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Abstract

This paper studies the effects of reporting credibility in capital markets and whether public regulatory oversight of the audit profession enhances reporting credibility. We analyze whether market responses to earnings news increase after the introduction of the Public Company Accounting Oversight Board (PCAOB), as predicted by information economics if such oversight enhances reporting credibility. We use a generalized difference-in-differences analysis, exploiting in our design that the new regime affects firms at different points in time, depending on their 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 also find an increase in volume responses to firms' 10-K filings once the new regime is in place. Overall, our results show that public audit oversight can enhance the credibility of audited financial reports, which in turn is priced in capital markets.

1. Introduction

In this paper, we study the effects of financial reporting credibility in capital markets. Information economics suggests that credibility enhances the extent to which investors respond 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 vanish quickly, often triggering regulatory responses to restore it (Hail et al., 2018). Thus, it is important to examine whether regulatory oversight enhances reporting credibility. We analyze this question and, more broadly, the role of reporting credibility in the 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. Given 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 make both independent auditing and firms' audited financials more credible.

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

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credibility of financial reporting. However, it is not clear whether public oversight necessarily improves upon peer review. Auditing faces economic tradeoffs 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 et al., 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 market-wide changes in reporting credibility after the introduction of the PCAOB to learn about the role of reporting credibility in capital markets and, specifically, 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., due to 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]

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, irrespective of its sign, if investors believe the numbers to be more credible. We operationalize reporting credibility with the earnings response coefficient ("ERC"),

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[1] See, e.g., Wall Street Journal (7/2/2010) "A Missed Opportunity to Kill Sarbox" and Washington Post (7/11/2010) "Critics question effectiveness of auditing oversight board." 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 et al. (2009) characterize the PCAOB's inspection model as "inefficient and dysfunctional."
[2] Our analysis is a joint test of the hypothesis that (i) audit oversight has effects and (ii) 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.

which measures short-window stock-market reactions to (standardized) unexpected earnings news at the earnings announcement. ERCs and their properties have been studied extensively in prior empirical work (e.g., Kormendi and Lipe, 1987; Collins and Kothari, 1989; Easton and Zmijewski, 1989) and have been used 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 et al., 2014). We build on this prior literature and use ERCs to study the effects of public audit oversight.[3]

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

First, we analyze 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 as

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[3] We also validate the ERC 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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well as other aspects of the U.S. regulatory regime, but their non-U.S. auditors were outside the scope of the PCAOB's initial inspections. 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 further after the PCAOB conducts the 2004 full inspections. We also find that the ERC effects are concentrated in profitable firms, as one would expect based on prior work that shows 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.

The key empirical challenge for our analysis is to isolate the effects of the PCAOB regime from other events, including (i) any other contemporaneous macroeconomic or capital-market changes, (ii) other SOX provisions unrelated to audit oversight, and (iii) market responses to the accounting scandals. For instance, regulatory changes for firms' internal controls (as 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 additional evidence that concurrent changes in firms' information environments are unlikely to drive our results.[4] Second, we show that the ERC increases after the regime change are *not* concentrated in

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[4] We show that our findings cannot be explained by changes in: (i) the magnitude of unexpected earnings, (ii) the

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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 robust to separately controlling for the onset of SOX Section 302[a] and Section 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 three years. This staggered implementation allows us to estimate ERC changes *within* small auditors using only 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 and concurrent events. As 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 in the new regime.

Overall, our paper contributes to the existing 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 literature provides evidence that assurance provided by external auditors enhances the pricing of earnings news in the capital markets, often focusing

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timing and relative amount of disclosure prior to the earnings announcement, (iii) analysts' forecast bias, (iv) the accrual component of reported earnings, (v) management earnings guidance, (vi) guidance bundling, or (vii) the bundling of earnings announcements with 10-K releases.

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on auditor reputation and similar attributes (e.g., Teoh and Wong, 1993; Moreland, 1995; Wilson, 2008; Chen et al., 2014; Marshall et al., 2018). By exploiting a regime change in audit oversight that arguably is 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 credibility after a major shock like the accounting scandals in the early 2000s. As such, our study also add to the literature on the merits of private versus public enforcement of regulation (La Porta et al., 2006; Jackson and Roe, 2009), which has tended to focus on the activities of securities regulators. For example, Duro et al. (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 to discipline or incentivize auditors (DeFond, 2010 & 2012). In contrast, we provide evidence on auditors and public regulatory oversight of auditing.

Second, we contribute to the literature evaluating the effects of SOX (see Coates and Srinivasan, 2014 and Leuz and Wysocki, 2016 for reviews). Many of these studies assess the effects of SOX as a whole (e.g., Chhaochharia and Grinstein, 2007; Iliev, 2010). For instance, Iliev (2010) examines specific costs (audit fees), benefits (earnings quality), and the net effects of SOX on market value for smaller firms. In turn, we provide evidence on the capital-market effects of an integral part of SOX—the introduction of the PCAOB. While there is a large literature studying the PCAOB (see Abernathy et al., 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 that are subject to PCAOB inspections using variation in inspections of non-U.S. auditors (e.g., Lamoreaux, 2016; Fung et al., 2014). They also

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study 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 et al., 2011; Boone et al., 2015). Among other things, these prior studies suggest that the PCAOB regime led to changes in auditing, and that capital markets and clients respond to PCAOB inspection reports. These findings are useful in explaining the mechanism of our results, but they do not imply that the new regime enhanced reporting credibility as perceived by investors. In fact, a frequent criticism of the PCAOB is that its inspections led 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 build upon this prior work by showing that these changes in public oversight of auditing matter to investors.

There are also a few studies that examine the PCAOB regime change overall. Specifically, our paper is complementary to Shroff (2017) and Aobdia and Shroff (2017), which focus on real- and audit-market effects. Perhaps closest to ours, Krishnan et al. (2017) 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 the inspected auditors. However, this effect is estimated relative to other clients of the *same* auditor and hence exist 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.

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[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 staggered rollout of the new regime as in our study.

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 market-wide 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, etc.). While studying such outcomes is clearly important, these outcomes do not indicate whether public audit oversight has enhanced 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.

Given this market-based approach, our analysis cannot separately evaluate the impact of specific regime elements, such as changes in audit standards, inspections, or penalties. Rather, our analysis assesses the effects of the new public oversight regime *overall* relative to the prior regime. Because this approach does not elucidate the specific 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 thus lead to improvements in auditing that extend beyond a single engagement; 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

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on each of the three elements of the presumed mechanism. We present this evidence in Section 1 of the Internet Appendix ("IA§1"). Here, we provide a brief summary.

To validate the mechanism's first element, 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, and auditors were inspected by other auditors. Even for large auditors, peer reviews were relatively infrequent—occurring only once every three 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 et al., 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) with respect to 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 provides an assessment of an auditor's compliance with SOX, the rules and standards of the PCAOB, SEC rules, and professional audit standards (PCAOB, 2004a). A full inspection consists of: (1) reviews of selected audits, (2) evaluations of the sufficiency, documentation, and communication of the quality control systems, and (3) other testing of audit procedures as deemed necessary. The PCAOB is endowed with 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 (i) PCAOB inspections identify meaningful weaknesses and deficiencies in the way that audits are conducted (see IA§1 Part 3), and (ii) that these findings lead to subsequent changes in auditing and

financial reporting that go beyond the respective engagement (see IA§1 Part 4). Conceptually, improvements in audit procedures beyond a single engagement are critical for the market to increase its assessment of credibility overall, as the mere revelation of previously unidentified deficiencies would 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.[6] Towards 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 which engagements 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* on future audit engagements. Consistent with this notion, DeFond and Lennox (2017) provide large-sample evidence that PCAOB inspections improve internal control audits.

The PCAOB also evaluates auditors' firm-wide quality control systems. If the auditor addresses any quality control criticisms successfully within a twelve-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-firm-wide quality control changes.

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[6] Note that the PCAOB does not reveal which engagements were inspected, yet produces publicly observable outcomes, in particular auditor-level inspection reports, that allow investors to update credibility assessments.

Such evidence is particularly important for our analysis because quality control criticisms by definition are broader and extend 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 clearly had meaningful information from several sources about the new regime—including 1) the legislation that created the PCAOB and the initial authoritative pronouncements issued by the PCAOB; 2) PCAOB inspection reports and auditors responses to these reports; and 3) the news media—based upon which they could form assessments of the regime's effects on reporting credibility.

