(5.074)
Filing delay after EA	-0.006***	-0.005***	-0.005***	-0.006***	-0.005***
	(-15.081)	(-7.570)	(-7.284)	(-7.954)	(-7.227)
Analyst following	-0.002*	0.000	0.000	0.001	-0.000
	(-1.862)	(0.020)	(0.085)	(0.299)	(-0.063)
log(10-K file size)	–	–	–	0.016	–
				(1.159)	
SOX404b	–	–	–	–	0.051
					(1.631)
SOX302a	–	–	–	–	0.076
					(1.345)
Treatment indicators	Yes	Yes	Yes	Yes	Yes
Fixed effects	Auditor, country, & year-quarter	Firm & year-quarter	Firm & year-quarter	Firm & year-quarter	Firm & year-quarter
Observations	68,830	68,830	67,178	65,051	67,178
Adjusted *R*-squared	.038	.244	.259	.260	.259

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Table 8 presents results for the analysis of changes in abnormal trading volume around 10-K filings after the introduction of the PCAOB regime. Following Equation (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). Table B1 in Appendix B defines each variable in detail. We include fixed effects for the auditor (at the 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 report the baseline specification estimated using OLS. We repeat this in Column 2, but substitute firm fixed effects for auditor and country fixed effects. In Column 3, we report the primary design with weighted least squares (WLS) using weights from a coarsened exact matching (CEM) procedure that uses 20 bins for the control variables *Size*, *Beta*, and *Loss*; unmatched bins result in 1,652 fewer observations. In Column 4, we use WLS with the CEM weights from Column 3 and include continuous variable *log(10-K file size)*. In Column 5, we use WLS with the CEM weights from Column 3 and include the indicator variables *SOX404b* and *SOX302a*. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. *$p < .1$; **$p < .05$; ***$p < .01$ (two sided).

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Internet Appendix:

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

This appendix provides descriptive information and supplemental analyses for our study "Public Audit Oversight and Reporting Credibility: Evidence from the PCAOB Audit Inspection Regime."

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Section 1: Institutional analysis

This appendix provides supplemental descriptive information and institutional details about the PCAOB regime as well as the prior AICPA peer-review program. We summarize these institutional details in Section 2 of the Manuscript. This descriptive analysis provides the institutional underpinnings for our empirical analysis in the Manuscript.

In the Manuscript, we analyze the link between the introduction of the PCAOB regime and changes in the market's credibility assessment of audited financial reports. In this appendix, we examine and provide descriptive evidence for the mechanism and for each of the conditions that must be met for our empirical link (documented in the Manuscript) to be plausible. Specifically, (1) the PCAOB's inspections must be meaningfully different than the prior peer-review regime; (2) the PCAOB inspections must identify meaningful weaknesses or deficiencies in the way audits are conducted, leading to subsequent improvements that persist beyond a single engagement; (3) the market and investors learn about these changes; and (4) these effects are empirically separable from other contemporaneous events.

Part 1: Comparison of the AICPA peer review program and PCAOB regime
In this section, we discuss major differences between the AICPA peer review program and the PCAOB regime.

Part 2: Penalties issued by the PCAOB
In this section, we provide a list of the possible penalties that the PCAOB can impose for audit firm deficiencies as well as descriptive information on the frequency (by year) with which these penalties have been issued. The purpose is to provide descriptive evidence on one important difference between the PCAOB regime and the prior AICPA peer review program.

Part 3: Analysis of restatements included in PCAOB inspection reports
In this section, we provide a descriptive analysis of restatements arising from PCAOB inspections. We also provide details on the nature of restatements for large audit firms in the 2004 inspection year. This illustrates that the new regime identifies meaningful weaknesses and deficiencies in the way audits are conducted.

Part 4: Details on the remediation process following PCAOB inspections
In this section, we provide details of the remediation process following PCAOB inspections. This includes specific examples of firms' responses and remedial actions. This illustrates that the PCAOB regime leads to subsequent changes in the way audits are conducted.

Part 5: Survey of publicly-available information about the PCAOB regime
In this section, we provide examples of publicly-available information through which capital-market participants could learn about the scope and effectiveness of the PCAOB regime, and hence could update their beliefs about the credibility of reported earnings.

Part 6: Timing of concurrent regulatory changes
In this section, we discuss the timing of other regulatory changes that occurred around the introduction of the PCAOB regime in order to assess whether these changes are plausible alternative explanations for our results or could bias against our findings.

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Part 1: Comparison of the AICPA peer review program and PCAOB regime

In this section, we discuss major differences between the AICPA peer-review program and the PCAOB regime. One major difference is that (even for large auditors with more than 100 public issuers) peer reviews were conducted only every three years under the AICPA, whereas the PCAOB conducts annual inspections for large auditors. In addition to frequency, there are three primary areas of difference on which we focus: 1) program independence, 2) program scope (or focus), and 3) program penalties. Within the PCAOB, inspections and enforcement are separate. We discuss them jointly here because we consider them to be complimentary changes in oversight.

Data sources: While the AICPA does not make available the historic information on its peer review program, academics have catalogued its rules and output during the pre-SOX period. The peer review program changed dramatically (in both rules and output) following the introduction of the PCAOB, both as a result of the perceived failures in the peer review program as well as the perceived overlap in outputs with the PCAOB inspection program. For data on the PCAOB, we primarily draw from PCAOB publications (first made available on the website during the first limited and full inspections in 2004 and hence observable to capital market participants). For the peer-review program, we attempt to provide sources that are independent of the PCAOB.

Tables IA1A-IA1D present quotations from academic publications: Table IA1A about the independence of the AICPA peer-review program; Table IA1B about the focus and scope of the peer-review regime; Table IA1C on the focus and scope of the PCAOB review regime; and Table IA1D about the scope of penalties under the peer-review regime. For a discussion and summary of PCAOB penalties, see Section 2.

Overall, the comparison presented in this section suggests that the differences between the AICPA and the PCAOB regime are substantial; the PCAOB regime results in greater independence, a larger array of/more frequently employed penalties, and a broader program scope.

Table IA1A - Peer-review program independence

Author(s)	Year	Journal	Quote
Fogarty	1996	Accounting Organizations and Society	Since peer reviewers are not centrally assigned, but instead are individually negotiated, nothing prevents the continuation of a reviewer from a pre-engagement appointment as a peer review consultant. This very likely shades what ultimately becomes an official program record and partially explains the high unqualified review rate (Wallace, 1991; Oliverio & Newman, 1993).
Public Oversight Board	2002	Self-Published	The current system of self-regulation of the accounting profession has significant problems. First, the funding of the [Public Oversight Board] (POB) is subject to control by the [audit] firms through the [Securities and Exchange Commission Practice Section] (SECPS), which in the past has cut off that funding in an effort to restrict POB activities…Other problems include the fact that the current governance structure does not have the weight of a congressional mandate behind it. There is also a perceived lack of candid and timely public reporting of why and how highly publicized audit failures and fraud occurred and what actions have or will be taken to ensure that such problems do not recur.
Hilary and Lennox	2005	Journal of Accounting & Economics	[It] was claimed that reviewers lacked incentives to perform independent reviews. For example, the Public Oversight Board (POB) stated in 2002, "peer review has come under considerable criticism from members of Congress, the media and others. 'You scratch my back, I'll scratch yours' is the prevailing cynical view of peer review raised by many". Along a similar vein, former Chair Williams of the Securities and Exchange Commission (SEC) testified

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			before the Senate Banking Committee (on February 12, 2002) that the peer review process is "too incestuous. A system needs to be established which is independent of the accounting profession."
Glover, Prawitt, and Taylor	2009	Accounting Horizons	Though the profession had instituted and conducted "peer reviews" under the auspices of the AICPA prior to SOX, the peer review system lacked and still lacks independence and the enforcement authority invested in the PCAOB by federal law. [See also: Footnote 18 in Table IA1D]
DeFond	2010	Journal of Accounting & Economics	Finally, studying the shift from the old AICPA Peer Reviews to the new PCAOB Inspections is potentially interesting because it represents a trade-off of expertise for independence. This is interesting because this trade-off is a central feature in long-standing debate between self-regulation and government regulation (e.g., Stigler, 1971; Peltzman, 1976). Traditionally, this trade-off arises because government regulators, while more objective than self-regulators, generally have less industry expertise. In contrast, self-regulators, while more expert than government regulators, are less objective. In the case being studied here, the PCAOB Inspectors are forbidden from being active auditing professionals, and the AICPA Peer Reviewers are practicing auditors.
Doty	2011	Texas Law Review	In twenty-five years of operation, the profession's self- regulatory system never issued an adverse or qualified report on a major accounting firm. In sharp contrast to the profession's quarter century of self-examination, PCAOB inspections have identified hundreds of deficiencies by firms in each of the large accounting firm networks and other firms that audit public company financial statements adequately to support their audit reports.

Table IA1B - Peer review program scope (or focus)

Author(s)	Year	Journal	Quote
Hilary and Lennox	2005	Journal of Accounting & Economics	Under the self-regulated peer review program, auditors were 'audited' (i.e., peer-reviewed) by other auditors. A firm could opt to be reviewed by either: (1) an AICPA-appointed review team; (2) a private CPA association; or (3) an individual audit firm. For the first type of review, the AICPA selected reviewers by matching the specialties of the reviewed firms and the reviewers. In the second case, the firm was reviewed by a private association of CPA firms. In AICPA and association reviews, review team members were drawn from different firms. For the third type of review, all members of the review team came from the same firm and these were known as 'firm-on-firm' reviews. The reviewed firm could choose which firm would perform the review but the AICPA prohibited reciprocal reviews because of concerns about collusion between reviewing and reviewed firms. We find no cases of reciprocal reviews in our sample, which suggests the AICPA's prohibition was adequately enforced. In each type of review, the focus was on the reviewed firm's quality control system. The review team was required to evaluate whether: (1) the firm's system of quality control was adequately designed; (2) the firm complied with its quality control system; and (3) the firm complied with the membership requirements of the SECPS. Reviewers were required to evaluate the following five elements of the quality control system (AICPA, 1996): (1) Independence, (2) Personnel management, (3) Client acceptance and continuation, (4) Engagement performance, and (5) Monitoring… Reviews were conducted at the firm level rather than at the office level. Therefore, one opinion was issued for the entire firm, irrespective of the number of engagements performed by the firm. Reviewers collected evidence on quality control systems by interviewing staff and checking a sample of working papers. Since testing was done on a sample basis, reviewers were not expected to identify all significant weaknesses. After collecting evidence, the review team issued an opinion, which was made publicly available by the AICPA.

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			There were four types of opinion: (1) clean, (2) unmodified with weaknesses, (3) modified, or (4) adverse. Clean opinions were issued if reviewers found no significant weaknesses. Weaknesses were disclosed in unmodified opinions if they were significant but not serious. Opinions were modified if weaknesses were serious or, in very serious cases, opinions were adverse. [References omitted for brevity.]

Table IA1C - PCAOB regime scope (or focus)

Author(s)	Year	Source	Quote
PCAOB	2004	Statement Concerning Inspection Reports	The [SOX] Act requires the Board to "conduct a continuing program of inspections to assess the degree of compliance of each registered public accounting firm and associated persons of that firm with this Act, the rules of the Board, the rules of the Commission, or professional standards, in connection with its performance of audits, issuance of audit reports, and related matters involving issuers." The Act provides that an inspection shall include at least the following three general components: 1. An inspection and review of selected audit and review engagements of the firm, performed at various offices and by various associated persons of the firm; 2. An evaluation of the sufficiency of the quality control system of the firm, and the manner of the documentation and communication of that system by the firm; and 3. Performance of such other testing of the audit, supervisory, and quality control procedures of the firm as are necessary or appropriate in light of the purpose of the inspection and the responsibilities of the Board.
PCAOB	2005	Annual Auditor Full Inspection Report Appendix B (using Deloitte as an example)	A. Review of Selected Audit Engagements The inspection team reviewed aspects of selected audits performed by Deloitte. The inspection team chose the engagements according to the Board's criteria. Deloitte was not allowed an opportunity to limit or influence the engagement selection process or any other aspect of the review. For each audit engagement selected, the inspection team reviewed the issuer's financial statements and certain SEC filings. The inspection team selected certain higher-risk areas for review and, at the practice offices, inspected Deloitte's work papers and interviewed engagement personnel regarding those areas. The areas subject to review included, but were not limited to, revenues, reserves or estimated liabilities, derivatives, income taxes, related party transactions, supervision of work performed by foreign affiliates, assessment of risk by the audit team, and testing and documentation of internal controls by the audit team. The inspection team also analyzed potential adjustments to the issuer's financial statements that had been identified during the audit but not recorded in the financial statements. For several engagements, the inspection team reviewed written communications between Deloitte and the issuer's audit committee. With respect to certain engagements, the inspection team also interviewed the chairperson of the issuer's audit committee. When the inspection team identified a potential issue, the inspection team spoke with members of the engagement team. If the inspection team was unable to resolve the issue through this discussion and any resultant review of additional work papers or other documentation, the inspection team ordinarily requested the engagement team to consult with Deloitte's professional practice personnel, who include local office professional practice directors ("PPDs"), regional professional practice partners ("RPPDs") and members of the National Office professional practice group.

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			B. Review of Seven Functional Areas The inspection team conducted the procedures related to the review of the seven functional areas primarily at Deloitte's National Office. With respect to six of the functional areas, the inspection team also conducted procedures at certain of Deloitte's practice offices. These procedures built on the foundation that was laid during the Board's limited inspection during 2003. The inspection team performed these procedures both to identify possible defects in Deloitte's system of quality controls and to update the Board's knowledge of Deloitte's policies and procedures in the seven functional areas. A more detailed description of the scope with respect to each of the seven functional areas follows. 1. Review of Partner Evaluation, Compensation, Promotion, and Assignment of Responsibilities and Disciplinary Actions… 2. Review of Independence Policies… 3. Review of Client Acceptance and Retention Policies… 4. Review of Internal Inspection Program… 5. Review of Practices for Establishment and Communication of Audit Policies, Procedures and Methodologies, Including Training… 6. Review of Policies Related to Foreign Affiliates… 7. Tone at the Top…
Church and Shefchik	2012	Accounting Horizons	The authors examine Part I findings for Big-Four and "Second-tier" auditors from inspection cycles from 2004 through 2009. They document disclosed inspection outcomes in areas including revenue recognition, fair value measurements, other accounting estimates, and internal controls, among others. Additionally, they find evidence that all sample auditors have remediated quality control criticisms in all years.
Hermanson, Houston, and Rice	2007	Accounting Horizons	The authors document the contents of 316 inspections reports for triennial auditors made available prior to July 2006. On average, these auditors have three issuer clients. The authors note Part I findings for about 60% of these audit firms. The scope of findings indicates a wide range including the auditing of revenue, equity, and investments. In a related paper, Hermanson and Houston (2008) find that triennial auditors also have many quality control criticisms, and a large fraction of them (179 of 199) successfully remediate to avoid Part II disclosures.

Table IA1D - Peer review program penalties

Author(s)	Year	Journal	Quote
Fogarty	1996	Accounting Organizations and Society	Peer review is purposefully non-punitive. The focus on positive improvement and educational direction is said to be jeopardized by structures whose aim was to penalize substandard professional practice. This is justified by a rather unsubstantiated belief that punitive actions are very likely to be brought by external groups (see Larson, 1983), and therefore are unnecessary to be duplicated within the profession. Discipline even as a theoretical possibility, was not part of the initial program. Even after its post facto incorporation, it has not materialized in actual operation (Berton, 1986; AICPA, 1990). By creating a separate body for the imposition of the occasional "corrective action", the main bodies that provide peer review further distance themselves from sanctions.
Public Oversight Board	2002	Self-Published	The current system of self-regulation of the accounting profession has significant problems…[The] disciplinary system is not timely or effective. Disciplinary proceedings are deferred while litigation or regulatory proceedings are in process. This results in years of delay and sanctions that have not been meaningful. The Professional Ethics Division

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			of the AICPA, which handles disciplinary matters against individuals, does not have adequate public representation on its Board. Investigations by the Quality Control Inquiry Committee (QCIC) of the SECPS, which handles allegations of improprieties in litigation against member firms arising out of audits of SEC clients, do not normally include access to firm personnel and work papers. The disciplinary system does not include the power to issue subpoenas or compel testimony. Thus, investigators must rely on the cooperation of the individual being investigated. The QCIC has no access to the complaining party or the client involved. Furthermore, there is no privilege or confidentiality protection for investigations or disciplinary proceedings, and disciplinary actions are often not made public.
Glover, Prawitt, and Taylor	2009	Accounting Horizons	[Footnote 18] Although the AICPA's peer review program lacks true enforcement authority, it should be noted that the process is not entirely without teeth. Follow-up actions are regularly imposed by the "administering entities" (AEs) that can range from requiring additional continuing education in a specific area to requiring the reviewed firm to have its next internal inspection overseen by an independent party that is pre-approved by the AE. More importantly, however, peer review reports are transparent and communicate any problems noted in the reviews through letters of comment appended to unmodified reports as well as through the issuance of modified or adverse report.

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Part 2: Penalties issued by the PCAOB

In this section, we provide a list of the penalties that can be imposed by the PCAOB for auditor deficiencies (Table IA2A) and the frequency (by year) with which these penalties have been issued (Table IA2B). The substantial array of available penalties and their frequent use illustrate a specific mechanism through which the PCAOB can affect auditor behavior; it also provides an example of a meaningful difference between the PCAOB regime and the peer-review program. From 2003 through 2012, the PCAOB issued 131 Rule 5300 sanctions based on 56 unique violation events. The breakdown of these events is presented below by year and penalty type (Table IA2B). Of these penalties, 75 were issued to individuals and 56 to audit firms. The average (median, total) value of the 18 civil monetary penalties is $331,611 ($25,000; $5,969,000).

Table IA2A - PCAOB regime penalties

Author(s)	Year	Source	Penalty
PCAOB	2004, 2007, 2014	Rule 4009 Firm Response to Quality Control Defects	(d) The portions of the Board's inspection report that deal with criticisms of or potential defects in quality control systems that the firm has not addressed to the satisfaction of the Board shall be made public by the Board: (1) upon the expiration of the 12-month period described in paragraph (a) of this rule if the firm fails to make any submission pursuant to paragraph (a); or (2) upon the expiration of the period in which the firm may seek Commission review of any board determination made under paragraph (c) of this rule, if the firm does not seek Commission review of the Board determination; or (3) in the event the firm requests Commission review of the determination, upon completion of the Commission's processes related to that request unless otherwise directed by the Commission.
PCAOB	2004, 2014	Rule 5300 Sanctions	If the Board finds, based on all of the facts and circumstances, that a registered public accounting firm or associated person thereof has engaged in any act or practice, or omitted to act, in violation of the Act, the Rules of the Board, the provisions of the securities laws relating to the preparation and issuance of audit reports and the obligations and liabilities of accountants with respect thereto, including the rules of the Commission issued under the Act, or professional standards, the Board may impose such disciplinary or remedial sanctions as it determines appropriate, subject to the applicable limitations under Section 105(c)(5) of the Act, including: (1) temporary suspension or permanent revocation of registration; (2) temporary or permanent suspension or bar of a person from further association with any registered public accounting firm; (3) temporary or permanent limitation on the activities, functions or operations of such firm or person (other than in connection with required additional professional education or training) Note: Limitations on the activities, functions or operations of a firm may include prohibiting a firm from accepting new audit clients for a period of time, requiring a firm to assign a reviewer or supervisor to an associated person, requiring a firm to terminate one or more audit engagements, and requiring a firm to make functional changes in supervisory personnel organization and/or in engagement team organization. (4) a civil money penalty for each such violation, in an amount not to exceed the maximum amount authorized by Sections 105(c)(4)(D)(i) and 105(c)(4)(D)(ii) of the Act, including penalty inflation adjustments published in the Code of Federal Regulations at 17 C.F.R. § 201 Subpart E; (5) censure;

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(6) require additional professional education or training;
(7) require a registered public accounting firm to engage an independent monitor, subject to the approval of the Board, to observe and report on the firm's compliance with the Act, the Rules of the Board, the provisions of the securities laws relating to the preparation and issuance of audit reports and the obligations and liabilities of accountants with respect thereto, or professional standards;
(8) require a registered public accounting firm to engage counsel or another consultant to design policies to effectuate compliance with the Act, the Rules of the Board, the provisions of the securities laws relating to the preparation and issuance of audit reports and the obligations and liabilities of accountants with respect thereto, or professional standards;
(9) require a registered public accounting firm, or a person associated with such a firm, to adopt or implement policies, or to undertake other actions, to improve audit quality or to effectuate compliance with the Act, the Rules of the Board, the provisions of the securities laws relating to the preparation and issuance of audit reports and the obligations and liabilities of accountants with respect thereto, or professional standards; and
(10) require a registered public accounting firm to obtain an independent review and report on one or more engagements.

Table IA2B - Count of Rule 5300 sanctions by year

		Year										
Rule 5300	**Sanction Description**	**2003**	**2004**	**2005**	**2006**	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**	**Total**
1(a)	Revocation of registration	0	0	3	1	3	2	5	4	5	4	27
1(b)	Temporary suspension of registration	0	0	0	0	0	0	0	0	0	1	1
2(a)	Bar individual	0	0	3	3	8	4	5	7	8	6	44
2(b)	Temporarily suspend individual	0	0	0	0	1	1	2	1	0	1	6
3	Limitation of activities	0	0	0	0	0	1	0	0	1	2	4
4	Civil monetary penalty	0	0	0	0	1	1	1	2	6	8	18
5	Censure	0	0	2	2	3	1	0	1	3	13	25
6	Additional professional education	0	0	0	0	0	0	0	0	1	1	2
7	Engage independent monitor	0	0	0	0	0	0	0	0	1	1	2
8	Engage consultant	0	0	0	0	0	0	0	0	0	0	0
9(a)	Adopt or implement new policies	0	0	0	0	1	0	0	0	1	0	2
9(b)	Remedial measures	0	0	0	0	0	0	0	0	0	0	0
10	Independent engagement review	0	0	0	0	0	0	0	0	0	0	0
	Total Sanctions	0	0	8	6	17	10	13	14	26	37	131
	Total Unique Events	0	0	4	3	9	4	6	9	10	11	56

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Part 3: Analysis of restatements included in PCAOB inspection reports

In this section, we provide descriptive evidence on restatements arising from PCAOB inspections that represent concrete examples of instances where PCAOB inspections led to publicly-observable reporting changes for audited issuers. In inspection reports, the PCAOB notes when an (unnamed) issuer restates financial statements or makes other financial reporting adjustments in connection with inspection findings. These restatements or reporting changes could arise from: (1) direct evidence of material departures from GAAP that are uncovered through PCAOB inspections, (2) auditors performing additional procedures as a result of inspection findings that uncover material departures from GAAP, or (3) issuers finding and correcting material departures from GAAP as a result of inspection findings. The link between inspection findings and subsequent restatements is not necessarily causal in all instances and the PCAOB inspection reports make no claims to this effect. However, the circumstances often suggest a clear link between a particular PCAOB finding, additional auditor procedures, and subsequent firm restatements or changes in reporting. In the first full inspection reports, the PCAOB also notes that "in some instances in which the inspection team identified GAAP departures, follow-up between the [audit] firm and the issuer led to a change in the issuer's accounting or disclosure practices." Table 3A tabulates instances in which restatements and other financial statement changes are noted in the inspection reports for the respective year and auditor. Importantly, the information in Table IA3A is publicly available and investors could use it to update their assessments of the PCAOB regime and their assessments of reporting credibility. The example of restatements related to EITF 95-22 that are identified in the 2003 Limited Inspections are noteworthy because they illustrate how an identified issue can extend beyond an engagement and a single audit firm. As an example, in Table IA3B, we provide further details on the nature of restatements for large (annually-inspected) auditors in the 2004 inspection year.

Table IA3A - Count of noted restatements in inspection report part 1

	Inspection Year									
Auditor	**2003**	**2004**	**2005**	**2006**	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**
Crowe Chizek (Horwath) LLP	-	0	0	1	0	0	1	0	0	0
BDO (USA), LLP	-	1	0	0	0	0	0	0	0	0
Deloitte & Touche LLP	11	4	1	1	2	1	0	0	2	1
Ernst & Young LLP	3	2	1	0	0	2	0	0	1	2
Grant Thornton LLP	-	2	4	1	0	0	0	1	1	1
KPMG LLP	7	1	2	1	0	0	0	0	0	2
McGladrey (Pullen) LLP	-	2	0	0	2	0	0	0	0	0
PricewaterhouseCoopers LLP	3	4	1	0	0	0	0	2	3	1
Annual firm subtotal	24	16**	9	4	4	3	1	3	7	7
Triennial: 75 audit firms with restatements	-	23	12	11	8	13	9	7	9	12
Total	24*	39	21	15	12	16	10	10	16	19
Fiscal years with annual report restatement announcements from Audit Analytics	409	494	749	789	512	366	281	320	337	378
Fiscal years (aligned with year of inspection)	**2002**	**2003**	**2004**	**2005**	**2006**	**2007**	**2008**	**2009**	**2010**	**2011**
Restated annual financial statements	1,003	1,100	970	664	491	395	409	430	467	420

*EITF 95-22 adjustments account for 23 of the 24 restatements in the 2003 (limited) inspection year.
**Details on these restatements are included in Table 3 below.

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Table IA3B - 2004 Inspection year restatements for Big-Four and Tier-2 auditors

Auditor	Issuer & Report Page	Restatement Topic
BDO (USA) LLP	Issuer A, Page 4	EITF 95-22 – Deals with the balance sheet classification (current versus long-term) of borrowings under a revolving credit line that includes a subjective acceleration clause and a lock-box arrangement. In the initial inspection years, failure to identify EITF 95-22 misclassifications was a widespread issue among many auditors and firms.
Deloitte & Touche LLP	Issuer A, Pages 4-5	Understated interest income
Deloitte & Touche LLP	Issuer B, Page 5	Overstated an impairment charge/statement of cash flow misclassifications
Deloitte & Touche LLP	Issuer C, Pages 5-6	EITF 95-22
Deloitte & Touche LLP	Issuer D, Page 6	Recorded leasing incentives from a landlord as a reduction in depreciation expense (as a result, also understated liabilities and fixed assets)
Ernst & Young LLP	Issuer A, Page 4	Misclassified capital lease(s) as operating lease(s)
Ernst & Young LLP	Issuer B, Page 4	Failed to comply with SFAS 131, consolidating several reportable segments into two segments
Grant Thornton LLP	Issuer A, Pages 4-5	Reported securitization transactions as sales that did not qualify under SFAS 140
Grant Thornton LLP	Issuer B, Pages 6-7	Reported various derivative instruments using hedge accounting that did not meet SFAS 133 criteria
KPMG LLP	Issuer A, Page 4	Recorded a non-qualifying transaction as a sale-leaseback transaction, understating fixed assets and debt
McGladrey & Pullen LLP	Issuer A, Page 4	Misclassified gains and losses as discontinued operations
McGladrey & Pullen LLP	Issuer A, Page 4	Recorded a non-qualifying gain on disposal of property to a related party
PricewaterhouseCoopers LLP	Issuer A or Issuer H, Page 5	Recorded revenue in the wrong period
PricewaterhouseCoopers LLP	Issuer S, Pages 6-7	Failed to accrue a DTL related to foreign currency translation on unremitted earnings in a non-US subsidiary
PricewaterhouseCoopers LLP	Issuer V, Page 7	Misclassified investment securities as cash equivalents
PricewaterhouseCoopers LLP	Issuer L, Page 12	Overstated liabilities related to employee medical costs

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Part 4: Details on the remediation process following PCAOB inspections

In this section, we discuss details of the PCAOB's remediation process and provide specific examples of auditors' responses and remedial actions. These details provide further insight into the specific interactions between auditors and the PCAOB and provide anecdotal evidence that these interactions precipitate meaningful changes in audit procedures that extend beyond a single engagement. Conceptually, improvements in audit procedures that last beyond a single engagement are critical because the mere identification of previously unidentified deficiencies would likely lower the market's overall assessment of credibility.

Addressing inspection report Part I findings

Table IA4A provides a description of the process through which auditors address and remediate Part I findings. Upon the identification of a potential issue, the PCAOB begins a dialogue with the auditor. This dialogue can include the issuance of comment forms that help the auditor respond to the proposed issue. If the issue is not satisfactorily clarified after this process, a Part I finding can be issued. Given a finding, the auditor is expected to remediate the issue, both contemporaneously, by performing additional audit work to validate the issued opinion, and prospectively, on future audit engagements. Findings can also result in restatements by the issuer (see Section 3 of this Appendix). As shown below (and in Section 3), auditors' remediation efforts often extend beyond the engagement during which the issue was raised.

Addressing inspection report Part II findings and quality control criticisms

SOX also codified the process by which auditors could remediate firm-wide, Part II findings (e.g., specific criticisms of and defects in their quality control systems identified during the inspections). Satisfactory remediation of Part II findings within a year avoids the public disclosure of these findings. We describe the rules and process in Table IA4B, including details that the PCAOB subsequently provided on its website. Importantly, in a report posted March 2006, the PCAOB stated that during the initial limited inspections of the Big-Four auditors in 2004, all four audit firms had quality control criticisms that were satisfactorily remediated within a 12-month window (and hence not disclosed), which clearly indicates firm-wide changes to Big-Four auditors' quality control systems in response to PCAOB inspections. Church and Shefchik (2012) note that all Big-Four auditors remediated quality control criticisms for inspection years 2004 through 2009.

In their replies to the initial limited and full inspection PCAOB reports, some of the Big-Four auditors provided an attachment that documented their impressions of the PCAOB's inspection process and outlined their planned responses to specific engagement-level findings (Part I) and quality-control criticisms (Part II). We provide examples in Table IA4C.

Any inspection is either resolved through remediation (remaining private) or is not (becoming public). Examples of remedial actions that auditors could and have taken in response to Part II findings include: updating audit templates (e.g., audit planning checklists), requiring reviews by concurring partners, requiring specific timing for the completion of various planning procedures, analytical procedures, substantive testing, reviews, and enhanced training (e.g., changes in the frequency, content, delivery, and levels of staff/partners). We obtained these examples from audit-firm response letters to PCAOB inspection reports (with and without Part II disclosures) and in conversations with PCAOB staff.

When Part II findings are disclosed, the PCAOB does not provide specifics on the attempted remediation. An exception to the confidentiality of Part II remediation efforts is that in the release of its second Part II, Deloitte included an explanatory letter about its efforts to remediate Part II findings

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for inspection years 2009 and 2010, effectively disclosing that findings existed and were remediated to the satisfaction of the Board. Specifically, Deloitte writes:

> **"Resolution of 2009 and 2010—**We are pleased that the PCAOB has determined that the remedial actions we took in response to Part II of our 2009 and 2010 inspection reports addressed the quality control criticisms in those reports to the Board's satisfaction. These determinations close the inspection cycles for 2009 and 2010. We believe the PCAOB's determinations concerning our remediation of the quality control criticisms contained in Part II of our 2009 and 2010 inspection reports are reflective of the significant progress we have made toward the achievement of our audit quality objectives in more recent years."

Importantly, the auditor responses in Table IA4C illustrate that PCAOB inspections prompt changes (retroactive, in the form of restatements, and proactive, in the form of audit policy) beyond the engagements under inspection. For example, the August 2004 PCAOB inspection report for Deloitte & Touche (PCAOB release no. 104-2004-002) notes that the PCAOB's finding of five EITF 95-22 misapplications led the auditor to undertake a firm-wide review that identified three additional engagements with the same misapplication. Aobdia (2018) also finds that auditors exert more effort (in terms of total and partner audit hours) on non-inspected engagements of offices or partners that receive a Part I finding.

Table IA4A - Description of the process to address Part I findings in PCAOB inspection reports

Author	Date	Report	Description
PCAOB	August 26, 2004	Limited Inspection Report (Deloitte as example)	When the staff identified a potential issue, the staff spoke with members of the audit engagement team. If the staff was unable to resolve the issue through this discussion and any resultant review of additional work papers or other documentation, the staff ordinarily requested the engagement team to consult with Deloitte's professional practice personnel, who include local office professional practice directors ("PPDs"), regional professional practice directors ("RPPDs") and members of the national office professional practice group. In many cases, this consultation process resulted in resolution of the matter, either because Deloitte agreed with the position the staff had taken and the firm or the issuer took adequate steps, in light of the significance of the error, to remedy the exception, or because Deloitte was able to provide additional information that effectively addressed the staff's concerns.
PCAOB	Oct 2005 – Jan 2006	Full Inspection Report (Deloitte as example)	When audit deficiencies are identified after the date of the audit report, PCAOB standards require a firm to take appropriate actions to assess the importance of the deficiencies to the firm's present ability to support its previously expressed opinions, and failure to take such actions could be a basis for Board disciplinary sanctions. In response to the inspection team's identification of deficiencies, the Firm, in some cases, performed additional procedures or supplemented its work papers. In some instances in which the inspection team identified GAAP departures, follow-up between the Firm and the issuer led to a change in the issuer's accounting or disclosure practices or led to representations related to prospective changes. In some cases, the deficiencies identified were of such significance that it appeared to the inspection team that the Firm had not, at the time it issued its audit report, obtained sufficient competent evidential matter to support its opinion on the issuer's financial statements. In some of those audits, that conclusion followed from the omission, or insufficient performance, of a single procedure, while other audits included more than one such failure. The deficiencies that reached

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			this degree of significance are described below [as Part I findings], on an audit-by-audit basis (without identifying the issuers). [References omitted for brevity]

Table IA4B - PCAOB remediation process rules and procedures

Author	Date	Forum	Description
107th Congress	July 30, 2002	Legislation	**Section 104(g)(2): REPORT**.—A written report of the findings of the Board for each inspection under this section, subject to subsection (h) shall be… made available in appropriate detail to the public (subject to 105(b)(5)(A), and to the protection of such confidential and proprietary information as the Board may determine to be appropriate, or as may be required by law), except that no portions of the inspection report that deal with criticisms of or potential defects in the quality control systems of the firm under inspection shall be made public if those criticisms or defects are addressed by the firm, to the satisfaction of the Board, not later than 12 months after the date of the inspection report.
PCAOB	Sept – Oct 2003	PCAOB Website	**Rule 4009(a):** With respect to any final inspection report that contains criticisms of, or potential defects in, the quality control systems of the firm under inspection, the firm may submit evidence or otherwise demonstrate to the Director of the Division of Registration and Inspections that it has improved such systems, and remedied such defects no later than 12 months after the issuance of the Board's final inspection report. After reviewing such evidence, the Director shall advise the firm whether he or she will recommend to the Board that the Board determine that the firm has satisfactorily addressed the criticisms or defects in the quality control system of the firm identified in the final inspection report and, if not, why not.
PCAOB	March 21, 2006	PCAOB Website	*Observations on the Initial Implementation of the Process for Addressing Quality Control Criticisms within 12 Months after an Inspection Report* and *Description of the Process for Board Determinations Regarding Firms' Efforts to Address Quality Control Criticisms in Inspection Reports* **PCAOB Release 104-2006-078** In August 2004, the Public Company Accounting Oversight Board ("PCAOB" or "Board") issued its first inspection reports - reports on initial limited inspections of Deloitte & Touche LLP, Ernst & Young LLP, KPMG LLP, and PricewaterhouseCoopers LLP. Pursuant to Section 104(g)(2) of the Sarbanes-Oxley Act of 2002 ("the Act"), the Board did not make public any portions of those reports that dealt with criticisms of a firm's quality control systems. Both the Act and the Board's rules, however, made plain that the Board would publicly disclose such criticisms if the firm failed to address them to the Board's satisfaction within 12 months. Aware of the prospect of such disclosure, each firm engaged in substantial dialogue with the Board's staff during the 12-month period concerning the firm's efforts to address the criticisms, and each firm made a timely submission, pursuant to PCAOB Rule 4009, concerning those efforts ("Rule 4009 submission"). With respect to each of those Rule 4009 submissions, the Board determined that the firm addressed the quality control criticisms to the Board's satisfaction for purposes of Section 104(g)(2) of the Act. As a result, under the Act, "no portions of the inspection report that deal with (the quality control criticisms) shall be made public." **PCAOB Release 104-2006-077** Every Board inspection report that includes a quality control criticism alerts the firm to the opportunity to prevent the criticism from becoming public. The inspection report specifically encourages the firm to initiate a dialogue with the Board's Inspections staff about how the firm intends to address the criticisms. The Board provides the opportunity for dialogue so that a firm acting in good faith can receive timely feedback from the staff and enhance its efforts accordingly before the 12-month deadline.

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	By the 12-month deadline, a firm that seeks to keep the criticism nonpublic may make a submission, pursuant to PCAOB Rule 4009, concerning the ways in which the firm has addressed the criticism (a "Rule 4009 submission")… After a firm makes a timely Rule 4009 submission, the Board must determine whether the firm has addressed the criticisms satisfactorily for purposes of Section 104(g)(2) of the Act. In connection with each Rule 4009 submission, the Board receives a recommendation from the Director of the Division of Registration and Inspections, which, among other things, takes into account any dialogue between the firm and the Inspections staff during the 12-month period. In some cases, the Board may make its determination on the basis of a firm's written submission in circumstances where there was little or no dialogue between the firm and the staff during the 12-month period. In other cases, the Board may make its determination on the basis of the firm's written submission in circumstances where the firm shaped its remediation efforts through substantial dialogue with the staff. In all cases, the process results in a determination favorable or unfavorable to the firm as to each quality control criticism in the inspection report.

Table IA4C - Examples of replies made by (some) Big-Four auditors in response to the initial limited and full PCAOB inspection reports

Auditor	Date	Topic	Quotes
Deloitte & Touche LLP	July 22, 2004	EITF 95-22	The [PCAOB] staff initially identified several situations in which revolving credit debt had been misclassified. Further investigation [by Deloitte & Touche LLP] identified additional similar situations. In all cases, our clients restated their balance sheets to reflect the appropriate classification. We take these misclassifications very seriously, and we have undertaken a process to evaluate precisely what was omitted and how new processes and procedures can preclude a recurrence. We will be modifying our audit procedures and conduct appropriate training once our evaluation is complete.
Deloitte & Touche LLP	July 22, 2004	Documentation	We are actively reviewing and revising our documentation policies and procedures to address the new standards proposed by the PCAOB.
Ernst & Young LLP	July 22, 2004	EITF 95-22	[We] issued an alert to our partners and staff to reemphasize our firm's guidance regarding EITF 95-22, specifying that they particularly review debt agreements during our 2003 audits, and requiring consultation whenever the applicable conditions were present.
KPMG LLP	July 22, 2004	EITF 95-22	[KPMG's Department of Professional Practice] issued enhanced technical guidance and practice aids to assist our professionals in identifying the existence of financial agreements that might meet the criteria of EITF No. 95-22 and analyzing such agreements.
KPMG LLP	July 22, 2004	Firm-wide improvements	First, the Tax Provision Reviewing Partner Network was formed. These audit and tax partners will provide additional training to professionals in the area of tax provisions and have introduced a more extensive tax provision audit program to enhance substantive audit procedures in this area. Second, an audit training and methodology partner role was created. Residing in local offices, this partner specializes in the firm's audit methodology and serves as a resource to local engagement teams as we roll out the implementation of new professional auditing standards and continue to evolve our audit methodology to address today's changing environment. These individuals allow us to bring real time training, developed nationally and delivered locally, to our audit professionals.