In sum, the institutional facts and descriptive evidence presented above support the three elements of the mechanism through which the new oversight regime could translate into 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 Approach

3.1   *Defining and Measuring Reporting Credibility*

Information economics suggests that credibility enhances the extent to which investors respond to a signal. More specifically, investors should respond more strongly to a given level of earnings surprise (relative to expectations) if it is more credible 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, e.g., an analyst forecast and an earnings announcement. They show under fairly general conditions that the

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variance of the price reaction to the second release (i.e., the earnings announcement) is unambiguously non-decreasing in the signal-to-noise ratio of the earnings surprise (see their Proposition 1). An increase in credibility is 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). We therefore use the strength of short-term market responses to earnings surprises (or ERC) as a proxy for credibility.

In addition to having a sound theoretical underpinning, there is substantial empirical precedent for using ERCs as a proxy for investors' assessments of reporting credibility (see Kothari, 2001; Dechow et al., 2010, for reviews).[7] Many empirical studies use ERCs in audit-specific settings to assess the capital-market effects of changes in audit quality and as a proxy for reporting credibility (e.g., Teoh and Wong, 1993; Hackenbrack and Hogan, 2002; Francis and Ke, 2006; Wilson, 2008; Chen et al., 2014; Marshall et al., 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 along with variation in auditors and firms' fiscal year ends.[8] ERCs do not change in anticipation of expected improvements in audit quality and reporting credibility unlike other capital-market outcomes such as stock returns or the cost of capital. That is, investors are not expected to change their responsiveness to earnings surprises until the new regime is in place and auditors have been treated (i.e., inspected by the PCAOB). This feature is critical when using a staggered design. Moreover,

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[7] In IA§2, we provide additional evidence to support our use of ERCs to capture audit-related differences in reporting credibility including an examination of 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.
[8] In IA§3 we present a stylized timeline for the introduction of the PCAOB regime and the related changes in reporting credibility and ERCs. This timeline provides the conceptual underpinnings for our research design.

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ERCs are measured over short windows at specific points in time (i.e., an earnings announcement). This construction, along with the fact that firms' earnings announcements are spread out in calendar time and depend on pre-determined fiscal year ends, are desirable features from an identification perspective.

Of course, using ERCs requires assumptions and has its drawbacks. First, ERCs require a measure of expected earnings 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. This requirement likely introduces noise and reduces the power of the analyses. We use several approaches to deal with the noise in the ERC estimation, 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 (e.g., Collins and Kothari, 1989) and employ a difference-in-differences design, which strips out time-invariant biases in the ERC estimation in order to isolate credibility effects.

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

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3.3   Timing of the Regime Change and Control Firms

If public audit oversight is effective in increasing reporting credibility, we expect ERCs to increase after auditors and firms have been treated under the new regime. Thus, it is critical to isolate when the treatment occurs and 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 Appendix A, Panel 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 over which the PCAOB regime was introduced (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 event, the limited or full inspections, is most relevant. The initial 2003 inspections were limited in scope.[9] Moreover, it takes some time for a) auditors to adjust their audit procedures in meaningful ways and b) investors to learn about these changes. Thus, the first full inspections are more likely to be the relevant treatment dates 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, but also potential drawbacks. First, cross-listed control firms are audited by non-U.S. Big-Four and Grant Thornton affiliates that are not subject to PCAOB inspections in 2003 or 2004, but are required to comply with other SOX provisions at the same

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

time as U.S. issuers (with one exception discussed later).[10] This feature helps us separate the PCAOB regime and other SOX provisions. Second, these issuers are exposed to the 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 U.S. (e.g., Bailey et al., 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, cross-listed issuers could be subject to similar treatments in their home countries if these countries implement audit-oversight reforms similar to those in the U.S.[11] Moreover, it is possible that non-U.S. auditors change their audit procedures because the PCAOB inspects their U.S. affiliates. Both types of 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 inspections of small auditors over three years, which allows us to (only) use 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 the timing of the inspections. This within-group design greatly mitigates concerns about the parallel-trends assumption.[12] The three-year rollout helps to control for unrelated macroeconomic shocks and concurrent regulatory changes (including SOX). The primary drawbacks of this analysis are: (i) the relatively small

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[10] 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.

[11] 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.

[12] 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).

sample of U.S. issuers with triennially-inspected, small auditors and (ii) the possibility that auditors in later inspection cohorts could make anticipatory adjustments based on the results from earlier inspection cohorts. This concern about auditor adjustments ahead of inspections 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 three events are not independent. Moreover, there is the possibility that our effects could be biased downwards 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 the later stages of the regime rollout) provides little indication of anticipatory improvements by the auditors. Thus, it seems reasonable to expect that investor responses to earnings surprises do not change until auditors have actually been treated by the new regime and investors learn about these changes.

Based on this logic, the earliest possible date that the ERC would reflect an increase in credibility is after the completion of the PCAOB's inspection fieldwork for a particular auditor. The latest date for an ERC response is the public release of the inspection report. As it is not obvious when exactly 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 announcements after these alternative dates.

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Using the fieldwork-end date as the cutoff, we define an issuer as treated when its fiscal year end occurs in or after the month that inspection fieldwork ends for its auditor.[13] By that time, the auditor can use information gathered from its PCAOB inspection to improve other audits that have not advanced from the planning stage.[14] If the inspection leads to 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 the completion of the fieldwork. However, many fiscal year ends occur well after the completion of fieldwork and there is an additional 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 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 date, then 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 as the cutoff date, we define an issuer as treated when it announces its 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 of how strict audit oversight is (e.g., relative to their expectations at the enactment of SOX). In principle, this adjustment could go in either direction. For instance, it is conceivable that the inspection reports

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[13] For the Big Four, the fieldwork typically lasts between five to seven months. For small auditors, inspections are shorter, and hence we add 30 days to the completion of the fieldwork in defining the cutoff date. See Appendix A, Panels A-C for more details on timing and an illustration of our research design.

[14] Aobdia (2018) notes that PCAOB inspectors normally share feedback on the deficiencies identified in the audit engagement during the on-site inspections. For example, the substance of the inspection comment forms, which are a precursor to Part I findings, is shared on-site when conducting the fieldwork (Riley et al., 2008).

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reveal that the oversight regime is less strict than expected, so credibility goes 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 website. All data are publicly available. For the first analyses of annually-inspected auditors, we use observations over a four-year window surrounding treatment, i.e., two fiscal years *before* and *after* the respective cutoff date. For the limited inspections, using the fieldwork (inspection report) cutoff date, the sample includes firms with fiscal year-end dates between December 2001 and November 2005 (June 2002 and May 2006). 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 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 includes firms with year-end dates between June 2002 and December 2006 (July 2003 and November 2007). For the full-inspection control sample, we exclude cross-listed firms from countries that had an inspection agreement with the PCAOB during or before the analysis window.[15] We also include firms from countries that are unavailable for

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

inspections in the control group.[16]

Panel A of Table 1 provides details on the sample composition for the treatment and control groups by auditor, inspection type, and treatment dates for the limited and full inspection analyses. For the limited inspections, the number of treatment firms is similar across auditors. For the full inspections, the Big Four have a similar number of treatment firms while other large (Tier-Two) auditors have fewer firms. Combining inspections and respective groups our treatment sample includes 4,289 unique domestically-audited firms over 37,001 firm-years, and our control sample includes 579 unique non-U.S. firms over 3,765 firm-years.[17] In IA§5, we provide a breakdown of the treatment and control samples by the location of the auditor.

Panel B of Table 1 provides details on 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, our analysis excludes fiscal years ending beyond Q2 of 2008.

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 cumulative abnormal return (*CAR*) at the earnings announcement and the earnings surprise or unexpected earnings (*UE*), which are the key inputs to estimate the ERC. Our primary control variables are *Loss*, *Size*, *Market-to-Book*,

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[16] http://pcaobus.org/International/Inspections/Pages/IssuerClientsWithoutAccess.aspx (Accessed January 2015).
[17] Non-U.S. Grant Thornton affiliates are included in the full inspection control sample. Other Tier-Two auditors are not included because Audit Analytics does not identify foreign affiliates of these auditors. We do not include Grant Thornton in the control group for the limited inspections to provide a clean within-Big-Four comparison.

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Leverage, *Persistence* and *Beta*. The other variables in Table 2 are used in additional sensitivity tests. We also count the number of days between the respective cutoff date (i.e., either the end of fieldwork or the report release) for the auditor's initial treatment 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 not surprising given that exchange-traded, cross-listed firms tend to be quite large. It is therefore 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.

The descriptive statistics for the control variables for the firms with triennially-inspected auditors are reported in Panel D. 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 and initial full inspections in 2004. We estimate the following equation:

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

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CAR is the 3-day (*t-1*, *t=0*, and *t+1*) cumulative abnormal return, 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 respective cutoff date for the new regime, and zero otherwise. As discussed in Section 3.3, we use two alternative cutoff dates—the fieldwork-end and inspection-report-release dates (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 ends, 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 date 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-end dates, which provides more variation. Our primary coefficient of interest in Eq. (1) 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 increase in the response to earnings news following 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, as well as *UE×Loss*. As losses are less persistent than profits, the earnings response to negative earnings is likely lower than for positive earnings (Hayn, 1995). Second, we include *Size*, *Market-to-Book*, *Leverage*, *Persistence*, *Beta*, and 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, 1989;

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Easton and Zmijewski, 1989; Dhaliwal et al., 1991).

We include fixed effects for the auditor's global network and 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 et al., 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.[18] We rely on the robust regression as our primary specification because we view it as an effective and non-discretionary way to reduce the influence of outliers.[19] In all tests, we cluster standard errors by firm.[20] We provide definitions of each variable in Appendix B.

Table 3 Panel A presents the robust regression results of estimating Eq. (1) using each of the four alternative dates for the onset of PCAOB regime: limited inspection fieldwork (Column 1), limited inspection report release (Column 2), full inspection fieldwork (Column 3), and full inspection report release (Column 4). Because there is significant overlap in the measurement

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

[19] Prior studies use a variety of approaches to deal with extreme *UE* observations, including deleting observations for which *UE* exceeds a specified percentage of price (e.g., 100%) and deleting observations with large standardized residuals (e.g., Collins and Kothari, 1989; Teoh and Wong, 1993; Francis and Ke, 2006; Chen et al., 2014). In IA§6, we present scatter plots 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.

[20] We calculate robust, firm-level-clustered standard errors using a WLS regression based on the weights from the robust regression. In our setting, clustering (i.e., by year or year-quarter) is problematic given the short time-series of the analysis (e.g., Petersen 2009; Conley et al., 2018). However, our inferences are very similar with double clustering by firm and earnings-announcement month (untabulated).

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), using the limited inspection fieldwork end date, *UE×Post×Treated* is positive but statistically insignificant. In Column (2), the treatment effect following the limited-inspection report release is significant at the 10% level. In Columns (3) and (4), where *Post* is defined based on the full inspection fieldwork-end date and the full-inspection report release, respectively, *UE×Post×Treated* is positive and significant at the 5% level (at least) and ranges in magnitude between 1.149 and 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 have taken place.

In Table 3, Panel B, we present results from an alternative design that reduces the potential contamination effects from the overlap in the pre- and post-period when using alternative cutoff dates (e.g., in the primary design, the pre-period for the report release overlaps with the post-period for the fieldwork). In the alternative design, we exclude pre-period fiscal year ends that occur during fieldwork and prior to the release of the inspection report. Appendix A, Panels B and C provide an illustration of the limited and full inspection designs without such overlap (which we call the "dropped observations" design). Results with this design are stronger, particularly in Column (4) where the overlap or contamination is likely to be 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.

In Table 4, we present results stacking the samples for the limited and full 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 measurement dates.

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This presentation is parsimonious without favoring a particular date and exploits the variation in firms' fiscal year ends more effectively, which is why we use it for the subsequent analyses. In this specification, $UE \times Post \times Treated$ is positive and significant at the 1% level (Column 1).[21] We view this specification as delivering a conservative estimate of the treatment effect because it pools the relatively small response following 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 also significant at the 1% level.

The key assumption underlying our identification strategy is that our 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 $Post \times Treated \times UE$ interaction term with separate interactions for each of the years in our sample period, 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 provides support for the parallel-trends assumption.[22] 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, showing 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 reason, we also conduct an analysis using coarsened exact matching (CEM) (see Blackwell et al., 2009) based on these two firm characteristics. CEM relies on covariate weighting to construct a

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[21] In IA§7, for completeness, we report a full tabulation of all coefficient estimates, excluding fixed effects and fixed effects interacted with *UE*.

[22] In IA§4, we examine past trends in ERCs for our treatment and control firms over an extended time period and again find no evidence that calls into question the validity of the parallel-trends assumption.

synthetic control sample, allowing us to preserve sample size. We coarsen our sample into 20 CEM bins (per matching variable), which reflects a tradeoff between preserving observations and the ex-post similarity of treatment and control groups for the matching variables. We then use the weights from this coarsening in estimations of Eq. (1). After applying the CEM weights, the 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 those in Column (1) without CEM weights.

Next, we introduce a cross-sectional split into our analysis. This split 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 due to the transitory nature of losses (Hayn, 1995).[23] 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 the *Loss* indicator 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 firms, we find that the results become stronger and that the credibility effects of the new regime is concentrated in profitable firms, which is consistent with our expectations and corroborates our

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[23] We confirm in untabulated results that the ERC for firms with losses (i.e., negative earnings) is near zero.

interpretation (i.e., the *UE×Post×Treated* coefficient is 0.942).[24]

Finally, in Table 4 Column (5), we present results for a within-firm analysis where we demean both *CAR* and *UE* at the firm level (by taking the residual of each of these indicators regressed on firm-indicators). Although this approach is not exactly equivalent to a specification that includes firm fixed effects interacted with *UE*, it provides a way to control for static firm level differences that might explain our results. The fact that the ERC is estimated from an interaction, and that we have fairly few observations in the time-series, limits the number of fixed effects that we can include and interact with *UE* in Eq. (1). That is, it effectively controls for a firm's average ERC without including a large number of additional interaction terms. To increase the number of observations per firm in this analysis, we increase our treatment window to three years before and after treatment. Results based on this approach are consistent with our earlier analyses and provide further evidence that our results are not attributable to time-invariant firm characteristics.[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). Then, 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

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[24] 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, as well as their composition.
[25] In IA§9, we present results for two additional analyses that confirm that our results are robust to the inclusion of fixed effects for the Fama and French 12 industries and "pseudo-firm fixed effects," which are based on firm characteristics and industry groupings.

that in the post-period an earnings surprise of 1% translates into a price change of about 4.4% compared to 3.7% in the pre-period. This effect is sensible and economically meaningful.

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 analyses. For instance, it is conceivable that the new regime itself affects elements in the information environment that are 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 subsequent to 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 firms disclose prior to the earnings announcement as a proportion of the total amount of 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*). We describe each of these measures in detail in Appendix B. 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 primary analyses, successively replacing *CAR* in Eq. (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

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and auditor-, country-, and year-quarter fixed effects. Table 5 presents the regression results. Across all seven of the information environment proxies, the coefficient on *Post×Treated* is economically small, suggesting that the other information proxies are not much affected. The effects are generally not significant, except for *UE* in Column (1) and *Relative Information* in Column (4). The documented decrease in *UE* in Column (1) suggests that analyst forecast bias slightly decreases for treated firms in the post period. But, aside from being small in magnitude, this change is not a concern as we already control for *UE* in Eq. (1) when we estimate the ERC for a given level of earnings surprise.

The observed increase in *Relative Information* in Column (4) suggests that in the new regime, treated firms release more of the year's total information prior to the earnings announcement. The increase of 0.026 is about 18% of one standard deviation of the measure (0.144). Again, this effect is relatively small. More importantly, if firms release more of the year's total information prior to the earnings announcement, this shift 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* working against us, the treatment effect for *UE×Post×Treated* increases slightly (0.876) 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 controls (untabulated). Overall, there is no evidence that significant changes in pre-earnings-announcement disclosures, management guidance, earnings'

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properties, and/or analyst forecast behavior explain our findings.[26]

In our second set of sensitivity analyses, we address the possibility that the observed ERC change could be 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 gauge this concern, 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 firms with other auditors. Thus, if our results reflect the effects of 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 alternative 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 the financial statements, and 3) Section 404[b] rules regarding the assessment of internal controls.[27] Rules on

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[26] In untabulated analyses, we also examine the possibility that our results could be explained by an increase in external monitoring accompanying the additional raising of external capital following PCAOB inspections 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 comes first and is the mechanism that leads to subsequent increases in capital raising.