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KPMG LLP (continued)			Third, an Audit Quality Council (AQC) has been formed to reassess the firm's audit-related training. The AQC brings together the experiences of a group of partners to discuss issues arising from audit engagements and internal and PCAOB inspection results to recommend areas that training should address. For example, in our national and local office training sessions, we have increased the focus on the importance of including appropriate documentation within the audit work papers. Furthermore, we stress the critical need for the audit engagement partner and manager to analyze the accounting implications that the client's major contracts may have on the company's financial statements.
PricewaterhouseCoopers LLP	July 22, 2004	Part II Findings	Part II of the draft report describes some concerns about potential quality control defects that, under the Act and PCAOB rules, will be made public only if they are not addressed, to the satisfaction of the Board, within twelve months from the date of the final inspection report. In part because this is the first report the Board is issuing, but also because of our intention to be fully responsive, we would like to meet with members of the Board and its staff soon after the final report is issued so that we may ensure that we fully understand the potential quality control issues raised in Part II, as well as to discuss the Board's expectations as to our response. Our goal would be to come to an understanding of the process through which we can satisfy the Board that the actions we are and will be taking are adequate to address the matters contained in the report.
PricewaterhouseCoopers LLP	Oct 22, 2005	Firm-wide improvements	We have taken substantive steps to address the Board's concerns, and we believe the steps we have taken, and are continuing to take, will contribute to improved audit quality and are responsive to these [PCAOB inspection report] findings. We have updated our policies, conducted training, improved technology, increased internal inspections, hired more resources, communicated our leadership expectations related to audit quality, and modified our partner evaluation and compensation process.

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Part 5: Survey of publicly available information about the PCAOB regime

In this section, we provide examples illustrating that capital market participants could learn about the scope and effectiveness of the PCAOB regime from public sources. Overall, the sources in this section (as well as in Table IA4C) support the notion that there was a substantial amount of meaningful public information that investors could use to form and update their assessments of the new PCAOB regime, ensuring changes in auditing as well as reporting credibility. For example, Offermanns and Peek (2011) find evidence of a statistically and economically significant market response to the release of PCAOB inspection reports, which is consistent with investors using the reports in their assessments.

Table IA5A - Authoritative pronouncements

Author	Topic	Date	Forum	Brief Description
107th Congress	Creation of the PCAOB	July 30, 2002	Legislation	Sections 104 (Inspections) and 105 (Enforcement) of SOX gave very specific mandates to the PCAOB in conducting its oversight program. SOX was highly publicized and widely followed during its creation and passage.
PCAOB	Proposed Inspection and Enforcement Rules	July 23 - 28, 2003	PCAOB Website	The PCAOB first made available publicly the proposed rules which would govern inspection and enforcement on registered firms.
PCAOB	Final Inspection and Enforcement Rules	Sept – Oct 2003	PCAOB Website	The PCAOB made available publicly the final rules.
PCAOB	Report on 2003 Limited Inspections of Big Four Accounting Firms	August 26, 2004	PCAOB Website	In conjunction with the limited inspection reports, the PCAOB released a supplemental report that described the nature and scope of the limited inspections. Because the limited inspection reports were highly publicized by the financial press, this report was likely useful in helping readers process the information.
PCAOB	"Appendix B": The Inspection Process	Oct 2005 – Jan 2006	Appendix to Full Inspection Reports	With each full inspection report, the PCAOB included an appendix describing the inspection process. This appendix uses plainer language and describes fully the "quality-control" focus areas. This appendix is likely useful in updating readers on the underlying regime.

Table IA5B - Media coverage

Author	News Outlet	Date	*Title* / Brief Description and/or Quotes
Bryan-Low	The Wall Street Journal	July 23, 2003	*Corporate reform: The first year: Modest digs, tough job for an accounting cop* The PCAOB started the limited inspections on the Big Four firms which focused on culture, compensation and career paths. The head of regulatory affairs at PwC said that the board focused on detailed reviews of specific engagements while also emphasizing more structural issues such as partner compensation, client-retention procedures, and processes for consulting on technical issues; he also said that he was impressed with the inspectors' work.
O'Kelley	Financial Times	July 30, 2004	*Happy second birthday, Sarbanes-Oxley* Gene O'Kelley, chairman and chief executive of KPMG, acknowledged the work of the PCAOB, saying that "(inspection) reports will indicate some issues to be addressed by the accounting profession" and that "we shall take those reports to heart and respond robustly. In the end, I see the PCAOB as there to help us be better auditors."

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Johnson	The Washington Post	August 27, 2004	*Accounting board finds violation: Inspections of Big Four firms' audits reveal poor recordkeeping* "Our findings say more about the benefits of the robust, independent inspection process…than they do about any infirmities in these firms' audit practices," Chairman William J. McDonough said. Accounting scholars and industry experts who read the reports said they were surprised at their thoroughness, especially because board inspectors were operating with a bare-bones staff at the time. "'We are taking all appropriate steps to address all findings and resolve any concerns,' James S. Turley, Ernst & Young's chief executive, wrote in a letter posted on the firm's Web site."
Michaels	Financial Times	August 27, 2004	*Watchdog promises 'unflinching candour': Board found plenty to criticise in first reports, says Adrian Michaels* The US's new accounting regulator, in publishing its first reports on inspections at the Big Four auditors, cheerfully admits it is being harsh. The Public Company Accounting Oversight Board said: "An essential ingredient of the board inspection process is an unflinching candour with firms about the points on which we see a need for improvement." The board said it was not its job to repeat the good work it had found at the firms, acknowledging that the reports therefore "appear to be laden with criticism". KPMG went further than its rivals in explaining what it had done to answer criticisms - even revealing some of the PCAOB's issues in parts of the report that were kept private. The PCAOB said KPMG did not make clear internally that audit quality was the most important factor in evaluation and compensation reviews. That has changed. The PCAOB was also confused - no longer, says KPMG - by the fact that KPMG often sent audit proposals addressed to a company's management, not highlighting "that the audit committee is the 'primary' client".
Norris	The New York Times	August 27, 2004	*Federal regulators find problems at 4 big auditors* The board reviewed the details of 16 audits at each firm in 2003. The public versions of the reports left out large parts of the actual reports because Congress ordered that the firms be given a year to clean up many problems before negative assessments could be made public. William J. McDonough, the board's chairman, tried to soften the blow on the firms by saying the "criticisms do not reflect any broad negative assessment of the firms' audit practices" and emphasizing that "our findings say more about the benefits of the robust, independent inspection process envisioned in the Sarbanes-Oxley Act of 2002 than they do about any infirmities in these firms' audit practices." He added that "none of our findings has shaken our belief that these firms are capable of the highest quality auditing." Nonetheless, the reports document cases where the four firms failed to apply one accounting rule, leading companies to understate the amount of their current liabilities – debts due within one year – and therefore overstate their working capital, an item that analysts often follow.
Weil	The Wall Street Journal	August 27, 2004	*Big Four get mixed marks from U.S. Panel* Yesterday's reports by the accounting board, which was created by Congress in 2002 under the landmark Sarbanes-Oxley securities-overhaul legislation, mark the first time that auditors of publicly held companies have submitted to public evaluations by an independent authority. While the board's initial round of inspections was limited in scope, the fact that the reports contained any criticisms at all marks an improvement over the firms' prior system of "peer review." Under that approach, at a time when the auditing profession still was allowed to regulate itself, the major accounting firms reviewed each other every few years and refrained from criticisms.

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Part 6: Timing of other concurrent regulatory changes

In this section, we discuss the timing of other regulatory changes occurring around the introduction of the PCAOB regime in order to assess whether the timing of these changes makes them potential alternative explanations for our findings. There are three specific regulatory changes that are particularly relevant: 1) foreign countries' adoption of their own public audit oversight regimes; 2) additional SOX provisions not related to the formation of the PCAOB, including: a) Section 302 (management certification of the financial statements), b) Section 404 (rules regarding the preparation and certification of internal control reports), and c) rules regarding auditor independence; and 3) auditing standards.

Table IA6A provides details on the adoption dates of other significant SOX provisions for both US and foreign filers and across large accelerated, accelerated, and non-accelerated filers. Section 302 and auditor independence rules were passed at the same time and apply to both treatment and control groups simultaneously. Section 404 affects our treatment and control groups at different times depending on firm size. We conduct a variety of additional analyses (discussed in Section 4.2 of the Manuscript) to address the possibility that the adoption of SOX 404 is an alternative explanation for our results. Overall, we find little evidence that our results are attributable to the adoption of SOX 404. In Table IA6D, we explicitly gauge the overlap in fiscal-year ends with the onset of SOX Sections 302 and 404 (more details below.)

Table IA6B provides details on the timing of new public audit oversight regulation for the major countries from which we draw our control firms. If the adoption of this regulation coincides with the introduction of the PCAOB, it could bias against finding an effect. As the table shows, there is some overlap in the legislation establishing audit oversight in our sample countries. However, further investigation of the overlap reveals that although many countries passed laws establishing audit oversight, few had already established an actual oversight process and regime. Thus, it is unlikely that any of our sample countries experience practical changes in audit oversight that would materially bias against our findings. Additionally, we find comparable results when we exclude countries with legislation from 2003 to 2005 and redo the combined, unmatched analyses from Table 3 Panels A and B. The Panel A equivalent coefficient of interest is 0.664 with a t-statistic of 2.2, and the Panel B equivalent coefficient of interest is 0.971 with a t-statistic of 3.1.

Table IA6C provides details on the timing of the new PCAOB auditing standards. If the adoption of these standards coincides with the introduction of the PCAOB regime and these standards require auditors to conduct new procedures, our results could reflect the joint effects of new auditing standards and the PCAOB regime. Such effects would still be attributable to the introduction of the PCAOB regime, but there would be another mechanism through which the PCAOB could affect capital-market outcomes. As this mechanism has a qualitatively different interpretation, we provide an overview of changes in auditing standards to gauge the overlap with the rollout of the regime. Initially, the PCAOB made existing Generally Accepted Auditing Standards (GAAS) effective on an interim basis and changed how auditing standards should be referred to in the audit report (AS1). This change should have no effect on our research design. AS2, and its subsequent replacement of AS5, deal with the SOX Section 404 mandate and hence represent more substantive changes to the audit environment. However, as discussed above, we conduct a variety of additional analyses to assess the extent to which the adoption of (audited) internal control reporting affects our inferences. These tests would also capture Section 404-related changes in auditing standards. AS3 relates to firms' required audit documentation. We view this change as intertwined with the new PCAOB regime, and is thus an element of the change we aim to examine and not a confounding event. AS4 represents a relatively minor change to the internal-control-weaknesses reporting requirements and is not adopted until 2006. Accordingly, it is implausible that it has a significant impact on our results.

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Table IA6D presents details on the adoption timing of other SOX provisions relative to the fiscal year in which our sample of triennial firms were first treated by the PCAOB regime. Panel A 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 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 little overlap between other SOX provisions and the treatment dates of the PCAOB regime, suggesting that it is unlikely that the triennial inspection analysis is confounded by the concurrent adoption of other SOX provisions.

Table IA6A - Adoption timing of other SOX provisions by filer status

Provision	US large accelerated filers	US accelerated filers	US non-accelerated Filers	Foreign large accelerated filers	Foreign accelerated filers	Foreign non-accelerated filers
302	FYE on or after August 29, 2002	FYE on or after August 29, 2002	FYE on or after August 29, 2002	FYE on or after August 29, 2002	FYE on or after August 29, 2002	FYE on or after August 29, 2002
404(a)	FYE on or after November 15, 2004	FYE on or after November 15, 2004	FYE on or after December 15, 2007	FYE on or after July 15, 2006	FYE on or after July 15, 2006	FYE on or after December 15, 2007
404(b)	FYE on or after November 15, 2004	FYE on or after November 15, 2004	N/A	FYE on or after July 15, 2006	FYE on or after July 15, 2007	N/A
Auditor Independence	FYE on or after August 29, 2003	FYE on or after August 29, 2003	FYE on or after August 29, 2003	FYE on or after August 29, 2003	FYE on or after August 29, 2003	FYE on or after August 29, 2003

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Table IA6B - International public audit oversight adoption years

Country	Year	Legal source	Country	Year	Legal source
Austria	2006	Quality Control for Audits Act 2005	Italy	2010	Lgs. Decree no. 39/2010
Argentina	2012	Nueva Ley de Mercado de Capitales N 26.831	Japan	2004	CPA Act as Amended 2003
Australia	2001	Corporations Act 2001	South Korea	2008	Revised: The Act on the Establishment of Financial Services Commission
Bermuda	2011	Bermuda Public Accountability Act 2011	Luxembourg	2010	The Law of 18 Dec. 2009
Brazil	1999	CVM Instruction 308/99	Mexico	N/A	N/A
Canada	2003	Canada Corporations Act	Netherlands	2006	Act of 19 January 2006
Chile	N/A	N/A	Norway	1992	Financial Supervision Act
China	N/A	N/A	Singapore	2004	ARCA Act
France	2003	Financial Security Act 2003 - 706	South Africa	2005	Auditing Profession Act
Germany	2004	Auditor Oversight Act WPO 12/2004	Spain	2011	Royal Legislative Decree 1/2011
Greece	2003	Law 3148/2003	Sweden	2002	Auditors Act (2001:883)
India	N/A	N/A	Switzerland	2005	Federal Act on the Licensing and Oversight of Auditors
Ireland	2003	Companies Act of 2003	Taiwan (China)	2007	CPA Act
Israel	N/A	N/A	United Kingdom	2004	Companies Acts 2004 and 2006

Table IA6C - Auditing standards

Auditing Standard	Effective Date	Description
PCAOB Rules 3200T, 3300T, 3400T, 3500T, and 3600T	FYE on or after April 16, 2003	The PCAOB adopted certain preexisting auditing and related standards (i.e., GAAS). Registered public accounting firms and their associated persons must comply with these interim standards to the extent they are not superseded or amended by the Board.
AS1	FYE on or after May 24, 2004	This standard requires that auditors' reports reference the standards of the Public Company Accounting Oversight Board.
AS2/AS5	FYE on or after November 15, 2004 with various amendments and postponements depending on accelerated filer status	This standard provides details on the rules regarding the preparation and certification of internal control reports.
AS3	FYE on or after November 24, 2004	This standard establishes general requirements for the documentation that the auditor should prepare and retain in connection with engagements conducted pursuant to the standards of the PCAOB.
AS4	FYE on or after February 6, 2006	This standard establishes requirements for reporting on whether a previously reported material weakness continues to exist.

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Table IA6D - Details on the timing of the adoption of other SOX Provisions for small auditors

Panel A: 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)	(11)	(12)
Fiscal Year of Treatment	**Fiscal Year of Treatment minus Fiscal Year of First SOX 302 Opinion**									**No 302 Obs (&Other)**	**Total**
	-2	**-1**	**0**	**1**	**2**	**3**	**4**	**5**	**6**		
2005	1	3	1	4	5	15				35	64
2006		2	7	4	2	9	55			51	130
2007			4	1	2	2	9	73		90	181
2008				5	4	1	3	6	41	77	137
Other					1			1		15	17
Total	1	5	12	14	14	27	67	80	41	268	529

SOX Adoption Year	Firm Count
2002	184
2003	29
2004	11
2005	8
2006	15
2007	12
No Obs (&Other)	268
Total	529

Panel A compares the timing of the fiscal year when *Post* first equals one 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 firms by the fiscal year—as indicated in Column (1)—where the treatment indicator, *Post*, switches from zero to one. Each column separates the firms by the timing distance—as indicated in Columns (2) through (10)—of the fiscal year of the firm's first SOX 302 opinion. Each cell counts the number of unique firms where the initial PCAOB inspection overlaps with the firm's first SOX Section 302 opinion. For instance, the highlighted cell in Column (5) shows that four firms had *Post* equals one for the first time in 2006, but were issued their first SOX 302 opinion in 2005. Column (11) enumerates the number of firms where we observe no SOX 302 opinion. We indicate the SOX adoption year by color and include a reconciliation to the total. Note that in Column (4), only 12 of 529 total firms (2.3%) adopt SOX 302 at the same time as the initial PCAOB inspection. When the fiscal year of treatment is later than 2007, *Post* equals zero for the series that includes fiscal years 2001 through 2007, i.e., our triennial tests in the Manuscript.

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Table IA6D - Details on the timing of the adoption of other SOX provisions for small auditors (continued)

Panel B: 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)	(10)	(11)
Fiscal Year of Treatment	**Fiscal Year of Treatment minus Fiscal Year of First SOX 404 Opinion**							**No 404 Obs (&Other)**	**Total**
	-2	**-1**	**0**	**1**	**2**	**3**	**4**		
2005	6	5	6	5				42	64
2006		10	12	17	29			62	130
2007			10	20	20	41		90	181
2008				8	13	26	30	60	137
Other					1	1	1	14	17
Total	6	15	28	50	63	68	31	268	529

SOX Adoption Year	**Firm Count**
2004	105
2005	69
2006	50
2007	34
No Obs (&Other)	271
Total	529

Panel B compares the timing of the fiscal year when *Post* first equals one 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 the fiscal year—as indicated in Column (1)—where the treatment indicator, *Post*, switches from zero to one. Each column separates the firms by the timing distance—as indicated in Columns (2) through (8)—of the fiscal year of the firm's first SOX 404 opinion. Each cell counts the number of unique firms where the initial PCAOB inspection overlaps with the firm's first SOX Section 404 opinion. For instance, the highlighted cell in Column (5) shows that 17 firms had *Post* equals one for the first time in 2006, but were issued their first SOX 302 opinion in 2005. Column (10) enumerates the number of firms where we observe no SOX 404 opinion. We indicate the SOX adoption year by color and include a reconciliation to the total. Note that in Column (4), only 28 of 529 total firms (5.3%) adopt SOX 404 at the same time as the initial PCAOB inspection. When the fiscal year of treatment is later than 2007, *Post* equals zero for the series that includes fiscal years 2001 through 2007, i.e., our triennial tests in the Manuscript.

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Section 2: ERC validation and ERC analysis around 2007 PCAOB enforcement action

In this section, we provide additional evidence to support our use of ERCs to capture audit-related differences in reporting credibility. First, we examine the relative rankings of the ERCs and F-scores of Big-Four clients. Prior research shows that a firm's F-Score is associated with the incidence of accounting fraud, earnings management, SEC enforcement actions, and restatements (Feroz, Park, and Pastena 1991; Dechow et al. 2011; Schrand and Zechman 2012). Thus, firms with higher F-Scores should have lower reporting credibility and lower ERCs. Consistent with this idea, we find that ERCs and F-Scores provide similar rankings of Big-Four clients (in reverse order), which provides more comfort about our measure of the change in reporting credibility used in the main analyses.

Second, we examine ERC changes after the PCAOB's December 2007 enforcement action against Deloitte & Touche, which was the first Big-Four auditor subject to a settled enforcement action (Dee, Lulseged, and Zhang 2010; Boone, Khurana, and Raman 2015).[1] We expect that this enforcement event likely reduces the reporting credibility of Deloitte & Touche clients, at least temporarily, and we expect ERCs to respond accordingly.[2] For this test, we use the U.S. clients of other Big-Four auditors as a control group. Consistent with our prediction, we find that clients of Deloitte & Touche experience a significant decrease in their ERCs following the announcement of the PCAOB's enforcement action. This finding supports our use of ERCs as a measure for reporting credibility. Below, we discuss each of these analyses in further detail.

Auditor rankings based on ERCs and F-Scores:
Table IA7 reports regression results showing the average F-Score (calculated following Dechow et al., 2011) for each Big-Four audit firm relative to PricewaterhouseCoopers (PwC). We use observations from fiscal years 2003 through 2013. These regressions include the firm-characteristic controls from the paper's main analysis. Results in Column (1) indicate that PwC clients have the lowest F-Scores. KPMG clients have slightly higher F-Scores, but the difference is not statistically significant. Deloitte & Touche and Ernst & Young clients both have significantly higher F-Scores than do PwC clients. In Column (2), we include industry-year fixed effects and find that the rank order of the F-Scores is robust to these additional controls. These descriptive results suggest that there is meaningful variation in client F-Scores among the Big-Four auditors (although the differences between the clients of Deloitte & Touche, Ernst & Young, and KPMG are not statistically significant).

Table IA8 presents ERC regressions by Big-Four auditor. We use observations from fiscal years 2003 through 2013. The analysis uses Eq. (1) from the Manuscript with our standard control variables and our control variable interactions with unexpected earnings, but excludes indicators

[1] The PCAOB commences an enforcement action when they find violations similar to those documented in a Part I finding, though generally the breach is determined to be much more severe by the Board (PCAOB 2004d).
[2] This analysis is similar in spirit to studies by Wilson (2008) and Chen, Cheng, and Lo (2014), which document declines in ERCs subsequent to firm-specific restatements.