[27] In addition, the PCAOB adopted several new auditing standards. However, we consider audit changes attributable to the new standards 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.

audit committee independence became effective on April 25, 2003 for domestic *and* foreign issuers, and thus affect both our treatment and control groups simultaneously (SEC Release Nos. 33-8220; 34-47654). Similarly, Section 302 had an effective date of August 29, 2002 for all domestic and foreign issuers (SEC Release No. 33-8124).

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-end dates on or after November 15, 2004. For non-accelerated filers, the SEC deferred the implementation 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 et al., 2008). Thus, if firms improve their internal controls, and if better internal 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 non-accelerated 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 non-accelerated filers. Results in Columns (3) and (4) of Table 6 are consistent with this prediction. The treatment effect for non-accelerated filers is 1.139 compared to 0.871 for accelerated filers. These coefficients are not significantly different and, if anything, indicate a larger ERC change for non-accelerated filers—a result that goes against the alternative explanation.

Second, we separately examine ERC changes within the subsample of 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 (rather than public audit oversight), then 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), and 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.[28] 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 the triennial inspections, estimating the following equation:

$$CAR_{i,t} = \alpha + \beta_1 UE_{i,t} + \beta_2 Post_t + \beta_3 UE \times Post_{i,t} + \lambda_n Controls_{i,t} + \gamma_n FixedEffects +$$
$$\beta_n UE \times Controls_{i,t} + \beta_n UE \times FixedEffects_{i,t} + \varepsilon_{i,t} \tag{2}$$

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

CAR, *Post,* and *UE* are calculated as defined previously.[29] 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*.[30] With this fixed-effects structure, the identification of the treatment effect, *UE×Post*, comes solely from variation in the timing of the 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 subsequent to Q2 of 2008 to mitigate the potentially confounding effects of the financial crisis. As in Table 3, we separately examine two alternative cutoff dates: the completion of fieldwork and inspection report release.[31]

Table 7 presents results for this analysis. In Column (1), we estimate a robust WLS regression of Eq. (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 find similar results (using the report release date), 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 pre-inspection report release sample to avoid overlap and contamination. The treatment effect (1.022) is similar to the

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[29] There are two exceptions. First, for triennially-inspected firms, fieldwork is shorter and it is less clear that the market is aware of the timing of the fieldwork. Thus, we code the *Post* variable equal to one for any earnings announcement occurring 30 days after the end of the PCAOB's inspection fieldwork (or alternatively, the day following the inspection report release). Second, because small firms have less analyst coverage, we extend the window over which we measure the median analyst forecast (from which *UE* is computed) from 95 days to 360 days.

[30] 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.

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

other specifications. Again assuming a pre-period benchmark return response for a 1% earnings surprise of 3.7%, an earnings surprise of the same magnitude in the post-PCAOB-regime period leads to a price change of about 4.7%—an increase of about 28% for clients of triennially inspected auditors. Overall, the results for small auditors are consistent with our earlier findings for large auditors and hence a significant increase in reporting credibility following the introduction of the PCAOB regime.

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 disclosure (e.g., Asthana and Balsam, 2001; Asthana et al., 2004; Leuz and Schrand, 2009), it is likely also a function of 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 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 hence can be simply observed at the firm-year level, which should make it less noisy and allows us to introduce firm fixed effects. The drawback of this measure is that there is no obvious way to compute the news component (or surprise) for the 10-K filing to standardize

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reactions, as we do for the ERC.

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

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

$$Abnormal\ 10\text{-}K\ Volume_{i,t} = \alpha + \beta_1 Post_t + \beta_2 Treated_i + \beta_3 Post \times Treated_{i,t} + \lambda_n Controls_{i,t} + \gamma_n FixedEffects + \varepsilon_{i,t}$$

(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 same treatment and control samples as in our primary analyses for the 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 from a firm's fiscal year end to the 10-K release (*Filing Delay after FYE*) and from the earnings announcement to the 10-K release (*Filing Delay after EA*), following Asthana et al (2004). We also include *Analyst Following*, 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

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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.

We present regression results in Table 8. In Column (1), we estimate Eq. (3) using OLS and include auditor-, country-, and year-quarter fixed effects. In Column (2), we introduce 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 employ CEM, based on *Size*, *Beta*, and *Loss*, using a similar approach as the one described for Table 4.[32] 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 10K.

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] and 302[a]. While the magnitudes and standard errors for the coefficient of interest 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 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 how an increase in audit oversight by a quasi-public regulator affects capital-market responses to firms' earnings surprises—as information economics would suggest if the new

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[32] We additionally 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.

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 audit inspections regime established by SOX to replace the prior self-regulatory regime. The introduction of the PCAOB regime affects firms at different points in time depending on their fiscal year ends, auditors, and the timing of PCAOB inspections. Consistent with an increase in reporting credibility after the introduction of 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.

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 two years, ERCs are based on investor perceptions and can change as more information about the oversight regime (as well as reporting and audit quality) becomes publicly available. Second, the attribution of the credibility effect to public audit oversight depends critically on our ability to control for other concurrent changes in regulation and in markets with our difference-in-differences analyses. Third, because ERCs are difficult to measure and can be noisy, 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 other SOX 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. 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

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

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

Appendix A provides details on the timing of the introduction of the PCAOB audit oversight regime. Panel A provides the beginning and end dates for PCAOB fieldwork and the inspection report release dates for both limited and full inspections by auditor.

Appendix A –Details on the Introduction of the PCAOB Regime (continued**)**

Panel B: Limited Inspections – Treatment Timing

Fieldwork begins (6/2003)
Fieldwork concludes (12/2003)
Reports Released (8/2004)

06/01 12/01 06/02 12/02 06/03 12/03 06/04 12/04 06/05 12/05 06/06

Primary:

Fieldwork Pre	Fieldwork Post

Inspection Report Pre	Inspection Report Post

Dropped observations:

Fieldwork Pre	Drop	Fieldwork Post

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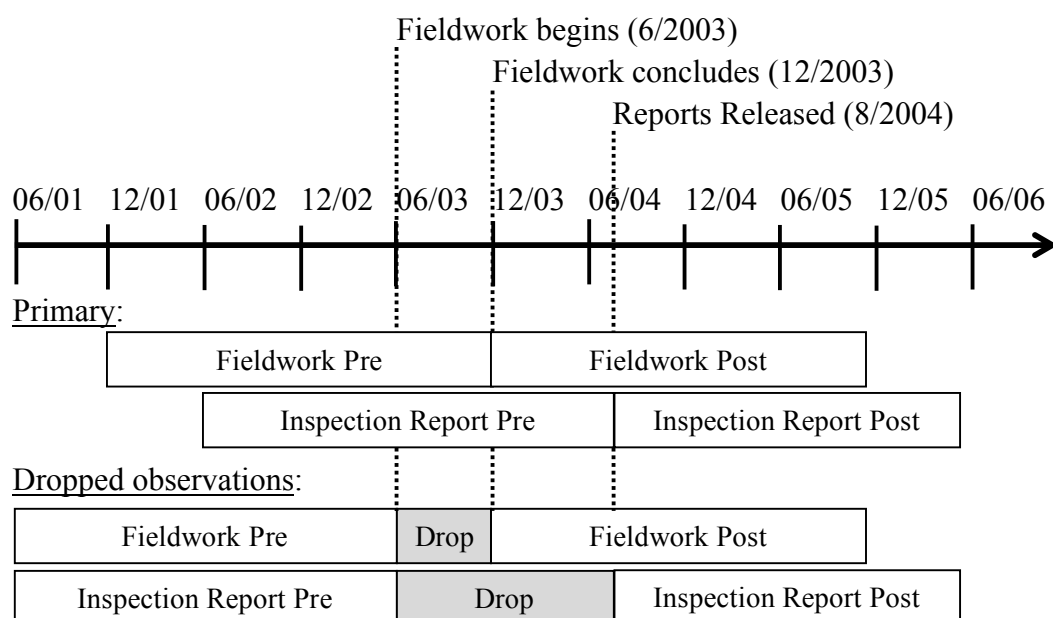
Panel B describes the coding of the *Post* variable around the Limited Inspections. We use two different designs. In the "primary design," we use the conclusion of the fieldwork or the release of the inspection report as alternative cutoff dates to define adjacent pre and post periods. We then use two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

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Appendix A –Details on the Introduction of the PCAOB Regime (continued**)**

Panel C: Full Inspections – Treatment Timing (specific dates presented from Deloitte & Touche as an example)
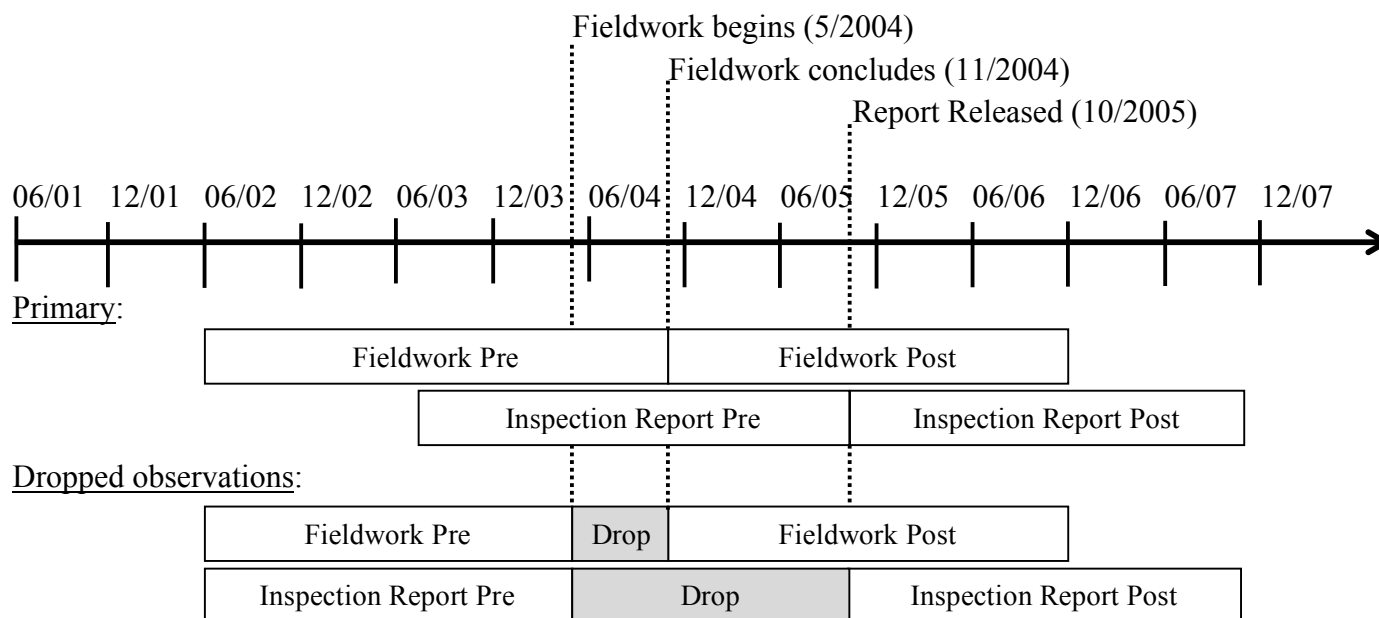
Panel C describes the coding of the *Post* variable around the Full Inspections. We use two different designs. In the "primary design," we use the conclusion of the fieldwork or the release of the inspection report as alternative cutoff dates to define adjacent pre and post periods. We then use two earnings announcements in the pre and the post period for a given firm. In the "dropped observations" design, we exclude fiscal year ends that occur *during* PCAOB fieldwork when using the fieldwork end as the cutoff date and fiscal year ends that occur between the start of fieldwork and the release of the inspection report when using the inspection report release as the cutoff date. The idea of the dropped observations design is to avoid contamination from overlapping cutoff dates. Timeline dates are presented MM/YY.

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Appendix B - Variable definitions

Variables Used in Calculating Earnings Response Coefficients

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

PCAOB Inspection Indicators

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

Control Variables

Analyst Following$_{i,t}$	The count of the number of unique analysts who issue at least one forecast on I/B/E/S in a window beginning 360 days prior to the earnings announcement and ending 3 days prior to the earnings announcement. When no forecasts are observed, we set this count to zero.
$Beta_{i,t}$	The coefficient from regressing excess daily returns for firm i on excess market returns over one calendar year, ending on the fiscal year end date. The risk free rate is collected from Ken French's data library.
Filing Delay after EA$_{i,t}$	The count of the number of days between the earnings announcement date defined as the earlier of that available on Compustat or I/B/E/S and the filing date of the 10-K defined as the earlier of the date reported by Audit Analytics or WRDS SEC Analytics.
Filing Delay after FYE$_{i,t}$	The count of the number of days between the firm's fiscal year-end date from Compustat and the filing of the 10-K, defined as the earlier of the date reported by Audit Analytics or WRDS SEC Analytics.
$Leverage_{i,t}$	The ratio of total liabilities to total equity, measured at the fiscal year end, from Compustat.
Log 10-K File Size	The natural logged value of the file size for the firm's 10-K SEC filing from WRDS SEC Analytics.

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$Loss_{i,t}$	An indicator variable coded as one when basic earnings per share excluding extraordinary items (Compustat epspx) is less than zero, and zero otherwise.
$Market\text{-}to\text{-}Book_{i,t}$	The ratio of the market value of equity to the book value of equity, measured at the fiscal year end, from Compustat.
$Nonlinear_{i,t}$	A variable equal to $UE_{i,t} \times |UE_{i,t}|$, equivalent to using a cubic term in the regression.
$Persistence_{i,t}$	The coefficient from regressing basic EPS excluding extraordinary items from Compustat on lagged EPS using (where available) up to 10 years of data.
$Size_{i,t}$	The log of market value of equity, measured at fiscal year end, from Compustat.
$SOX302a_{i,t}$	An indicator variable coded as one when the "IS EFFECTIVE" variable in the Audit Analytics SOX 302 data set is coded as a '0', '1', or '2', and zero otherwise. This variable is only coded 1 for domestic firms.
$SOX404b_{i,t}$	An indicator variable coded as one when the auditor internal control opinion (AUOPIC) variable in Compustat shows an adverse, qualified, or unqualified indicator, and zero otherwise. This variable is only coded 1 for domestic firms.