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for PCAOB inspection (i.e., there are no *Post* or *Treated* variables). In Columns (1), (2), (3), and (4), we separately analyze firm-year observations of the clients of Deloitte & Touche, Ernst & Young, KPMG, and PwC, respectively. We find that PwC clients have the highest ERC estimate, while Deloitte & Touche clients have the lowest. In Column (5), we examine ERCs simultaneously while using PwC clients as the benchmark. The ERC differences between PwC clients and Deloitte & Touche and Ernst & Young clients are statistically significant. ERC differences between PwC and KPMG clients are not statistically different at conventional levels. It is important to recognize that these results do not indicate that being audited by Deloitte & Touche or Ernst & Young *causes* firms to have lower ERCs. Firms are not exogenously assigned to an auditor and hence these differences need to be interpreted cautiously (for instance, they could partially reflect client composition).

For our purposes, the main takeaway is that ERCs and F-Scores produce similar rankings for clients of Big-Four auditors: PwC and KPMG have high ERC and low F-Score clients, while Deloitte & Touche and Ernst & Young have low ERC and high F-Score clients. Although this evidence is descriptive, it nonetheless consistent with the idea that ERCs reflect differences in reporting credibility.

ERCs around the Deloitte and Touche enforcement event:
For our analysis of the Deloitte & Touche enforcement event, we use a specification similar to that in Eq. (1). We include all Big-Four audited issuers whose fiscal year-ends fall between December 2005 and November 2009. *Post* equals one for all issuer-years with fiscal year ends after the December 2007 enforcement action announcement date. We consider a client of Deloitte & Touche to be treated by the credibility shock when its fiscal year ends after the PCAOB enforcement announcement in December 2007.[3] As in our primary analyses, we interact *Loss* and *UE*×*Loss* with the treatment indicators in order to measure the treatment effect for profitable firm years, which is when we expect the reporting credibility effects to be particularly strong.

Table IA9 presents the results of this analysis. In Column (1), the treatment effect *UE*×*Post*×*Deloitte* is -1.177 and statistically significant at the 5% level. In Column (2), we use a matched sample based on the CEM procedure. As in our prior analyses, we match on variables for which we observe significant differences between Deloitte & Touch and other Big-Four clients (i.e., *Loss*, *Market-to-Book*, and *Beta*). The coefficient of interest in this analysis, -1.131, is comparable to Column (1) in magnitude and statistical significance.

Overall, these results indicate that ERCs respond predictably to a negative shock to auditor reputation, providing added support for our use of ERCs as a proxy for reporting credibility.

[3] Using the earnings announcement date instead of the fiscal year-end reclassifies only 38 firm years from *Post* = zero to *Post* = one and does not affect our inferences.

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Table IA7 - F-Scores estimated by audit firm

Dependent Variable: F-Score	(1) Basic	(2) With Industry & Year FEs
Deloitte: Relative to PwC	0.017**	0.021***
	(2.358)	(2.913)
E&Y: Relative to PwC	0.019***	0.018***
	(2.842)	(2.619)
KPMG: Relative to PwC	0.009	0.011
	(1.186)	(1.467)
PwC: Baseline	0.864***	0.837***
	(65.377)	(57.618)
Observations	29,594	29,217
R-squared	0.025	0.115
Firm Characteristics	Yes	Yes
Fixed Effects	No	FF48 Industry × Year

Table IA7 presents descriptive analyses for simultaneously estimated F-Scores for all auditors. We use all available observations from fiscal years 2003 through 2013. Controls include *Loss, Nonlinear*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. For brevity, we do not report coefficients for the control variables. In Column (1), we examine simple differences in means by simultaneously estimating F-Scores with PwC as the baseline; note that the F-Score estimate for KPMG is significantly different from the F-Score estimate for Deloitte and E&Y. The F-Score estimate for E&Y is not significantly different from the F-Score estimate for Deloitte. In Column (2), we again examine simple differences in means by simultaneously estimating F-Scores with PwC as the baseline, but we also include industry-year fixed effects using Fama-French 48 industry definitions. We cluster all t-statistics (in parentheses) at the industry level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

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Table IA8 - Earnings response coefficients estimated by audit firm

Dependent Variable: *CAR*	(1) Deloitte	(2) E&Y	(3) KPMG	(4) PwC	(5) All Firms
UE	0.618***	0.828***	1.159***	1.682***	
	(3.451)	(3.718)	(5.840)	(6.923)	
UE×Deloitte: Relative to PwC					-0.171***
					(-2.642)
UE×E&Y: Relative to PwC					-0.249***
					(-4.210)
UE×KPMG: Relative to PwC					-0.085
					(-1.568)
UE×PwC: Baseline					1.031***
					(9.923)
Observations	4,678	6,928	4,765	5,433	21,800
R-squared	0.059	0.043	0.075	0.077	0.054
Firm Characteristics Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	No	No	No	No
UE×Firm Characteristics	Yes	Yes	Yes	Yes	Yes
UE×Fixed Effects	No	No	No	No	No

Table IA8 presents separate, descriptive analyses for ERC regressions for each auditor and a simultaneously estimated ERC regression for all auditors. We use all available observations from fiscal years 2003 through 2013. Generally, we use a simplified version of Eq. (1): We regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), control variables, and the interactions of *UE* with control variables; however, we exclude indicators for the PCAOB inspection (i.e., *Post* and *Treated*), fixed effects, and the interactions of the treatment indicators/fixed effects with *UE* (as noted in the table footer). For brevity, we do not report coefficients for the control variables or the interactions of control variables with *UE*. Controls include *Loss, Nonlinear*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. In all columns, we estimate a robust regression. In Columns (1), (2), (3), and (4), we examine ERCs for Deloitte, E&Y, KPMG, and PwC, respectively, in separately estimated regressions for observable clients. In Column (5), we examine ERCs simultaneously with PwC as the baseline; note that the ERC estimate for KPMG is significantly different from the estimate for E&Y, but is not different from Deloitte's ERC estimate. The ERC estimate for E&Y is not significantly different than the estimate for Deloitte. We cluster all t-statistics (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.

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Table IA9 - Changes in reporting credibility around the first Deloitte enforcement event in December 2007

Dependent Variable: CAR	(1) Baseline	(2) Matching
UE×*Post*×*Deloitte*	-1.177** (-2.173)	-1.131** (-2.071)
Observations	8,144	7,772
R-squared	0.068	0.070
Firm Characteristics Controls	Yes	Yes
Fixed Effects	Auditor & Year-Quarter	Auditor & Year-Quarter
UE×Firm Characteristics	Yes	Yes
UE×Fixed Effects	Yes	Yes
Treatment Indicators (*Deloitte* and *Post*)	Yes	Yes
UE×Treatment Indicators	Yes	Yes
UE×*Loss* Interacted with Treatment Indicators	Yes	Yes
Matching	No	CEM

Table IA9 presents analyses for the change in ERCs after the Deloitte enforcement action on December 10, 2007. Consistent with our main design analyzing the PCAOB regime, we define a four-year sample window and include all Big-Four audited issuers whose fiscal year ends fall between December 2005 and November 2009. We code *Post* as equal to one for fiscal year ends (and their respective earnings announcements) following December 10, 2007, and zero otherwise. We use a modified Eq. (1) with indicator variables adjusted for the Deloitte enforcement setting and exclude auditor country fixed effects because we limit our sample to firms audited by U.S. auditors. We regress cumulative abnormal returns (*CAR*) on: unexpected earnings (*UE*), indicators for the Deloitte enforcement event (i.e., *Post* and *Deloitte*), 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). We also interact the treatment indicators with the *Loss* and *UE*×*Loss* control variables. 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 and the respective fiscal year end as well as interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Column (1), we examine the changes in ERCs for profitable clients of Deloitte after the Deloitte enforcement event. In Column (2), we estimate the same regression, but we multiply the robust regression weights with weights from a coarsened exact matching procedure using 20 bins for control variables *Loss*, *Market-to-Book*, and *Beta*, variables found to be statistically significantly different between Deloitte and other Big-Four auditors; unmatched bins result in 372 fewer observations. We cluster all t-statistics (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.

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Section 3: Stylized timeline and *Post* variation

In this section, we present and discuss a stylized timeline that maps out the timing of events and its relation to our research design. This timeline illustrates our research design and also helps us determine when to measure changes in reporting credibility. We discuss this timing less formally in the Manuscript. In Figure IA1, we present a stylized timeline for the introduction of the PCAOB regime and the related changes in reporting credibility and ERCs. We begin with the accounting scandals in 2001-2002, which were a shock to reporting credibility that led to a decline in the baseline level of credibility (at t-5). The market's assessment of credibility $C_t[\cdot]$ is not readily observable, but can be measured at earnings announcements using the ERC. The credibility shock is captured by an ERC decline at t-4 relative to the ERC at t-6. At t-3, SOX passes and the PCAOB is established; investors form expectations about the new regime, i.e., the expected treatment E(T), and its ensuing effects on reporting credibility, C[E(T)].

The market response to earnings surprises should not change until after auditors are treated by the new regime and have adjusted their audit procedures accordingly, as indicated by $ERC_{t-2} = ERC_{t-4}$. We assume that the earliest possible date for this change is after the completion of the PCAOB's inspection fieldwork for a particular auditor (at t-1). The ERC at this time may also reflect an updated assessment of the treatment effects, indicated by E'(T). The latest date for an ERC response is the public release of the inspection report (at t+1). As it is not obvious when the market assumes the treatment (and hence when ERCs respond), we use both dates as alternative cutoffs and estimate ERC treatment effects at the first earnings announcements after these alternative dates (EA_t and EA_{t+2}, respectively). Note that the first earnings announcement is not only determined by the respective cutoff date, but also by a firm's fiscal year end, providing additional staggering that we can exploit for identification.

Using fieldwork end as the cutoff, we define an issuer as treated when its fiscal year-end occurs in or after the month that fieldwork ends for its auditor. By that time, the auditor can use information 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 that go beyond the inspected engagements and if investors learn about these improvements (or expect them to have taken place), reporting credibility should increase shortly after the completion of the fieldwork (t-1). 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 (EA_t). Thus, there is generally considerable time between the completion of the fieldwork and our measurement of the ERC effect, giving auditors time to adjust their audit procedures and for the market to become aware of these changes.

If we use the release date of the PCAOB inspection report as an alternative cutoff (t+1), there is an even longer period during which auditors can adjust procedures and investors can learn about these changes. Using the report release as the cutoff date, we define an issuer as treated when it

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first announces its earnings after the PCAOB posts the inspection report for the firm's auditor on its website (EA_{t+2}).

Importantly, the inspection reports do not reveal which specific audits were inspected, but provide investors with general information about audit quality as well as the potential changes in audit procedures arising from the inspections. Thus, the reports allow investors to update their assessments of PCAOB oversight and its effect on reporting credibility, C[E''(T)]. This adjustment could go in either direction (as indicated at t+1). For instance, it is conceivable that inspection reports reveal that the oversight regime is less strict than expected, so credibility goes down (i.e., C[E''(T)]<C[E'(T)]). For this reason, we do not compute incremental changes in the ERC from the end of fieldwork to the report release (i.e., a comparison of EA_t to EA_{t+2}). Instead, we estimate long-run changes in (short-window) ERCs relative to the pre-inspection regime. Specifically, our regime-change analysis tests the hypotheses that the post-fieldwork and the post-inspection-report-release ERCs exceed the pre-treatment ERCs (i.e., $ERC_t \geq ERC_{t-2}$ and $ERC_{t+2} \geq ERC_{t-2}$, respectively).

This timing results in meaningful variation for the *Post* variable. We attach Figure IA2 to show the variation for the limited and full inspections; the design and PCAOB regime rollout provides substantial variation in the timing of the treatment across firms. For triennially inspected auditors, we provide specific examples showing how we code the *Post* indicator for a variety of fiscal year-end dates and inspection years in Figure IA3. Again, these examples highlight the variation in triennial inspections.

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Figure IA1 - Stylized timeline

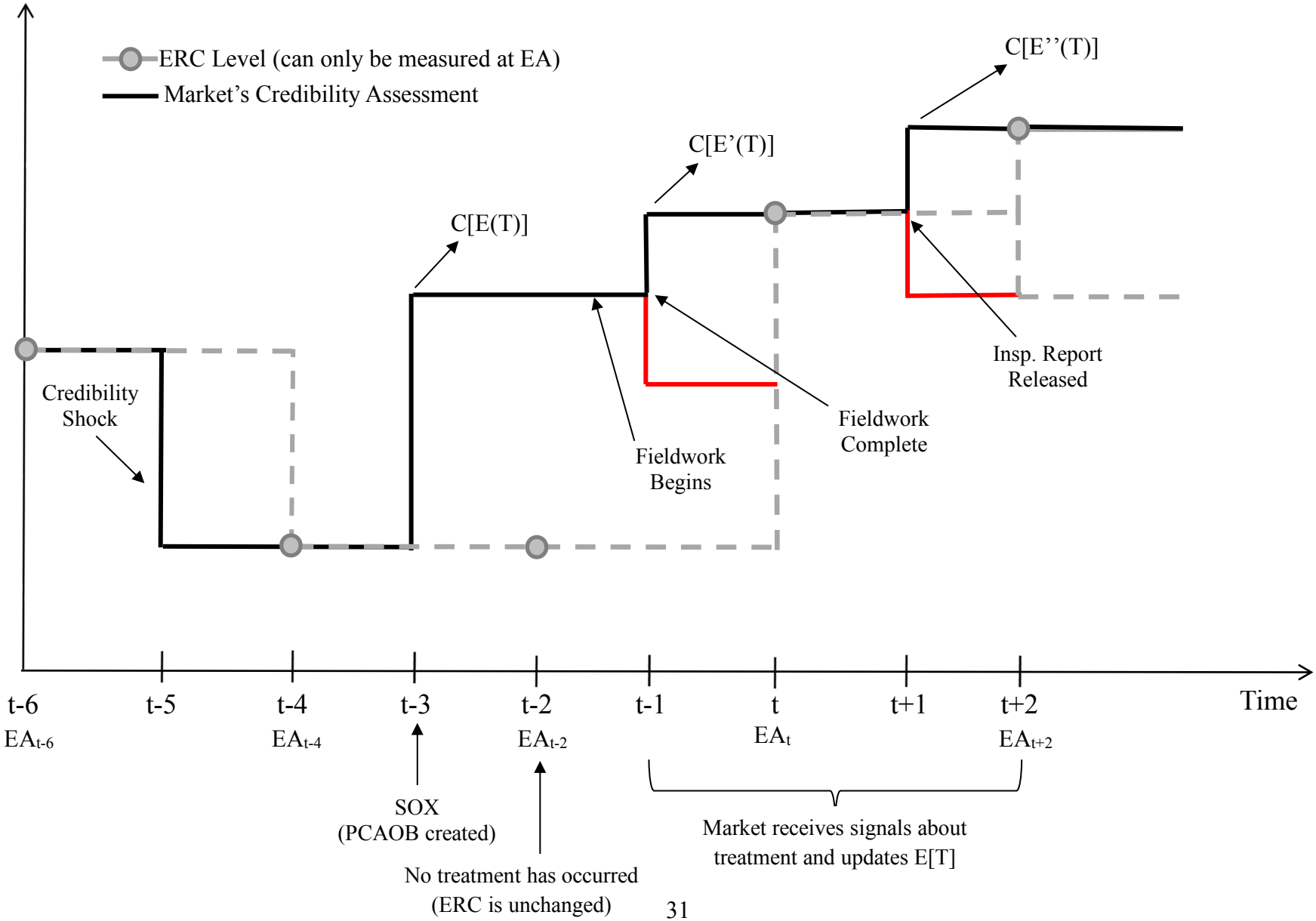

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Figure IA1 - Stylized timeline (continued)

Time	Event(s)	Market Assessment of Credibility (C)		Earnings Response Coefficient (ERC)	
t-6	Pre-treatment EA_{t-6} "normal" times	C_{t-6}	Baseline credibility	$ERC_{t-6} = C_{t-6}$	Baseline ERC
t-5	Shock to credibility (e.g., Enron)	$C_{t-5} < C_{t-6}$	Credibility declines	N/A (measured only at EA)	
t-4	Pre-treatment EA_{t-4}	$C_{t-4} = C_{t-5}$	No change	$ERC_{t-4} = C_{t-5} < ERC_{t-6}$	ERC declines
t-3	SOX & PCAOB established	$C_{t-3}[E(T)] > C_{t-4}$	Under the hypothesis that the regime has an impact, credibility increases based on E(T)	N/A (measured only at EA)	
t-2	Pre-treatment EA_{t-2}	$C_{t-2} = C_{t-3}$	No new info about E(T)	$ERC_{t-2} = ERC_{t-4}$	No change, as auditor has not yet been treated
t-1	Fieldwork Completed	$C_{t-1}[E'(T)] \lesseqgtr C_{t-2}$*	Market may have received new info about regime and updates to E'(T)	N/A (measured only at EA)	
First (potential) cutoff date					
t	Post-treatment EA_t	$C_t = C_{t-1}$	No change	$ERC_t = C_t$	ERC based on updated credibility assessment; Test $ERC_t \geq ERC_{t-2}$
t+1	Inspection Report Release	$C_{t+1}[E''(T)] \lesseqgtr C_t$*	Market receives new info about regime and updates to E''(T)	N/A (measured only at EA)	
Latest (potential) cutoff date					
t+2	Post-treatment EA_{t+2}	$C_{t+1} = C_{t+2}$	No change	$ERC_{t+2} = C_{t+2}$	ERC based on updated credibility assessment; Test $ERC_{t+2} \geq ERC_{t-2}$

Figure IA1 provides a stylized timeline of the change in the market's assessment of credibility and earnings response coefficient around the introduction of the PCAOB regime. The predictions are formed under the hypothesis that the PCAOB regime increases reporting credibility. We denote the market's expectation of the treatment with E(T), reflecting that the treatment is not directly observable. ERCs are assumed to be a function of the market's credibility assessment $C_t[\bullet]$ given the prevailing expectations about treatment.

* Note that under the hypothesis that the regime has an impact, $C_{t-1}[E'(T)] > C_{t-4}$ and $C_{t+1}[E''(T)] > C_{t-4}$. The regime-change analysis benchmarks against the pre-treatment ERCs, i.e., $ERC_t \geq ERC_{t-2} = ERC_{t-4}$ and $ERC_{t+2} \geq ERC_{t-2} = ERC_{t-4}$, respectively.

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Figure IA2 - Graphing the *Post* variable for annually inspected auditors' fieldwork and inspection report release dates

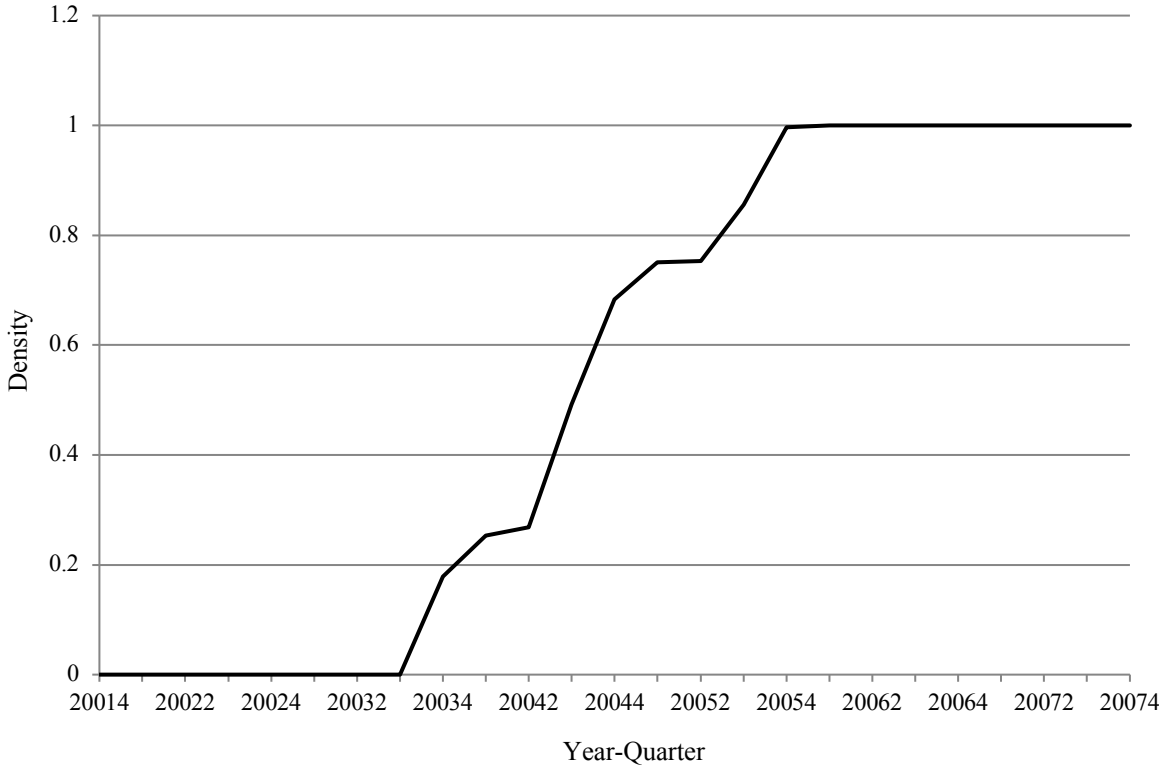

Figure IA2 maps out the *Post* variable for annually inspected auditors' fieldwork and inspection report release dates, i.e. using the "combined" sample as described in Section 4.1. The y-axis is defined as the percentage of firms for which *Post*=1 (i.e., the density). The x-axis is defined as the calendar year-quarter.