Alternative Dependent Variables

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

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

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

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Figure 1: Mapping Out the Estimated Treatment Effect in Event Time
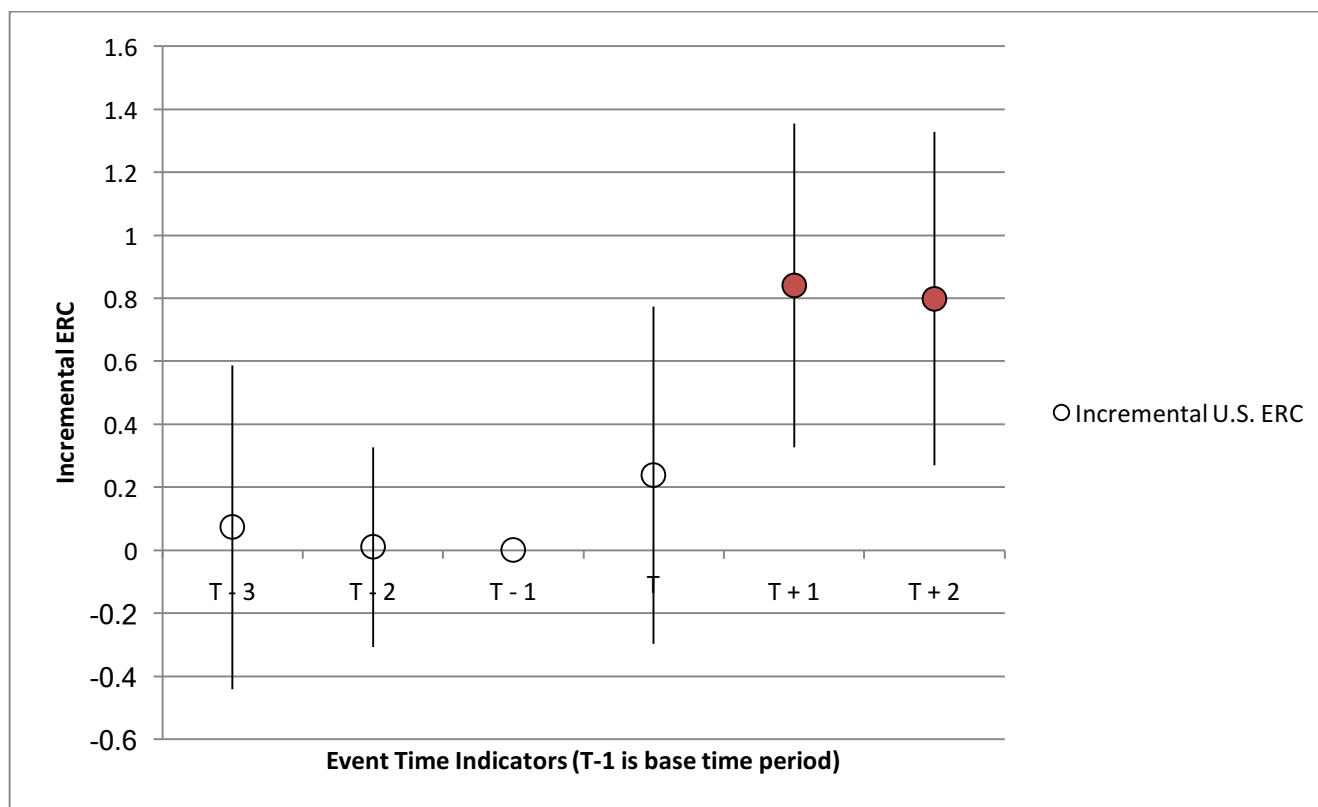
Figure 1 presents simultaneously-estimated trends in earnings response coefficients (ERCs) for the combined sample using profit firms only (i.e. *Loss* = 0), which stacks the limited and full inspections analyses for each cutoff date (i.e., the end of fieldwork and report release) using the dropped observation design (see Appendix A, Panels B and C). 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 pre-treatment 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 Eq. (1). We include auditor and country fixed effects interacted with *UE*. Each line bar represents two 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. We provide detailed variable definitions in Appendix B of the manuscript

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Table 1: Sample Composition

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

Table 1: Sample Composition (continued)

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

	Fieldwork		Inspection Reports	
	Newly Inspected Auditors	**Unique Firms**	**Newly Reported-on Auditors**	**Unique Firms**
Calendar Year	**(1)**	**(2)**	**(3)**	**(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 had registered with the PCAOB. At the end of the sample period, all but four auditors in our sample have been inspected, and thirty-two had not yet had an inspection report released. Column (1) [Column (2)] reports the number of newly-treated auditors by calendar year, using the inspection fieldwork [report release] as cutoff date. Column (3) [Column (4)] reports the number of unique client firms associated with the newly-inspected auditors. In the last row, we report the number of firm-years contributed by these firms.

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

Panel A: Annually-Inspected U.S. Auditors (Treatment Group)

Variable	N	Mean	Std. Dev	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. We provide detailed variable definitions in Appendix B. We include observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample), so the same firm enters multiple times (see Table 1). We truncate all continuous variables, except *UE*, at 1% and 99% by fiscal year. *UE* is truncated at 2.5% and 97.5% by fiscal year. Panel A presents descriptive statistics for firms with 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 this panel provides the average number of days from the respective cutoff date (end of fieldwork or inspection report release) to the (treated) firm's first earnings announcement.

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

Panel B: Non-U.S. Auditors of U.S. cross-listed firms (Control Group)

Variable	N	Mean	Std. Dev	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 non-U.S. 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.

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

Panel 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 uniquely contain U.S. or Non-U.S. subsamples. All t-statistics are based on standard errors clustered at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

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

Panel D: Triennially-Inspected Auditors

Variable	N	Mean	Std. Dev	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 give descriptive information on the timing assigned to the *Post* variable for the fieldwork, report release, and dropped-observation designs. In the dropped-observation design, we lose 325 (24.3% of the sample) earnings announcements that are between the beginning of fieldwork and the report release. The last row in this panel for fieldwork and report release timing provides the average number of days from the respective cutoff date (end of fieldwork or inspection report release) to the (treated) firm's first earnings announcement.

Table 3: Changes in Reporting Credibility around the Introduction of the PCAOB Regime for Limited and Full Inspections

Panel A: Analyses using the Primary Design

Dependent Variable: *CAR*	(1) Limited Inspections Fieldwork	(2) Limited Inspections Reports	(3) Full Inspections Fieldwork	(4) Full Inspections 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	0.048	0.037	0.041	0.067

Table 3 presents separate analyses for each inspection event (limited and full) and each measurement cutoff date (end of fieldwork and report release). Panel A reports results for our analysis using the "primary design" as described in Appendix A Panels B/C. Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). For brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or the interactions among many of these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. 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, included 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.

Table 3: Changes in Reporting Credibility around the Introduction of the PCAOB Regime for Limited and Full Inspections (continued)

Panel B: Analyses for the Dropped-Observations Design

Dependent Variable: *CAR*	(1) Limited Inspections Fieldwork	(2) Limited Inspections Reports	(3) Full Inspections Fieldwork	(4) Full Inspections 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	0.041	0.034	0.043	0.060

Panel B reports results for our analysis using the "dropped observations" design as described in Appendix A Panels B/C. Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). For brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or the interactions among many of these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. 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, included 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.

Table 4: Changes in Reporting Credibility around the Introduction of the PCAOB Regime in Combined Analyses

Dependent Variable: *CAR*	(1) Primary Design	(2) Dropped Obs. Design	(3) Primary Design CEM	(4) Primary Design Loss Interactions	(5) 6-Year Design Firm Effects Residuals
UE×Post×Treated	**0.788*****	**0.874*****	**0.719****	**0.942*****	**0.951*****
	(3.473)	**(3.543)**	**(2.230)**	**(3.589)**	**(2.690)**
UE×Loss	-0.761***	-0.699***	-0.755***	-0.093	-0.941**
	(-6.226)	(-5.857)	(-5.787)	(-0.378)	(-2.113)
UE×Size	-0.023	0.016	-0.033	-0.022	-0.060
	(-0.610)	(0.441)	(-0.777)	(-0.604)	(-1.054)
UE×Market-to-Book	0.054**	0.050**	0.061**	0.064***	0.033
	(2.384)	(2.335)	(2.557)	(2.845)	(1.164)
UE×Leverage	-0.024**	-0.025**	-0.032***	-0.024**	-0.014
	(-2.294)	(-2.371)	(-3.028)	(-2.271)	(-1.080)
UE×Persistence	-0.034	-0.009	-0.090	-0.043	-0.120
	(-0.405)	(-0.124)	(-1.028)	(-0.501)	(-0.963)
UE×Beta	0.301***	0.284***	0.378***	0.247**	0.271*
	(2.955)	(2.901)	(3.243)	(2.396)	(1.892)
UE×Loss×Post×Treated	-	-	-	-0.803	-1.428**
				(-1.520)	(-2.075)
Firm Characteristics	Yes	Yes	Yes	Yes	Yes
Treatment Indicators	Yes	Yes	Yes	Yes	Yes
*UE×*Treatment Indicators	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
*UE×*Fixed Effects	Yes	Yes	Yes	Yes	Yes
Loss & *UE×Loss* interacted with Treatment Indicators	No	No	No	Yes	Yes
Observations	40,766	38,511	39,843	40,766	58,554
Adjusted R-squared	0.051	0.049	0.058	0.052	0.027

Notes on subsequent page.

Table 4 presents analyses for a combined sample for the inspection events (limited and full) and the measurement cutoff dates (end of fieldwork and report release). Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). For brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or the interactions among many of these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Column (1), we estimate the primary design using the combined sample. In Column (2), estimate the dropped-observation design using the combined sample. We describe these designs in Appendix A Panel B/C. 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 Columns (4) and (5), we allow for heterogeneous treatment among profit and loss firms by interacting the treatment indicators with the *Loss* control variable. Column (4) is otherwise consistent with Column (1). Column (5) uses a 6-year window (as opposed to a 4-year window), and we estimate using residuals for *CAR* and *UE* regressed on firm effects. All t-statistics, included 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 for Columns (1) through (4). We calculate standard errors for Column (5), a two-stage estimation, using a bootstrap across both stages with 1,000 replications selecting clusters of firm observations with replacement.