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Figure IA3 - 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

Figure IA3 provides examples illustrating how we code *Post* for analyses using the end of the inspection fieldwork as the cutoff date. For triennially-inspected auditors, *Post* is an indicator variable equal to one for any fiscal year-end that is 30 days (or more) after the conclusion of the PCAOB's inspection fieldwork of the firm's auditor, and zero otherwise. As illustrated by the examples above, the inspection dates (and thus 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 time interval highlights the fact 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 procedures. Although the issuers in the table are clients of the inspected auditor, the table does not imply that a specific engagement with an issuer was or was not inspected (this information is not publicly available). The purpose of the analysis is to examine whether the treatment of an auditor with the PCAOB regime increases the reporting credibility of the issuers, irrespective of inspections of specific engagements (and their outcomes).

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Section 4: Historic parallel trends in ERCs

In this section, we examine historic parallel trends in ERCs. A standard concern in difference-in-differences analyses is the possibility that treatment and control firms differ systematically and would not have similar trends in the absence of treatment, i.e., that ERCs would not have evolved similarly in the absence of the PCAOB regime, violating the parallel-trends assumption. Our analysis is conducted in event time using a relatively short pre-period. Thus, it is difficult to assess pre-period trends. To address this issue, we provide evidence on the historical trends of the ERCs for both treatment and control firms over a longer period.

Figure IA4 graphs historical trends in ERCs for treatment and control firms in calendar 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 IA11 Column (1), 1.377. Throughout the entire period, the ERCs of the U.S. and non-U.S. firms change in similar ways, suggesting that the parallel-trends assumption is likely to be valid for our sample.

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Figure IA4 - Historic parallel trends in ERCs

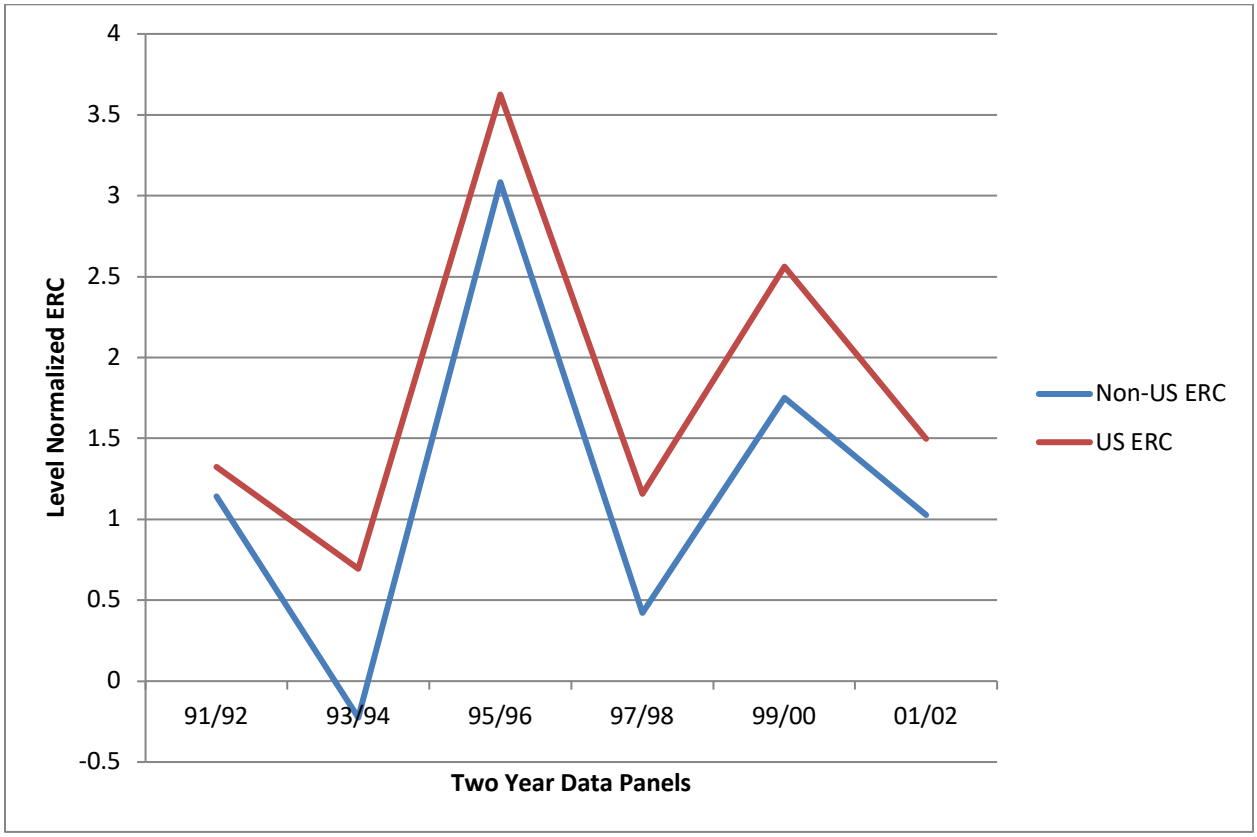

Figure IA4 presents historical trends in ERCs for U.S. firms audited by Big-Four and Tier-Two U.S. auditors and for non-U.S. firms audited by the Big Four and by Grant Thornton from 1991-2002. We estimate ERCs in rolling two-year intervals in calendar time based on Eq. (1). The sample includes the firms 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. 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 comparison with the 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 and magnitudes of the estimates for non-U.S. and U.S. firms.

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Section 5: Breakdown of the treatment and control samples by the location of the auditor

In this section, we provide descriptive information on auditor location. Table IA10 provides a breakdown of the treatment and control samples by the location of the auditor who 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. Most control firms are audited by Canadian and British auditors.

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Table IA10 - Sample composition: Breakdown of the treatment and control samples by auditor location

Auditor Country	All Events	
	Firms (1)	Firm-Years (2)
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**

Table IA10 provides details on the sample composition for our limited and full inspection analyses. The counts in this table comprise firms with available data for the four cutoff dates (limited inspection fieldwork end, limited inspection report release, full inspection fieldwork end, and full inspection report release) that enter the combined analysis, for which we stack firm-year observations for the four sample windows defined for each of the alternative cutoff dates. We provide the dates (month and year) of these four events for annually inspected auditors in Appendix A, Table A1. Due to the stacking, the combined analysis includes the same firm up to four times. For each firm, the sample includes fiscal year-ends that fall into the sample windows defined for the respective cutoff dates (see Appendix A, Figures A1 and A2, for details). We require that firms have available data on Audit Analytics, Compustat, CRSP, and I/B/E/S. Here, we provide a breakdown of the treatment and control samples by auditor location. Column (1) reports the number of unique sample firms by country. Column (2) reports the number of firm-years by country for the combined analysis, which comprises all inspections and sample windows.

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Section 6: Dealing with extreme values of *UE*

It is well known that ERCs are noisy and that extreme values of *UE* can play an important role in the analysis (e.g., Kothari, 2001). For this reason, this section provides more information on the empirical distribution of *UE* in our setting as well as several additional analyses that assess the sensitivity of our results to the treatment of extreme observations. We present scatter plots for untrimmed and truncated data across a variety of truncation levels. We also estimate our main specification using 1) a standard OLS regression, where UE is truncated at 2.5% and 97.5% and 2) an OLS regression using the percentile rank of UE, where UE is only truncated at 1% and 99%.

Figure IA5 provides a scatter plot of Cumulative Abnormal Returns (*CAR*) on Unexpected Earnings (*UE*) for untrimmed *UE* and *UE* trimmed at 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% at the top and the bottom.

Table IA11 presents additional results for our primary analysis (i.e., using a stacked sample that combines limited and full inspections and fieldwork-end and inspection-report-release dates). To facilitate a comparison with prior ERC studies, Column (1) provides ordinary least squares (OLS) regression results for a baseline ERC model, which includes all control variables, but excludes the treatment (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). Column (2) provides the same ERC model with a robust regression estimation, which is the primary estimation method from the Manuscript. Columns (3)-(6) present the primary analysis using alternative methods to deal with extreme values of *UE*. Column (3) provides estimates from a standard OLS regression, where *UE* is truncated at 2.5% and 97.5%. Column (4) repeats the analysis in Column (3) but includes the fixed effect structure from the Manuscript. Column (5) presents results based on an OLS regression using the percentile rank of *UE* (where we truncate *UE* only at 1% and 99%). Column (6) repeats the analysis in Column (5) but now includes the fixed effect structure from the Manuscript. For brevity, we suppress the coefficients on the main effects and (non-interacted) control variables in all columns.

The results in Table IA11 indicate that our main finding depends on the treatment of extreme observations. Using only a mild truncation and OLS, the coefficient of interest is insignificant, though quite close to conventional levels (and a coefficient of 0.582 vs. 0.788 in Table 4). We note that robust regressions are not the only way to account for extreme *UE*. Using percentiles and mild truncation, the coefficient of interest is significant. In Section 8 of this Internet Appendix, we discuss that extreme values can lead to non-linearities in the ERC. Accounting for non-linearities often yields stronger results and inferences, even using OLS and mild truncation.

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Figure IA5 - Scatterplot of cumulative abnormal return on unexpected earnings

Panel A: Scatterplot of cumulative abnormal return on untrimmed unexpected earnings

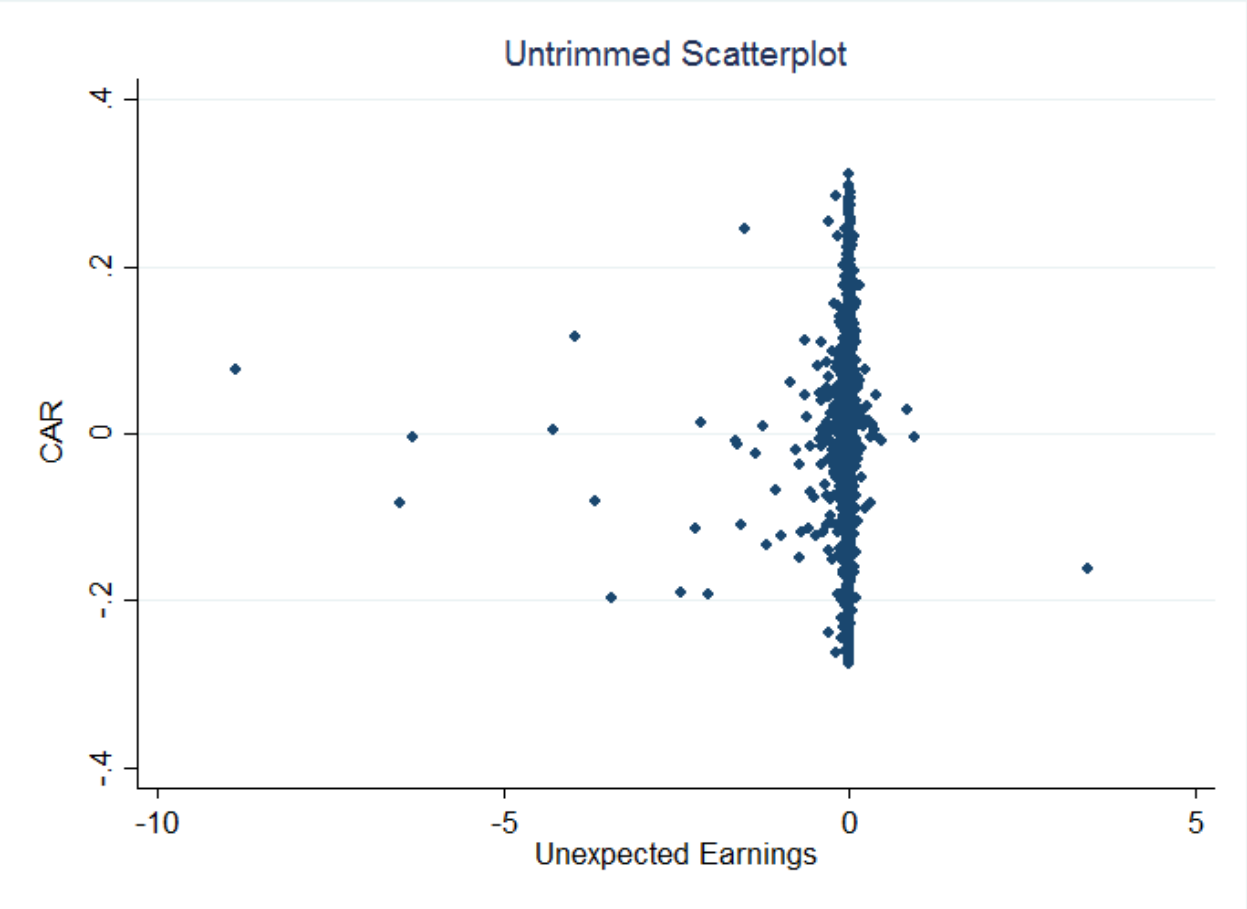

Figure IA5 includes scatterplots using various trimming thresholds for *Unexpected Earnings* (*UE*) along the x-axis and for *Cumulative Abnormal Returns* (*CAR*) along the y-axis. Panel A presents a scatterplot of *CAR* on untrimmed *UE*. The sample comprises 42,544 observations from limited and full inspections for annually inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample).

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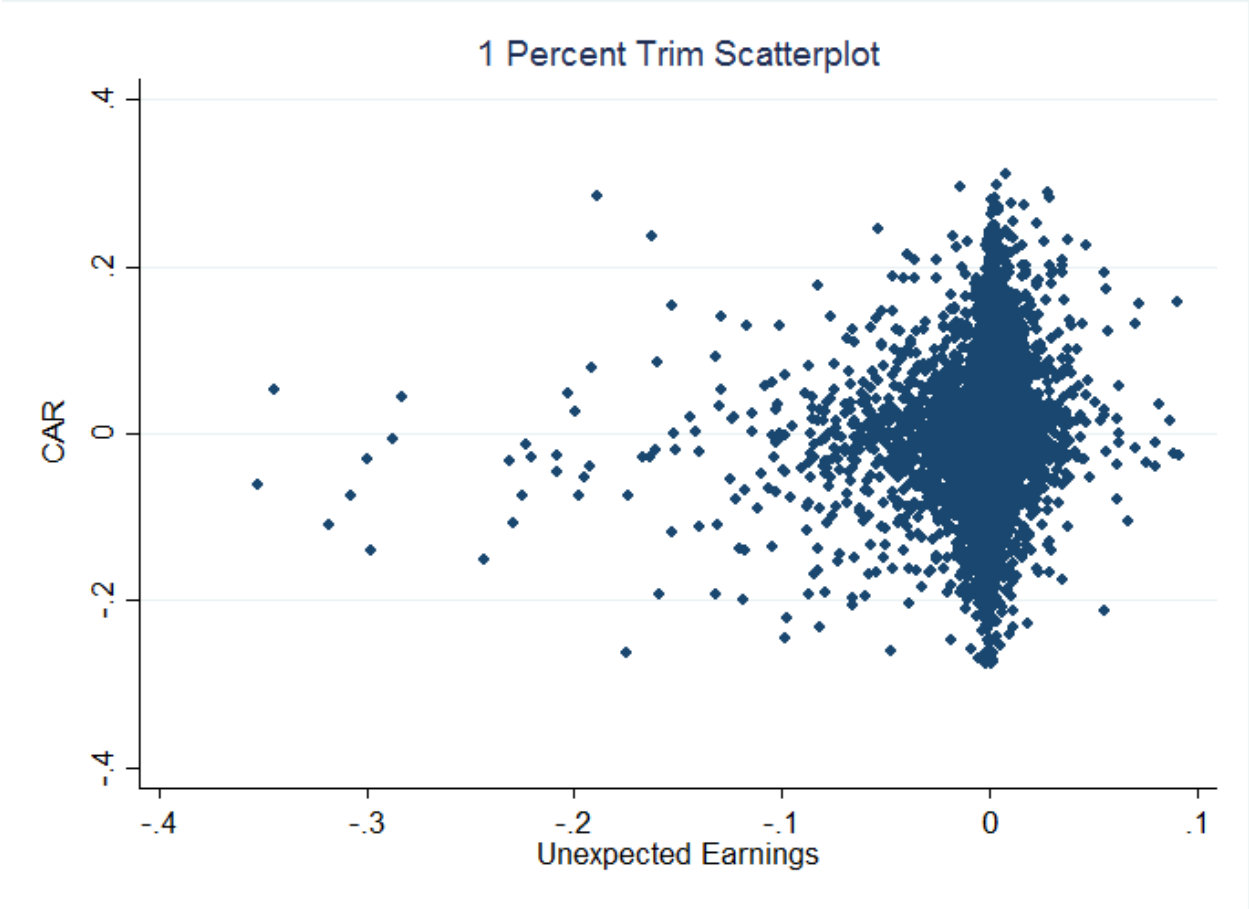
Figure IA5 - Scatterplot of cumulative abnormal return on unexpected earnings (continued)

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 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).

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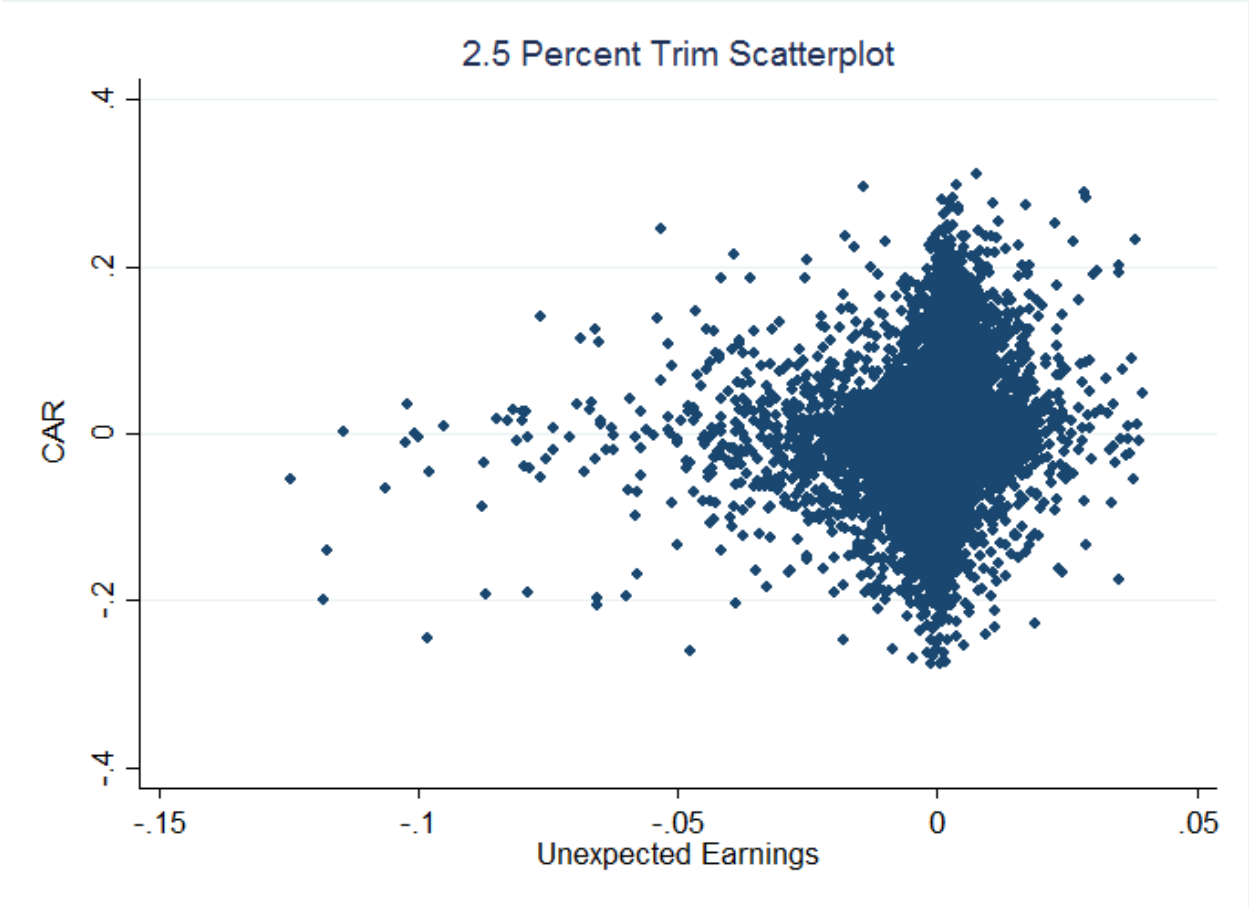
Figure IA5 - Scatterplot of cumulative abnormal return on unexpected earnings (continued)

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 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).

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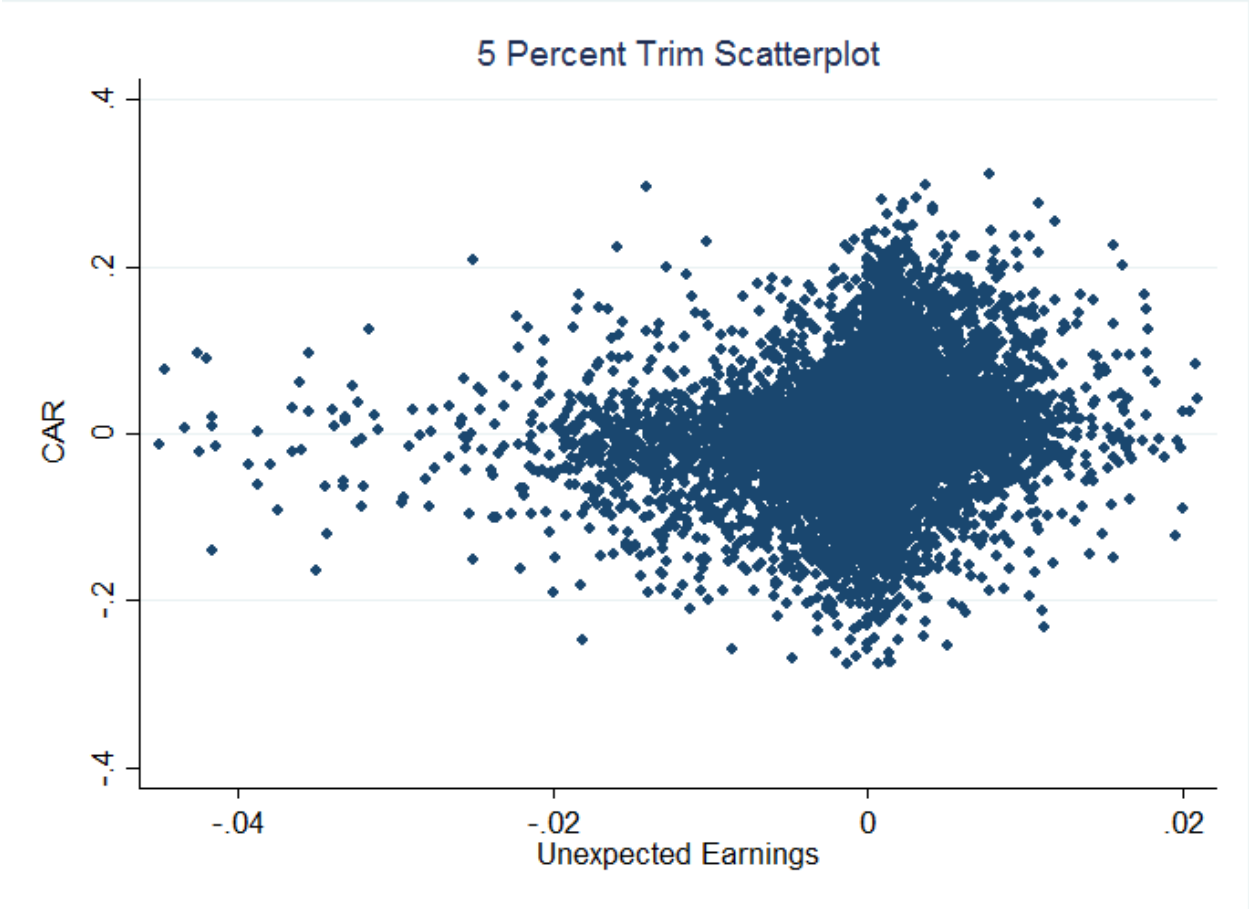
Figure IA5 - Scatterplot of cumulative abnormal return on unexpected earnings (continued)

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 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).

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Table IA11 - Baseline analyses and treatment effects using alternative regression methods

Dependent Variable: *CAR*	(1) OLS	(2) Robust	(3) OLS	(4) OLS	(5) Perc. OLS	(6) Perc. OLS
UE×Post×Treated			**0.530***	**0.582**	**0.024*****	**0.018****
			(1.739)	**(1.642)**	**(2.883)**	**(2.030)**
UE	1.377***	0.994***				
	(3.780)	(4.410)				
UE×Loss	-1.043***	-0.793***	-1.015***	-1.046***	-0.026***	-0.026***
	(-5.540)	(-6.974)	(-5.302)	(-5.379)	(-5.074)	(-5.023)
UE×Size	-0.064	-0.070**	-0.048	-0.029	-0.002*	-0.001
	(-1.291)	(-2.204)	(-0.901)	(-0.506)	(-1.753)	(-1.025)
UE×M2B	0.078**	0.075***	0.074**	0.051	0.002*	0.002*
	(2.350)	(3.356)	(2.206)	(1.493)	(1.851)	(1.796)
UE×Leverage	-0.040***	-0.024**	-0.040***	-0.037**	-0.001***	-0.001**
	(-2.818)	(-2.339)	(-2.802)	(-2.514)	(-3.174)	(-2.566)
UE×Persistence	0.032	0.107	0.021	0.009	0.001	0.001
	(0.233)	(1.175)	(0.152)	(0.065)	(0.208)	(0.188)
UE×Beta	0.438***	0.578***	0.394**	0.312*	0.017***	0.016***
	(2.966)	(6.305)	(2.521)	(1.931)	(4.337)	(4.042)
Firm Characteristics (Controls)	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	No	No	Auditor & Country & Year-Quarter	No	Auditor & Country & Year-Quarter
Treatment Indicators (*Post, Treated*)	No	No	Yes	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
UE×Fixed Effects	No	No	No	Yes	No	Yes
UE×Treatment Indicators (*Post, Treated*)	No	No	Yes	Yes	Yes	Yes
Observations	40,766	40,766	40,766	40,766	41,882	41,882
R-squared	0.022	0.033	0.022	0.034	0.055	0.65

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Table IA11 reports an analysis of the change in reporting credibility around the introduction of annual PCAOB inspections using standard estimation techniques. We report 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 inspections (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 (3)-(6), we do not report the coefficients for fixed effects, *UE* interactions with fixed effects, or variables pertaining only to the benchmark category (e.g., omitted fixed effects). Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. Where indicated, we include fixed effects for the auditor (defined at the global network level), the auditor's home country, and the year-quarter of the respective fiscal year end, as well as interactions of these fixed effects with *UE*. In Column (1), we use OLS and exclude the PCAOB regime treatment indicators in order to provide a benchmark ERC regression. In Column (2), we estimate a robust regression similar to that in Column (1), excluding the PCAOB regime treatment indicators in order to provide an alternative benchmark ERC regression using the primary regression strategy from the Manuscript. In Columns (3) and (4), we introduce the regime change indicators and use an OLS estimation; Column (4) includes the fixed effects from the Manuscript. In Columns (5) and (6), we use OLS and the percentile rank (on a scale of 0 to 1) of *UE*; Column (6) includes the fixed effects from the Manuscript. In all these columns, the sample size is larger because we only truncate *UE* at the 1% and 99% levels (like all other variables) before applying percentile rank. We cluster all t-statistics (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.

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Section 7: Tabulation of additional coefficient estimates for control variables

In this section, we present a full tabulation of all coefficient estimates while excluding fixed effects and fixed effects interacted with *UE*. Table IA12 reports this additional output from models in Table 4 of the Manuscript. Importantly, we caution that many of the main effects are difficult to interpret due to the extensive fixed effects structure. Specifically, we do not report the main effect of *UE* in the regressions because we interact *UE* with auditor, country, and year-quarter fixed effects. Thus, the coefficient on *UE* (alone) captures the association for the omitted fixed effect category (e.g., a single control country, like Argentina); this is not particularly meaningful and the coefficient on *UE* is not, by itself, sufficient to infer the total effect.

Similar interpretation issues arise for other variables and their interactions (e.g., *Treated* and *Post*) due to our country and year-quarter fixed effects; we also do not report these coefficients. For example, the coefficient on *UE* × *Treated* measures the difference in ERC for U.S. firms relative to the ERC of Argentinian cross-listed firms (i.e., the omitted country fixed effect) in the pre-treatment period. The unreported ERC estimates for the other countries (i.e., not Argentina) are incremental estimates relative to the omitted country and are not particularly meaningful by themselves.

Similarly, *UE*×*Post* measures the change in the control-group ERC for the periods after the PCAOB inspections that are not collinear with the year-quarter interacted effects. Thus, while the combined sample has some staggering, there is considerable overlap between the Post indicator and the year-quarter dummies (i.e., 64.5% in the combined sample and between 83.4% [limited fieldwork] and 99.0% [limited report release] for the individual cutoff dates). Due to this collinearity between the main and fixed effects, the point estimate on *UE*×*Post* is not particularly meaningful.

Finally, one might wonder whether *UE*×*Post*×*Treated* provides the average change in the ERC for U.S. firms relative to Argentinian firms in the post period. It does not because we have not interacted *Post* with all of the country effects or interacted *Post* with the country effects × *UE*. Had we done so, each country could be separately "tested" against Argentina for post-PCAOB changes in ERCs. The same applies for the year-quarter effects and interactions: The variable of interest does not measure the effects for the "non-collinear" periods in the post-PCAOB period because the year-quarter effects and interactions with *UE* have not been interacted with *Treated*. As a result of this coding, the variable of interest (*UE*×*Post*×*Treated*) measures the average, incremental change in ERCs for U.S. audited firms after the PCAOB inspections (relative to the control group).

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Table IA12 - Full model estimates

Dependent Variable: *CAR*	(1) Combined, Unmatched	(2) Combined, CEM
UE×Post×Treated	**0.788*** **	**0.719** **
	(3.473)	**(2.230)**
Loss	-0.009***	-0.008***
	(-7.220)	(-6.524)
Size	0.001***	0.001***
	(2.710)	(2.889)
Market-to-Book	-0.000*	-0.000
	(-1.730)	(-1.210)
Leverage	0.000**	0.000**
	(1.997)	(2.272)
Persistence	-0.001	-0.001
	(-1.034)	(-0.842)
Beta	-0.002***	-0.002*
	(-2.717)	(-1.837)
UE×Loss	-0.761***	-0.755***
	(-6.226)	(-5.787)
UE×Size	-0.023	-0.033
	(-0.610)	(-0.777)
UE×Market-to-Book	0.054**	0.061**
	(2.384)	(2.557)
UE×Leverage	-0.024**	-0.032***
	(-2.294)	(-3.028)
UE×Persistence	-0.034	-0.090
	(-0.405)	(-1.028)
UE×Beta	0.301***	0.378***
	(2.955)	(3.243)
UE	0.480	-0.336
	(0.755)	(0.684)
Post	0.005***	0.007**
	(2.772)	(2.427)
UE×Post	-0.914***	-0.818***
	(-4.220)	(-2.676)
Treated	-0.004	-0.000
	(-0.618)	(-0.068)
UE×Treated	0.722	1.693***
	(1.219)	(4.232)
Post×Treated	-0.005**	-0.007**
	(-2.575)	(-2.260)
Fixed Effects	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter
UE×Fixed Effects	Yes	Yes
Observations	40,766	39,843
R-squared	0.055	0.062

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Table IA12 reports the full model estimates for our analysis using the "primary design" as described in Appendix A, Figures A1 and A2. Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on: unexpected earnings (*UE*), indicators for PCAOB inspections (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 fixed effects or the interactions of the fixed effects with *UE*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (defined at the global network level), the auditor's home country, and the respective fiscal year end, as well as interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Column (1), we examine the combined analysis, using the cutoff dates from (i) fieldwork completion for limited inspections, (ii) inspection report releases for limited inspections, (iii) fieldwork completion for full inspections, and (iv) inspection report releases for full inspections. Column (2) is the same as Column (1), but we combine the robust regression weights with weights from a coarsened exact matching procedure using 20 bins for the control variables *Size* and *Beta*; unmatched bins result in 923 fewer observations. All t-statistics (in parentheses) are based on standard errors clustered 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.

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Section 8: Considering and plotting nonlinearities

In this section, we consider nonlinearities in the ERC relation that are driven by limited market responses to extreme values of *UE*. Freeman and Tse (1992) demonstrate that because extreme realizations of unexpected earnings are likely less persistent, the relation between unexpected earnings and announcement returns can be nonlinear (i.e., return responses decrease as the absolute magnitude of unexpected earnings increases). For that same reason, any credibility effect for extreme values of *UE* is likely small. Moreover, changes in the fraction of extreme and non-extreme values of *UE* across time, i.e., from the pre- to post-period, could bias the estimated treatment effect. In addition to macroeconomic shocks and the business cycle, such changes could also arise if auditors force firms to recognize impairments under the post-inspection regime. While this 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. We present results that confirm the existence of nonlinearities; one result plots our estimated ERC function while including and excluding the nonlinear term in a simple scatterplot of *CAR* and *UE* and in a fractional polynomial regression. Using the fractional polynomial regression, we show that the s-shaped ERC relation shifts upwards, which is consistent with a credibility effect. We also confirm that our results do not change if we allow for changes in the frequency of extreme values of *UE* through time.

Figure IA6 provides intuition for the importance of nonlinearities in the earnings-return relation by plotting our estimated ERC function including and excluding the nonlinear term in a simple scatterplot of *CAR* and *UE*, limiting the graph to *UE* that falls within 1% of a firm's stock price. The function that includes the nonlinear term 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 with the nonlinear term better fits the data (p-value<0.01).

Figure IA7 further explores the nonlinear relation between unexpected earnings and returns by estimating a fractional polynomial regression in which we determine the best nonlinear function by using fractional powers from -2 to +3.[4] The regression results indicate that a fractional polynomial with cubic terms best fits the relation between *CAR* and *UE*, supporting our *Nonlinear* specification that essentially preserves the sign of the variable in a way that is similar to using a cubic term. Figure IA5 Panel A provides a graph of this function before and after the introduction of the PCAOB regime using all treatment firms. In Figure IA5 Panel B, 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 (or tilt) in the absolute return response to unexpected earnings. This upward shift represents the increased

[4] 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 a more diverse set of functional forms. We only consider non-linear transformations of unexpected earnings, but include our full set of control variables in the estimation.

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reporting credibility of earnings surprises after the introduction of the PCAOB regime. Loss firms are expected to have fundamentally different ERCs because of the lower persistence of losses. Thus, loss firms should not exhibit the same treatment effect as do profit firms; Figure IA5 Panel B illustrates that loss firms do indeed have a fundamentally different ERC shape. In Figure IA5 Panel C, we plot fractional polynomial regressions of cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*) using Eq. (1), but include only the non-profitable (loss) firms from the treatment group in the pre-inspection (the dashed line) and 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 IA8 is similar to Figure IA7 Panel B, but uses triennially inspected auditors. We plot fractional polynomial regressions of cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*) using Eq. (1), but include only the profitable, triennially-inspected firms in the pre-inspection (the dashed line) and post-inspection (the solid line) periods. This figure illustrates a significant upward shift (or tilt) in the response to positive *UE*, as in the plot for annually-inspected firms in Figure IA5 Panel B. The plot shows no increase in the response to negative values of *UE*. Note, however, that the graphs are not benchmarked against control firms as in our regression analysis.

Table IA13 extends our main regression analysis accounting for extreme values of *UE* by allowing for a more general, non-linear treatment response. In these tests, the effect is benchmarked against the control firms. Following prior research (e.g., Chen, Cheng, and Lo 2014), we model the nonlinear relation while including the interaction between *UE* and the absolute value of *UE* as well as losses interacted with *UE* in Column (1). Consistent with prior research, we find that these interactions are significantly negative. Moreover, the baseline ERC increases considerably, indicating that the low baseline ERC is (in part) attributable to the failure to account for naturally less persistent surprises. As additional controls, we consider the effect of nonlinearity (Columns (2)-(3)) and nonlinearities [Columns (4)-(5)] on the estimated change in ERCs. Across all columns, the estimated treatment effect is stronger, indicating that ERCs are nonlinear and that a large post-treatment response does not necessarily confer credibility linearly across the range of *UE* but is likely concentrated in small surprises.

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Figure IA6 - Scatterplot and fitted values including and excluding *Nonlinear*

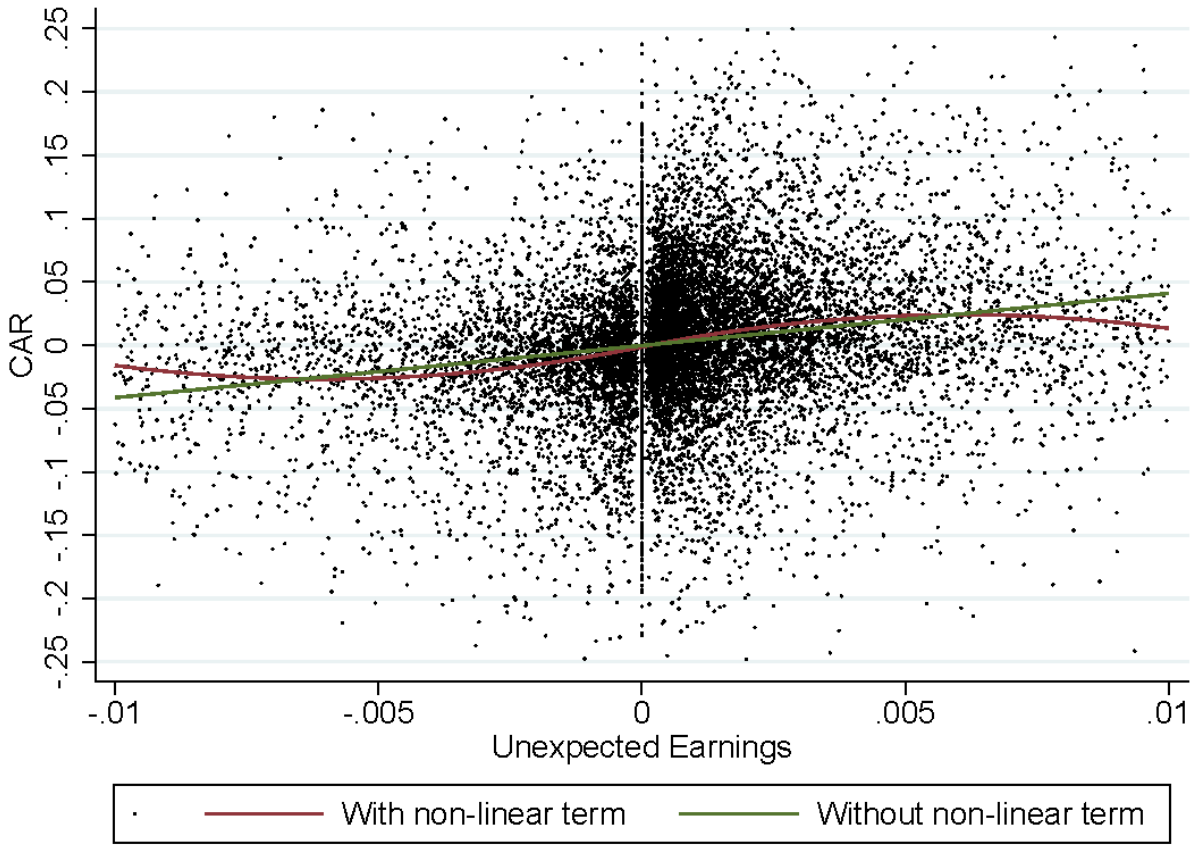

Figure IA6 presents the scatterplot and fitted values for cumulative abnormal returns (*CAR*) and unexpected earnings (*UE*). We plot all firm-year observations from the combined analysis (stacking limited and full inspections and using both alternative cutoff dates), provided that unexpected earnings 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 on *Nonlinear*, essentially preserving the sign of the variable in a way that is similar to using a cubic term. We provide detailed variable definitions in Appendix B of the Manuscript.