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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, and report the effects for profit firms. In Column (1) [(2), (3), (4), (5), (6), (7)], we regress *UE* [*Forecast*, *Timeliness*, *Relative Information*, *Scaled Raw Accruals*, *Earnings Guidance*, *Guidance Bundle*] on indicators for PCAOB inspection (i.e., *Post* and *Treated*), controls, and fixed effects. In all columns, for brevity, we do not report coefficients for the control variables, fixed effects, and treatment indicator main effects. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (at the global network level), the firm's country of domicile, and the year-quarter of the respective fiscal year end. In all columns, we report OLS regressions. All t-statistics, included 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.

Table 6: Are the Results Driven by Changes in Reporting Incentives or Other Provisions of the Sarbanes-Oxley Act?

Dependent Variable: *CAR*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Reporting Incentives		**Sarbanes-Oxley Act**				
	Non-AA clients	**Only AA clients**	**Non-Accelerated**	**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	0.054	0.075	0.091	0.050	0.057	0.062	0.053

Notes on subsequent page.

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Table 6 presents sensitivity analyses examining 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, and report the effect for profit firms only. Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on *UE*, indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). In all columns, for brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or the interactions among these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (at the global network level), the firm's country of domicile, and the year-quarter of the respective fiscal year end. In all columns, we estimate a robust regression. 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. In Column (7), we include the indicator variables *SOX404b* and *SOX302a* and their interactions with *UE* as additional controls. All t-statistics, included 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. + indicates significance (two-sided) at the 10% level for tests of coefficient magnitudes relative to the adjacent column on the left. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

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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
UE×Post	0.789**	1.063**	0.915**	1.022**
	(2.125)	(2.387)	(2.231)	(2.247)
UE×SOX404b	-	-	-0.566	-
			(-1.595)	
UE×SOX302a	-	-	-0.120	-
			(-0.604)	
Firm Characteristics	Yes	Yes	Yes	Yes
*UE×*Firm Characteristics	Yes	Yes	Yes	Yes
Treatment Indicator (*Post*)	Yes	Yes	Yes	Yes
Fixed Effects	Auditor & Year-Quarter	Auditor & Year-Quarter	Auditor & Year-Quarter	Auditor & Year-Quarter
*UE×*Fixed Effects	Yes	Yes	Yes	Yes
Loss & *UE×Loss* interacted with Treatment Indicator	Yes	Yes	Yes	Yes
Observations	1,338	1,338	1,338	1,013
Adjusted R-squared	0.682	0.681	0.681	0.823

Table 7 presents results for the analysis of changes in reporting credibility around the introduction of triennial PCAOB inspections. Following Eq. (2), we regress cumulative abnormal returns (*CAR*) on *UE*, an indicator for PCAOB inspection (i.e., *Post*), control variables, fixed effects, the interactions of *UE* with the control variables, the fixed effects, and the treatment indicator (as shown in the table footer). Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Detailed variable definitions are in Appendix B. We include fixed effects for the auditor and the year-quarter of the respective fiscal year end. We estimate robust regressions. In all columns, we estimate the treatment effect for profit and loss firms separately by including additional interactions as noted in the table footer. We report the coefficient of interest for profit firms only. In Column (1), we examine changes in ERCs using the fieldwork cutoff date (i.e., *Post* equals 1 if the firm's fiscal year end is at least 30 days after the date of fieldwork completion). 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) and (4), we perform additional robustness test. In Column (3), we re-estimate the report release design (Column (2)) adding the indicator variables *SOX404b* and *SOX302a* and their interactions with *UE* as additional controls. In Column (4), we examine the triennial inspections setting using the dropped-observation design. We describe these designs in Appendix A Panel B/C. All t-statistics, included 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. We calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Table 8: Changes in Abnormal Trading Volume around 10-K filings after the Introduction of the PCAOB 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	0.038	0.244	0.259	0.260	0.259

Notes on the subsequent page.

Table 8 presents results for an analysis of changes in abnormal trading volume around 10-K filings after the introduction of the PCAOB regime. Following Eq. (3), we regress *Abnormal 10-K Volume* on indicators for PCAOB inspections (i.e., *Post* and *Treated*), control variables, and fixed effects (as indicated in the table footer). We provide detailed variable definitions in Appendix B. We include fixed effects for the auditor (at global network level), the firm's country of domicile, the year-quarter of the respective fiscal year end, and the firm (as indicated in the table footer). In Column (1), we report the baseline specification estimated using OLS. In Column (2), we repeat Column (1), 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 using 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 weights from CEM from Column (3) and include continuous variable *Log 10-K File Size*. In Column (5), we use WLS with weights from CEM from Column (3) and include the indicator variables *SOX404b* and *SOX302a*. All t-statistics, included 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.

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 and the prior AICPA peer-review program. We summarize these institutional details in Section 2 of the manuscript. The primary purpose of this descriptive analysis is to provide 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 <u>descriptive</u> evidence for the mechanism as well as each of the conditions that must be in place for the empirical link documented in the manuscript to be plausible. Specifically, (1) there must be meaningful changes in the way the PCAOB conducts inspections relative to 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, which go 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 new PCAOB regime.

Part 2: Penalties issued by the PCAOB
In this section, we provide a list of the possible penalties that can be imposed by the PCAOB for audit firm deficiencies as well as descriptive information on the actual 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 the restatement for large audit firms for the 2004 inspection year. The purpose of this analysis is to illustrate 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. The section provides specific examples of firms' responses and specific remedial actions. The purpose of this section is to illustrate that the PCAOB regime gives rise 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 have learned about the scope and effectiveness of the PCAOB regime, and hence could have updated 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 to assess the extent to which these changes are plausible alternative explanations for our results or could bias against our findings.

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 greater than 100 public issuers, peer reviews were conducted only every three years, whereas the PCAOB conducts inspections for large auditors annually. Setting frequency aside, the remainder of the discussion focuses on three primary areas of differences: 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 historic information on its peer review program, academics have catalogued its rules and output during the pre-SOX period. Current rules and output of the peer review program are quite different than those in place prior to SOX. The peer review program changed dramatically following the introduction of the PCAOB, both as a result of the perceived failures in the peer review program and the perceived overlap in outputs with the PCAOB inspection program. For 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.

Table IA1A presents quotations from academic publications related to the independence of the AICPA peer review program. Table IA1B discusses the focus and scope of the peer review regime. Table IA1C discusses the focus and scope of the PCAOB review regime. Table IA1D discusses 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 in that the PCAOB regime results in greater independence, a larger array and more frequently-employed penalties, and a broader program scope.

Table IA1A - Peer-review program independence

Author(s)	Year	Journal	Quotes
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 and 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

Author(s)	Year	Journal	Quote
			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 the footnote under "Penalties" below]
DeFond	2010	Journal of Accounting and 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. There were four types of opinion: (1) clean, (2) unmodified with weaknesses, (3) modified, or (4) adverse. Clean |

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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. [Footnotes excluded.]

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 […]

Author(s)	Year	Journal	Quote
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.

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 actual frequency, by year, with which these penalties have been issued (Table IA2B). The availability of a substantial array of penalties and their frequent usage illustrates a specific mechanism through which the PCAOB can affect auditor behavior as well as provides an example for 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 by year and penalty type is presented below (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

Rule 5300	Sanction Description	Year										Total
		2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	
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 its inspection reports, the PCAOB notes instances when an (unnamed) issuer restates audited (and, less frequently, reviewed quarterly) financial statements or makes other financial reporting adjustments in connection with inspection findings. These restatements or reporting changes could arise by: (1) direct evidence of material departures from GAAP uncovered through PCAOB inspection procedures, (2) auditors performing additional procedures as a result of inspection findings that uncover material departures from GAAP, or (3) issuers finding and making corrections of material departures from GAAP as a result of inspection findings. We hasten to add that the link between the inspection findings and subsequent restatements is not necessarily causal in all instances and that PCAOB inspection reports make no claims to this effect. However, in many instances, the circumstances described 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 hence investors could use it to update their assessments of the PCAOB regime as well as their assessments of reporting credibility. The example of restatements related to EITF 95-22 identified in the 2003 Limited Inspections are noteworthy as they illustrate how an identified issue can extend beyond an engagement and a single audit firm. In Table IA3B, we provide further details on the nature of the restatements for large (annually-inspected) auditors for the inspection year 2004, as an example.