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Figure IA7 - Fractional polynomial regression plots

Panel A: Treatment pre vs. post

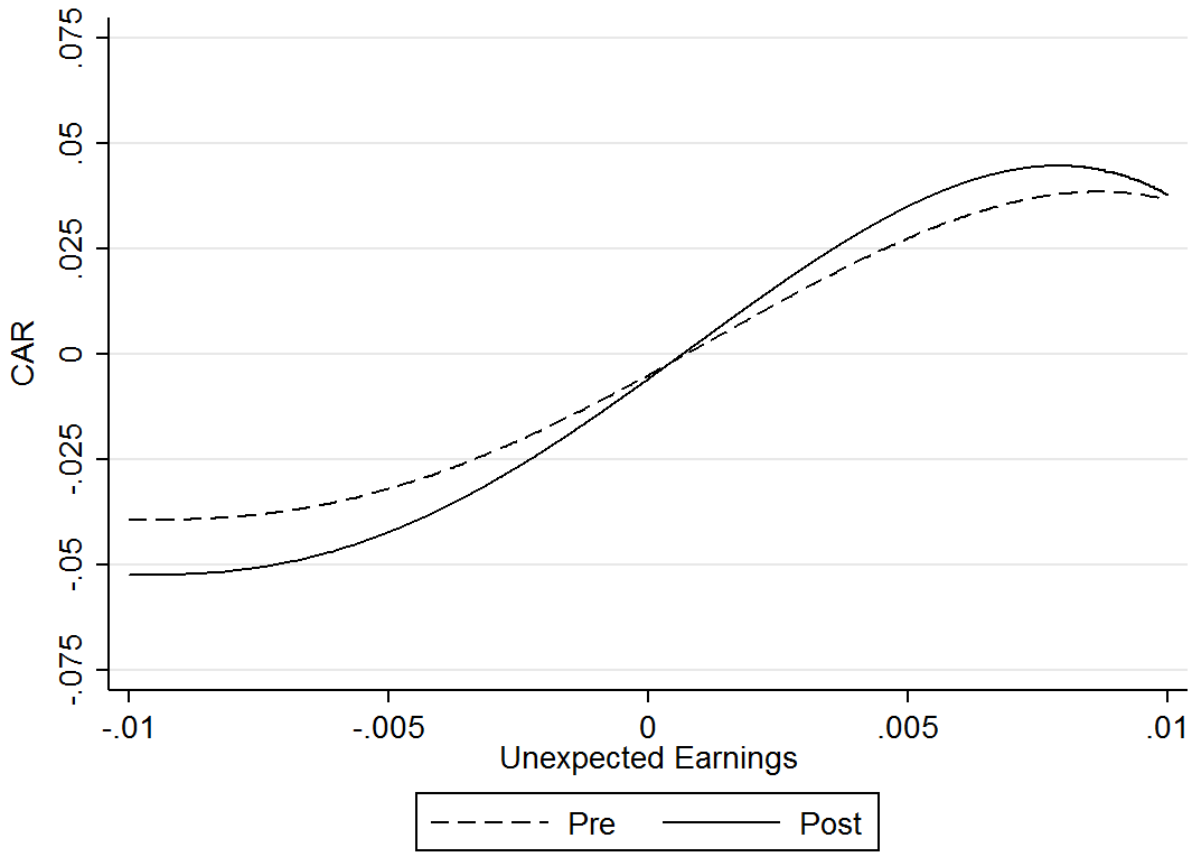

Figure IA7 presents plots of fractional polynomial regressions of cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*) using Eq. (1) from the Manuscript. Fractional polynomial regressions provide flexible parameterization for continuous variables without predetermining the shape. The procedure searches over a set of possible polynomial functions for the model that best fits the data. We use Stata's 'fp' function, which allows for the non-integer powers -2, -1, -0.5, ln(x), 0.5, 1, 2, 3 as well as repeated powers multiplied by ln(x). We only include powers for unexpected earnings, but include the full set of control variables. The sample consists of treated firms from the combined analyses, provided that unexpected earnings are within ±1% of price. Panel A plots the fractional polynomial for treatment firms in the pre-inspection (the dashed line) and post-inspection (the solid line) periods. The sample comprises 33,908 observations (or 91.6% of the combined treatment sample). We provide detailed variable definitions in Appendix B of the Manuscript.

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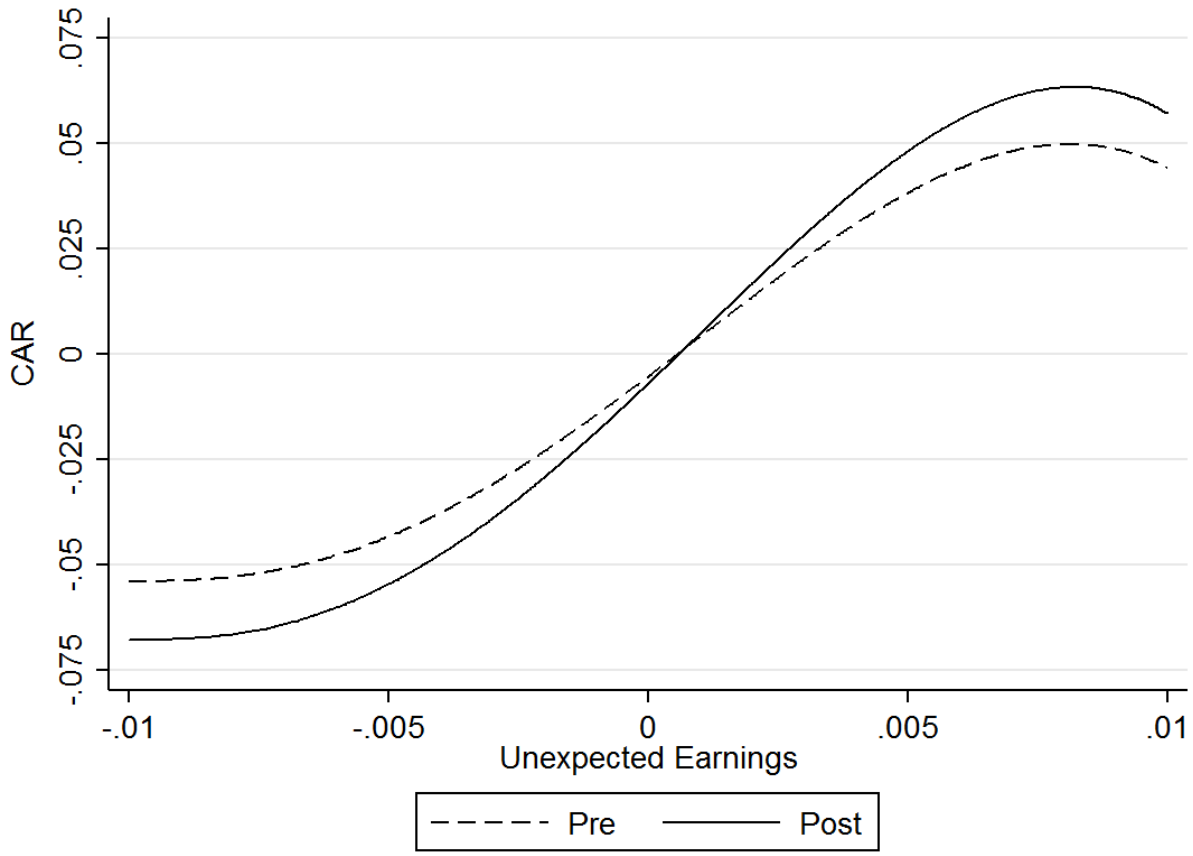
Figure IA7 - Fractional polynomial regression plots (continued)

Panel B: Treatment pre vs. post, profitable firms only



Panel B plots the fractional polynomial using only profitable treatment firms in the pre-inspection (the dashed line) and post-inspection (the solid line) periods. This sample comprises 28,682 observations (or 97.4% of the combined treatment sample of profitable firms).

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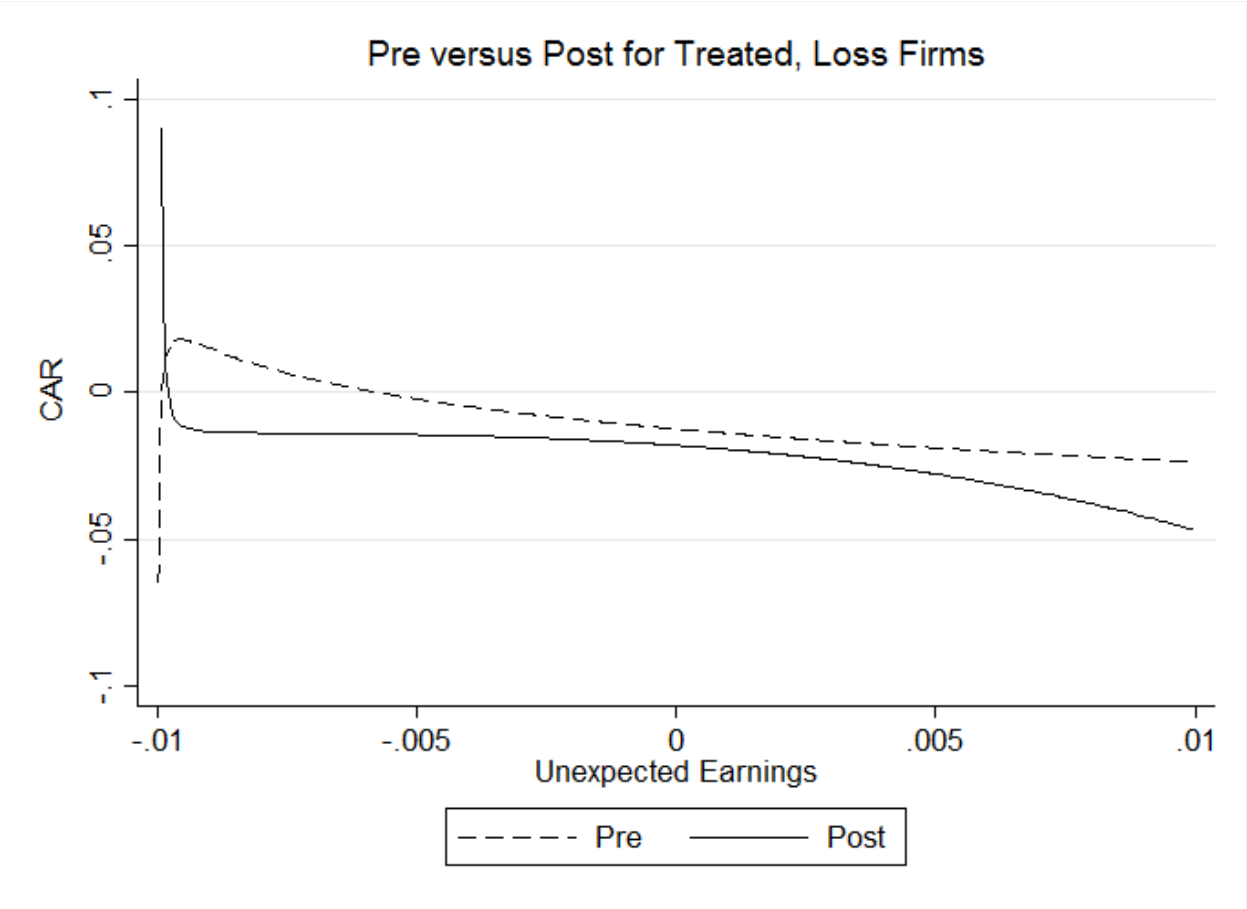
Figure IA7 - Fractional polynomial regression plots (continued)

Panel C: Loss firms



Panel C plots the fractional polynomial using only loss treatment firms in the pre-inspection (the dashed line) and post-inspection (the solid line) periods. This sample comprises 5,226 observations (or 12.8% of the sample).

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Figure IA8: Fractional polynomial regression plots for triennial firms

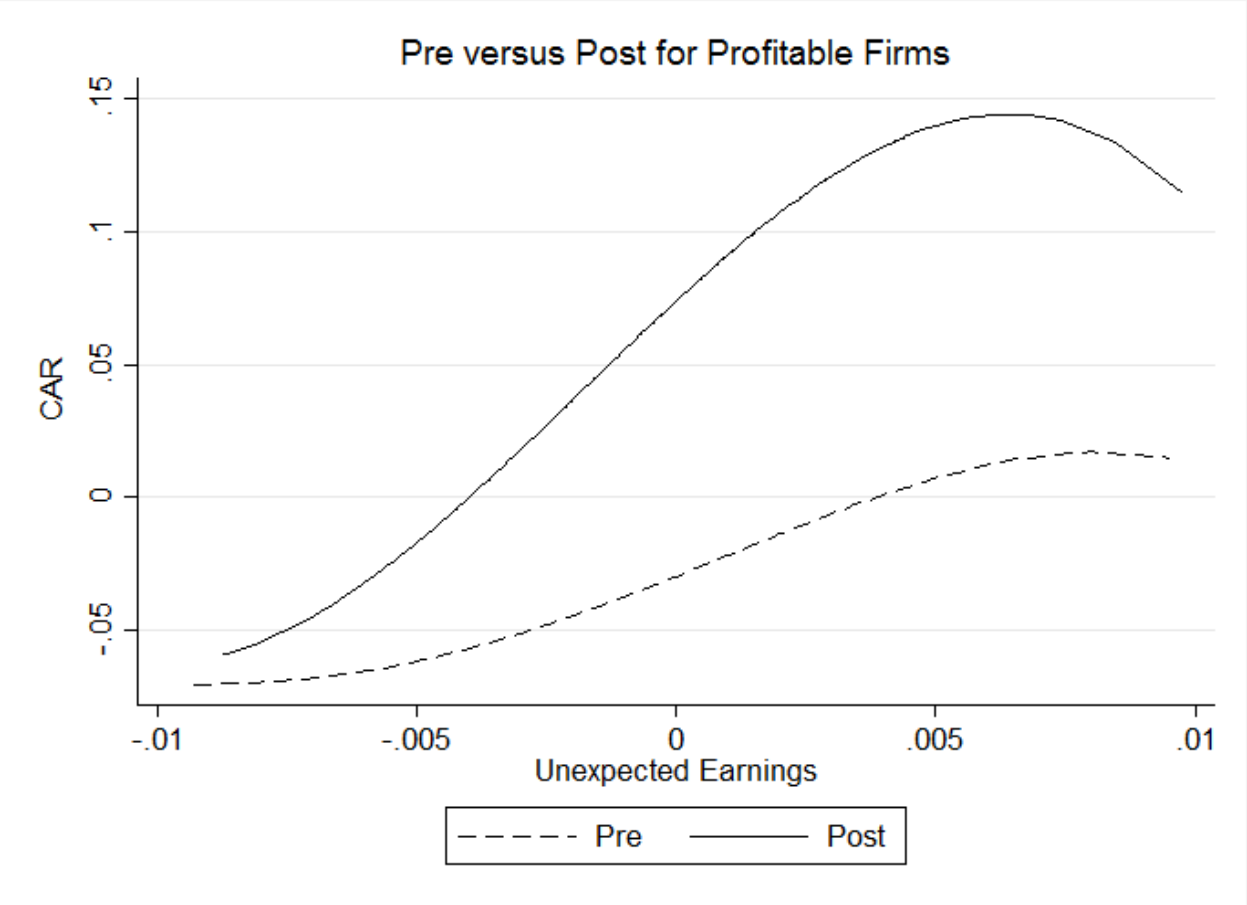

Figure IA8 is comprised of treated profitable firms (i.e., *Loss* = zero) from the triennial inspection analyses, provided that unexpected earnings are within ±1% of price. The pre-inspection period is represented by the dashed line and the post-inspection period is represented by the solid line. This sample comprises 1,242 observations (or 57.8% of the sample).

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Table IA13 - Introducing nonlinearities in the main specification as a control and as an interaction to examine changes in the ERC shape

Dependent Variable: *CAR*	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
UE×Post×Treated	**-**	**0.972*** (4.142)**	**1.006*** (3.865)**	**2.012*** (4.724)**	**2.181*** (5.099)**
UE	2.304*** (9.245)	-	-	-	-
Nonlinear: UE×|UE|	-24.463*** (-11.860)	-29.302*** (-10.725)	-30.169*** (-10.884)	-	-
UE×Loss×Post×Treated	-	-	-0.490 (-0.901)	-	-0.896 (-1.627)
Nonlinear×Post×Treated	-	-	-	-78.975*** (-4.554)	-73.317*** (-4.023)
Firm Characteristics (Controls)	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	Auditor & Country & Year-Quarter	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
Loss & *UE×Loss* interacted with Treatment Indicators	No	No	Yes	No	Yes
Nonlinear interacted with Treatment Indicators	No	No	No	Yes	Yes
Observations	40,766	40,766	40,766	40,766	40,766
R-squared	0.041	0.063	0.064	0.073	0.074

Notes on the next page.

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Table IA13 reports results for the combined analysis while allowing for nonlinearities in the estimation of the ERC. We add *Nonlinear* (i.e., $UE \times |UE|$) and its interactions to Eq. (1); this is equivalent to introducing a sign-preserving squared term. We regress *CAR* on: *UE*, indicators for PCAOB inspections (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). 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 home country, and the year-quarter of the respective fiscal year end, as well as interactions of these fixed effects with *UE*. In all columns, we estimate robust regressions. In Column (1), we exclude the regime change indicators in order to provide a benchmark ERC regression that includes *Nonlinear*. In Column (2), we report the equivalent specification from Table 3 Panel A(1) of the Manuscript, but include the control variable *Nonlinear*. In Column (3), we report the equivalent specification from Table 3 Panel B(1) of the Manuscript, but include the control variable *Nonlinear*. In Column (4), we interact *Nonlinear* with the treatment indicators to allow nonlinearities to change due to the new regime. In Column (5), we include interactions between the treatment indicators and *Loss*, *UE*×*Loss*, and *Nonlinear*. We cluster all t-statistics (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.

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Section 9: Including additional fixed effects in the analysis

In this section, we present the results of several additional analyses that gauge the extent to which our results are robust to the inclusion of additional fixed effects. Specifically, we add industry fixed effects, firm-group fixed effects, and consider an alternative approach that allows us to directly proxy for the ERC (as opposed to using a regression-based coefficient estimate) and include it as an LHS variable in our regression analyses. These analyses supplement Table 4 Panel B of the Manuscript, but add firm-level main effects (in addition to firm-group fixed effects).

Industry fixed effects

Table IA14 Panels A and C report analyses including industry fixed effects (interacted with *UE*) and industry×time fixed effects (interacted with *UE*) for our annually and triennially inspected samples, respectively. Across both panels, our results are robust to the inclusion of these fixed effects, mitigating the concern that static industry-level differences or industry trends drive our results.

Firm-group fixed effects

Table IA14 Panels B and D report analyses including firm-group fixed effects as main effects and firm-group fixed effects interacted with *UE*. The rationale for using firm groups is that because our main outcome variable is estimated as an interaction on the RHS, a within-firm analysis with firm-fixed effects requires an interaction between the firm indicator and *UE*. However, our main analysis typically has four observations per firm, making it infeasible to estimate firm fixed effects and their interaction with *UE*. Given this constraint, we instead estimate results using firm-group fixed effects for firms with similar characteristics (this method is similar to a portfolio sort in asset pricing). First, we group firms into portfolios based on *Size* and *Beta*. We choose *Size* and *Beta* because treatment and control firms exhibit differences along these two dimensions and because both are important determinants of firms' ERC. As a substitute for traditional firm fixed effects, we include indicators for each firm group and its interaction with *UE*.

In Panel B, we present results over four- and six-year periods for between 100 and 900 firm groups for our annually inspected auditor sample. In Panel D, we present results for between nine and 25 groups for the triennially inspected auditor sample. Overall, the results are similar to those in the Manuscript. We note that the annual inspected sample with 900 portfolios and a four-year series is not significant at conventional levels in a two-sided test (i.e., $p = 0.144$).

Alternative ERC estimation approach

In Table IA15, we present results from an alternative approach that estimates changes in the ERC around the introduction of the PCAOB. Here, we proxy for the ERC using *CAR* divided by *UE*, which is then the dependent variable in our analyses. This approach allows for a more conventional use of firm fixed effects. This approach still estimates the firm fixed effect with relatively few observations per firm, but it allows us to perform a within-firm analysis without interacting *UE*

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with the firm fixed effect. This approach is closer to a conventional difference-in-differences analysis with the outcome variable of interest on the LHS. However, this ERC proxy is noisy and so we use the same robust regression approach to construct weights (in order to reduce the influence of outliers) and then estimate a WLS regression that uses these weights. In addition to dropping extreme values of the ERC variable, we also drop observations when *UE* equals zero.

Table IA15 Panel A estimates benchmark models using the LHS ERC (*CAR*/*UE*) variable in three different approaches that each control for outliers. The benchmark estimate of the average magnitude of this variable helps us assess the economic significance of our later results. We use the same control variables as in the Manuscript and use a time period before the Enron/Arthur Andersen scandals to estimate a benchmark ERC, which is our constant in these regressions. In addition to using WLS, we control for outliers in a few different ways: (1) we restrict the range of *UE*, (2) we trim the sample, and (3) we restrict the range of the dependent variable. The estimated magnitudes for the average ERC are larger than traditional ERC estimates (see, e.g., Kothari 2001). The controls *Loss*, *Market-to-Book*, and *Leverage* all load in the predicted directions. *Beta* does not load in the predicted direction, but is not consistently significant at conventional levels across columns. *Size* and *Persistence* do not have significant coefficient estimates.

Table IA15 Panel B replicates Table 4 Panel A of the Manuscript using the ERC proxy as the dependent variable. In these specifications that use the same fixed effects, *Leverage* and *Persistence* are not significant. *Size* and *Beta* are now significant in the expected direction. The main variable of interest, *Post*×*Treated*, is positive and statistically significant. Thus, the main result holds across specifications when using this alternative approach. The coefficient estimates are larger than those presented in the Manuscript, but as shown in Panel A, the baseline estimates for average ERC are also larger.

In Table IA15 Panel C, we introduce firm or firm-group fixed effects, essentially replicating Table 4 Panel B of the Manuscript. We continue to find significant increases in the ERC around the introduction of the PCAOB regime.[5]

Overall, the analyses above provide additional, corroborating evidence that neither time-invariant, firm-level heterogeneity in the market response to firms' earnings nor sample composition changes over time are likely to drive our main findings around the introduction of the PCAOB regime.

[5] A similar analysis for triennially inspected auditors does not yield statistically significant coefficient estimates, but the coefficients are positive and quite large, suggesting that noise in the ERC variable and lack of power are responsible for the insignificance.

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Table IA14 Additional fixed effects (industry or firm-group)

Panel A - Main Test with industry fixed effects

	(1)	(2)	(3)	(4)
Dependent Variable: *CAR*				
UE×Post×Treated	**0.621*** **	**0.614** **	**0.600** **	**0.732** **
	(2.647)	**(2.212)**	**(2.444)**	**(2.470)**
UE×Loss×Post×Treated		-0.371		-0.633
		(-0.708)		(-1.215)
Firm Characteristics	Yes	Yes	Yes	Yes
Fixed Effects	Auditor, Country, YQ, & FF48 Industry	Auditor, Country, YQ, & FF48 Industry	Auditor, Country, & YQ × FF12 Industry	Auditor, Country, & YQ × FF12 Industry
Treatment Indicators	Yes	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes
UE×Fixed Effects	Yes	Yes	Yes	Yes
UE×Treatment Indicators	Yes	Yes	Yes	Yes
Loss & *UE×Loss* interacted with Treatment Indicators	No	Yes	No	Yes
Observations	40,285	40,285	40,766	40,766

Table IA14 presents analyses comparable to tests from the Manuscript but with expanded fixed effect specifications. In Panel A, we report 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 inspections (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). Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include (at least) fixed effects for the auditor (defined at the global network level), the auditor's home country, and the year-quarter of the respective fiscal year end, as well as interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Columns (1) and (2), we also include fixed effects indicating the firm's Fama French 48 industry classification. In Columns (3) and (4), we additionally include fixed effects indicating the firm's Fama French 12 industry classification interacted with the year-quarter of a firm's respective fiscal year end. Columns (2) and (4) replicate Columns (1) and (3), respectively, but allow *Loss* and *UE×Loss* to vary with the introduction of the PCAOB. In all columns, incremental fixed effects are also interacted with *UE*. We cluster all t-statistics (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.

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Table IA14 Additional fixed effects (continued)

Panel B - Main test with firm-group fixed effects

Dependent Variable: *CAR*	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Series	**4-Year**	**4-Year**	**4-Year**	**6-Year**	**6-Year**	**6-Year**
Firm-Group Portfolio (N)	**100**	**400**	**900**	**100**	**400**	**900**
UE×Post×Treated	**0.784*** **	**0.620** **	**0.406**	**0.791*** **	**0.674*** **	**1.015*** **
	(2.959)	**(2.297)**	**(1.460)**	**(3.101)**	**(2.651)**	**(3.855)**
UE×Loss×Post×Treated	-0.136	-0.011	-0.050	-0.105	-0.126	-0.850*
	(-0.249)	(-0.020)	(-0.083)	(-0.234)	(-0.252)	(-1.701)
Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Auditor, Country, Y-Q, & **Firm-Group**	Auditor, Country, Y-Q, & **Firm-Group**	Auditor, Country, Y-Q, & **Firm-Group**	Auditor, Country, Y-Q, & **Firm-Group**	Auditor, Country, YQ, & **Firm-Group**	Auditor, Country, YQ, & **Firm-Group**
Treatment Indicators	Yes	Yes	Yes	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
UE×Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
UE×Treatment Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Loss & *UE×Loss* interacted with Treatment Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Observations	40,766	40,766	40,766	58,554	58,554	58,554

Table IA14 presents analyses comparable to tests from the Manuscript but with expanded fixed effect specifications. In Panel B, we report 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 inspections (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). 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 home country, the year-quarter of the respective fiscal year end, and the firm-group portfolio (described below), as well as interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. Columns (1)-(3) [(4)-(6)] use the four- [six-] year panel. In Columns (1) and (4), we include firm-group fixed effects; these are generated by forming 100 portfolios (10-by-10) using the controls *Size* and *Beta* from the first year that a firm enters the sample. In Columns (2) and (5), firm-group fixed effects are generated by forming 400 portfolios (20-by-20) using the same approach. In Columns (3) and (6), firm-group fixed effects are generated by forming 900 portfolios (30-by-30) using the same approach. We cluster all t-statistics (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.

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Table IA14 Additional fixed effects (continued)

Panel C - Triennially inspected auditors with industry fixed effects

Dependent Variable: *CAR*	**(1)**
UE×*Post*	1.048**
	(2.494)
Firm Characteristics	Yes
Fixed Effects	Auditor, Year-Quarter, & FF48 Industry
Treatment Indicators	Yes
UE×Firm Characteristics	Yes
UE×Fixed Effects	Yes
UE×Treatment Indicators	Yes
Loss & *UE*×*Loss* interacted with Treatment Indicator	Yes
Observations	1,338

Table IA14 presents analyses comparable to tests from the Manuscript but with expanded fixed effect specifications. In Panel C, we report results for the triennially inspected auditor sample using the report release cutoff. 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, 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, the year-quarter of the respective fiscal year end, and the firm's Fama French 48 industry classification, as well as interactions of these fixed effects with *UE*. We estimate a robust regression. We cluster all t-statistics (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.

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Table IA14 Additional fixed effects (continued)

Panel D - Triennially inspected auditors with firm-group fixed effects

Dependent Variable: *CAR*	**(1)**	**(2)**	**(3)**
Firm-Group Portfolio (N)	**25**	**16**	**9**
UE×*Post*	2.064***	1.077**	1.722***
	(4.413)	(2.380)	(4.384)
Firm Characteristics	Yes	Yes	Yes
Fixed Effects	Auditor, Year-Quarter, & **Firm-Group**	Auditor, Year-Quarter, & **Firm-Group**	Auditor, Year-Quarter, & **Firm-Group**
Treatment Indicators	Yes	Yes	Yes
UE×Firm Characteristics	Yes	Yes	Yes
UE×Fixed Effects	Yes	Yes	Yes
UE×Treatment Indicators	Yes	Yes	Yes
Loss & *UE*×*Loss* interacted with Treatment Indicator	Yes	Yes	Yes
Observations	1,338	1,338	1,338

Table IA14 presents analyses comparable to tests from the Manuscript but with expanded fixed effect specifications. In Panel D, we report results for the triennially inspected auditor sample using the report release cutoff. 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, 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, the year-quarter of the respective fiscal year end, and the firm-group portfolio (described below), as well as interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Column (1), we include firm-group fixed effects; these are generated by forming 25 portfolios (5-by-5) using the controls *Size* and *Beta* from the first year that a firm enters the sample. In Column (2), firm-group fixed effects are generated by forming 16 portfolios (4-by-4) using the same approach. In Column (3), firm-group fixed effects are generated by forming 9 portfolios (3-by-3) using the same approach. We cluster all t-statistics (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.

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Table IA15 Main tests with the ERC Variable on the LHS

Panel A - Pre-Enron period estimates as a benchmark: Baseline ERC regressions

Dependent Variable: ***ERC = CAR / UE***	**(1) WLS, |*UE*|<1%**	**(2) WLS, Trim 1%, 99%**	**(3) OLS, |*ERC*| < 100**
Intercept	**5.902*** **	**3.479*** **	**5.360*** **
	(4.177)	**(4.202)**	**(3.920)**
Loss	-3.226***	-2.424***	-2.054**
	(-3.314)	(-4.938)	(-2.527)
Size	-0.110	0.004	-0.247
	(-0.484)	(0.030)	(-1.081)
Market-to-Book	0.250**	0.185***	0.072
	(2.205)	(2.755)	(0.646)
Leverage	-0.332***	-0.189***	-0.217***
	(-4.160)	(-3.750)	(-2.676)
Persistence	0.352	0.099	0.172
	(0.673)	(0.340)	(0.357)
Beta	-1.480**	-0.526	-1.337*
	(-1.989)	(-1.260)	(-1.892)
Observations	8,552	10,061	8,802
Adjusted R-squared	0.002	0.003	0.018

Table IA15 Panel A presents baseline ERC regressions using an alternative approach without *UE* interactions. Here, we proxy for the ERC using CAR divided by UE, which is then the dependent variable in the regression analyses. This approach allows for a more conventional use of firm fixed effects. For the benchmark regressions, we use a firm-year sample that extends for five years and ends in November 2001, i.e., prior to the Enron and Arthur Andersen events (a standard ERC regression for this sample period is available in Table IA16). Column (1) presents estimates from a WLS specification using weights from a first stage robust regression with observations where |*UE*| is less than 1%. Column (2) trims dependent variable outliers at 1% and 99% and then presents coefficient estimates from a WLS specification using weights from a first stage robust regression. Column (3) presents estimates from an OLS specification where we limit the dependent variable to the range (-100,100). We provide detailed variable definitions in Appendix B of the Manuscript. All t-statistics (in parentheses) are based on standard errors clustered at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

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Table IA15 Main tests with the ERC Variable on the LHS (continued)

Panel B - Changes in reporting credibility around the introduction of the PCAOB regime in combined analyses (i.e., Table 4 Panel A)

Dependent Variable: ***ERC = CAR/UE***	**(1) Primary Design**	**(2) Dropped Obs. Design**	**(3) Primary Design CEM**	**(4) Primary Design Loss Interactions**
Post×Treated	**2.135****	**2.334****	**3.219*****	**1.955****
	(2.560)	**(2.461)**	**(2.636)**	**(2.037)**
Loss	-3.448***	-3.202***	-3.410***	0.088
	(-6.816)	(-6.609)	(-6.440)	(0.056)
Size	0.668***	0.680***	0.662***	0.687***
	(4.233)	(4.508)	(3.966)	(4.346)
Market-to-Book	0.432***	0.417***	0.467***	0.442***
	(4.885)	(5.071)	(4.983)	(4.993)
Leverage	-0.047	-0.015	-0.046	-0.050
	(-0.962)	(-0.318)	(-0.893)	(-1.026)
Persistence	-0.033	-0.139	0.130	-0.031
	(-0.081)	(-0.368)	(0.291)	(-0.076)
Beta	0.882**	0.998***	1.019**	0.906**
	(2.239)	(2.693)	(2.356)	(2.297)
Loss×Post×Treated	-	-	-	-1.017
				(-0.470)
Firm Characteristics	Yes	Yes	Yes	Yes
Treatment Indicators	Yes	Yes	Yes	Yes
Fixed Effects	Auditor, Country, & Year-Quarter	Auditor, Country, & Year-Quarter	Auditor, Country, & Year-Quarter	Auditor, Country, & Year-Quarter
Loss interacted with Treatment Indicators	No	No	No	Yes
Observations	36,672	34,540	35,749	36,672
Adjusted R-squared	0.014	0.024	0.014	0.014

Table IA15 Panel B presents analyses that are analogous to those in Table 4 Panel A of the Manuscript. However, here, we proxy for the ERC using CAR divided by UE, which is then the dependent variable in the regressions. This approach allows for a more conventional use of firm fixed effects. Zero-valued *UE* observations result in 4,094 fewer observations than in the sample from the Manuscript. Other table/panel/column descriptions are comparable to those in Table 4, including estimation and standard error calculations.

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Table IA15 Main tests with the ERC variable (continued)

Panel C - Changes in reporting credibility around the introduction of the PCAOB regime in combined analyses (i.e., Table 4 Panel B)

Dependent Variable: ***ERC = CAR/UE***	**(1) Primary Design** **Firm Effects**	**(2) Primary Design** **Firm-Group Effects**	**(3) 6-Year Design** **Firm Effects**	**(4) 6-Year Design** **Firm-Group Effects**
Post×Treated	**2.859*** **	**2.062** **	**3.101*** **	**2.121** **
	(2.890)	**(2.131)**	**(3.144)**	**(2.235)**
Loss×Post×Treated	-1.371	-0.601	-1.671	-1.925
	(-0.552)	(-0.275)	(-0.662)	(-0.913)
Firm Characteristics	Yes	Yes	Yes	Yes
Treatment Indicators	Yes	Yes	Yes	Yes
Fixed Effects	**Firm**, Auditor, Country, & Year-Quarter	**Firm-Group**, Auditor, Country, & Year-Quarter	**Firm**, Auditor, Country, & Year-Quarter	**Firm-Group**, Auditor, Country, & Year-Quarter
Loss interacted with Treatment Indicators	Yes	Yes	Yes	Yes
Observations	36,672	36,672	52,735	52,735
Adjusted R-squared	0.255	0.030	0.211	0.029

Table IA15 Panel C presents analyses that are analogous to those in Table 4 Panel B of the Manuscript. However, here, we proxy for the ERC using CAR divided by UE, which is then the dependent variable in the regressions. This approach allows for a more conventional use of firm fixed effects. Zero-valued *UE* observations result in 4,094 (5,819) fewer observations than in the sample from the Manuscript for the four-year (six-year) panel. Other table/panel/column descriptions are comparable to those in Table 4, including estimation and standard error calculations.

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Section 10: Pre-Enron baseline ERC and calculation of magnitude

In this section, we estimate a baseline pre-treatment-period ERC (calculated without fixed effects for treated firms in the pre-Enron period), which we use as a benchmark against which to assess the economic magnitude of the estimated treatment effects in our primary analyses. We discuss the return response to an earnings surprise of 1% for firms in the pre-PCAOB-inspection period relative to same size effect in the post period (see page 27 of the Manuscript). Table IA16 is the pre-Enron benchmark model.

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Table IA16 - Pre-Enron period estimates as a benchmark

Panel A: Baseline ERC regression

Dependent Variable: *CAR*	**OLS, |UE| < 1%**
UE	**3.681*** **
	(3.472)
Loss	-0.004
	(-1.386)
Size	-0.000
	(-0.333)
Market-to-Book	0.001**
	(2.065)
Leverage	-0.001***
	(-3.779)
Persistence	-0.002
	(-1.606)
Beta	-0.004**
	(-1.996)
UE×Loss	-1.971**
	(-2.560)
UE×Size	-0.132
	(-0.767)
UE×Market-to-Book	0.137
	(1.313)
UE×Leverage	-0.138**
	(-2.213)
UE×Persistence	0.334
	(0.885)
UE×CAPM Beta	0.553
	(0.905)
Observations	9,569
R-squared	0.018

Table IA16 presents measures to assess economic magnitudes. Panel A provides estimates of a baseline ERC regression specification that follows a modified Eq. (1) but without fixed effects or indicators for PCAOB inspections. We use a firm-year sample that extends for five years and ends in November 2001, i.e., prior to the Enron and Arthur Andersen events. We limit the sample to cases where unexpected earnings are less than 1% of price (which excludes 15.2% of the sample). We regress cumulative abnormal returns (*CAR*) on: unexpected earnings (*UE*), control variables, and the interactions of *UE* with control variables. We provide detailed variable definitions in Appendix B. We estimate an OLS regression. All t-statistics (in parentheses) are based on standard errors clustered at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

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Table IA16 - Pre-Enron period estimates as a benchmark (continued)

Panel B: Magnitude estimate

	(1) Calculation	(2)
Baseline ERC from Panel A, pre-Enron	A	3.681
Price change from a 1% of price earnings surprise	B = 1% * A	3.7%
Difference-in-differences ERC (see Table 4 Column 3)	C	0.719
Total ERC	D = A + C	4.400
Price change from a 1% of price earnings surprise	E = 1% * D	4.4%
Changes from pre-Enron to Post = 1 sample period:		
Incremental price change from a 1% of price earnings surprise	F = E - B	0.7%
Estimated percentage increase of price change effects	G = E / B - 1	18.9%

Panel B estimates the total effect using the pre-Enron period sample from Panel A in combination with estimates of the coefficient of interest (the diff-in-diffs from Table 4 of the Manuscript). In Column (1), we describe the calculation performed. In Column (2), we give the magnitudes of interest.

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Section 11: Descriptive statistics for abnormal trading volume sample

Table IA17 presents descriptive statistics for the analysis of the change in abnormal trading volume around 10-K filings after the introduction of the PCAOB regime (Table 8 of the Manuscript). The sample size is much larger than for the ERC analysis (68,830) because we do not require analyst coverage (which is necessary for the ERC analysis). However, as before, the majority of the sample observations (89%) are from the treatment group. On average, *Abnormal 10-K Volume* is positive. A median 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 other control variables, the sample is comparable to the main ERC tests.

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Table IA17 - 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 IA17 presents descriptive statistics for the variables used in the abnormal trading volume analysis (i.e., Table 8 of the Manuscript). We provide detailed variable definitions in Appendix B. We include observations from limited 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 that the same firm enters the sample 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).

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