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 16 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 include a subjective acceleration clause and a lock-box arrangement. In the initial inspection years, failure to identify EITF 95-22 misclassifications were found to be a widespread issue among many auditors and firms.
Deloitte & Touche LLP	Issuer A, Page 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, Page 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, Page 4-5	Reported securitization transactions as sales that did not qualify under SFAS 140
Grant Thornton LLP	Issuer B, Page 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, Page 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

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 specific remedial actions. These details provide further insight into the specific interactions between auditors and the PCAOB as well as provide anecdotal evidence that these interactions precipitate meaningful changes in audit procedures that extend beyond a single engagement. Conceptually, *subsequent* improvements in audit procedures *beyond a single engagement* are critical for the market to increase its overall assessment of reporting credibility, as 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 during an inspection, the PCAOB begins a dialogue with the auditor to clarify the issue at hand. This dialogue can include the issuance of comment forms for the auditor to respond to the proposed issue. If after this process, the issue is not satisfactorily clarified, 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 as well as 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* for which the issue was raised.

Addressing Inspection Report Part II Findings and Quality Control Criticisms

SOX also codified the process through which auditors could remediate firm-wide, Part II findings (e.g., specific criticisms of and defects in their quality control systems that were identified during the inspections). Satisfactory remediation of Part II findings within a one-year window avoids public disclosure of these findings. We describe the rules and process in Table IA4B, including additional details that the PCAOB subsequently provided on its website. Importantly, in a report posted in 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, which were satisfactorily remediated within a 12-month window (and hence not disclosed), clearly indicating 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 findings for inspection years 2004 through 2009.

In their replies to the initial limited and full inspection reports issued by the PCAOB, some of the Big-Four auditors provided their impressions of the PCAOB's inspection process as well as outlined planned responses to both specific engagement-level findings (Part I) and quality-control criticisms (Part II) in attachments to the publicly-released inspections reports. We provide examples in Table IA4C.

Any inspection case is either resolved through remediation and remains nonpublic or fails to be resolved through remediation and is publicly disclosed. Examples of remedial actions that auditors could take and have taken in response to Part II findings include the following: updating audit templates (e.g., audit planning checklists), required reviews by concurring partners, required timing of completing various planning procedures, analytical procedures, substantive testing, review, enhanced training (e.g., more required, different content, different delivery, different 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. One exception to the confidentiality of the Part II remediation efforts is that Deloitte, on release of its second Part II, included an explanatory letter about its efforts to remediate Part II

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findings 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 (both retroactive in the form of restatements and proactive in the form of audit policy) beyond the specific 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 in which it identified three additional engagements with the same misapplication. Aobdia (2018) also finds that auditors exert more effort (in terms of total audit hours and partner audit hours spent) 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 through 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). [footnotes 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	7/22/04	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	7/22/04	Documentation	We are actively reviewing and revising our documentation policies and procedures to address the new standards proposed by the PCAOB.
Ernst & Young LLP	7/22/04	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	7/22/04	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	7/22/04	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	7/22/04	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	10/26/05	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 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 have learned about the scope and effectiveness of the PCAOB regime from public sources. Overall, the sources in this section (as well as in Table IA4C of the previous section) support the notion that there was a substantial amount of meaningful public information based on which investors could have formed and updated their assessments of the new PCAOB regime, ensuing 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 the notion that investors use 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 release 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 which describes the inspection process. In this description, the PCAOB uses plainer language to describe an inspection and describes fully the "quality control" focus areas that greatly supplement the legal and broad wording used for the inspection rules. Like the prior report, 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 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, and he was impressed with the work the inspectors had done.
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 "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 at bare-bones staffing levels 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 in 2003 at each firm. The versions of the reports that were made public 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 that occurred around the introduction of the PCAOB regime to assess the extent to which 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); c) rules regarding auditor independence; and 3) auditing standards.

Table IA6A provides details on the adoption date of the 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, importantly, they 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 the manuscript in Section 4.2) to address the possibility that the adoption of SOX 404 represents an alternative explanation for our results. Overall, we find little evidence suggesting that our results are attributable to the adoption of SOX 404. In Table 6D, 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 major countries from which we draw control firms in our analyses. 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 between the legislation establishing audit oversight in our sample countries. However, further investigation of the cases where there is some overlap in timing with the introduction of the PCAOB regime reveals that although many countries passed laws establishing audit oversight, few had already established an oversight process and actual 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 redo the combined, unmatched analyses from Table 3, Panels A and B excluding countries with legislation in years 2003 through 2005 and find comparable results. 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 t-statistic of 3.1.

Table IA6C provides details on the timing of 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 new regime. Such effects would still be attributable to the introduction of the PCAOB but they would give rise to 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 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 with AS5 pertain to 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 the audit documentation firms must maintain. We view this change as intertwined with the new PCAOB regime, and thus an element of what we aim to examine. It is 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 this standard has a significant impact on our results.

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Table IA6D presents details on the adoption timing of the other SOX provisions relative to the timing of 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 the triennial inspection analysis is unlikely to be confounded by the concurrent adoption of other SOX provisions.

Table IA6A - Timing of the adoption 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 Nov. 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). The standards that the Board adopted require registered public accounting firms and their associated persons to 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 references in auditors' reports to 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 documentation 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.

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* = 1 for the first time relative to the timing of the firm's first observed SOX 302 opinion using the inspection report release as the cutoff date (i.e., *Post* equals one if a firm's fourth quarter earnings announcement falls on or after the release date of the inspection report, and zero otherwise). Each row separates the firms by fiscal year—indicated in Column (1)—where the treatment indicator, *Post*, switches from 0 to 1. Each column separates the firms by timing distance—indicated in Columns (2) through (10)—to the fiscal year of the firm's first SOX 302 opinion. Each cell counts the number of unique firms for which the firm's initial PCAOB inspection overlaps with the firm's first SOX Section 302 opinion. For instance, the highlighted cell in column (5) shows that four firms had *Post* coded as 1 for the first time in 2006, but issued their first SOX 302 opinion one year earlier in 2005. Column (11) enumerates the number of firms where we observe no SOX 302 opinion. We indicate the SOX adoption year by color and include a reconciliation to the total. Note in Column (4), 12 firms of 529 total firms, only 2.3%, adopt SOX 302 at the same time as the initial PCAOB inspection. When the fiscal year of treatment exceeds 2007, the *Post* variable is equal zero for the series that includes fiscal years 2001 through 2007, i.e. our triennial tests in the manuscript.

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* = 1 for the first time relative to the timing of the firm's first observed SOX 404 opinion using the inspection report release as the cutoff date (i.e., *Post* equals one if a firm's fourth quarter earnings announcement falls on or after the release date of the inspection report, and zero otherwise). Each row separates the firms by fiscal year—indicated in Column (1)—where the treatment indicator, *Post*, switches from 0 to 1. Each column separates the firms by timing distance—indicated in Columns (2) through (8)—to the fiscal year of the firm's first SOX 404 opinion. Each cell counts the number of unique firms for which the firm's initial PCAOB inspection overlaps with the firm's first SOX Section 404 opinion. For instance, the highlighted cell in Column (5) shows that 17 firms had *Post* coded as 1 for the first time in 2006, but issued their first SOX 302 opinion one year earlier in 2005. Column (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 in Column (4), 28 firms of 529 total firms, only 5.3%, adopt SOX 404 at the same time as the initial PCAOB inspection. When the fiscal year of treatment exceeds 2007, the *Post* variable is equal zero for the series that includes fiscal years 2001 through 2007, i.e. our triennial tests in the manuscript.

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 auditor 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 et al., 1991; Dechow et al., 2011; Schrand and Zechman, 2012). Thus, firms with higher F-Scores should have lower reporting credibility and hence lower ERCs if the latter reflect reporting credibility. Consistent with this idea, we find that ERCs and F-Scores provide similar rankings of Big-Four auditor clients (in reverse order), which in turn provides more comfort in our way of measuring changes in reporting credibility in the main analyses.

Second, we examine ERC changes following the PCAOB's December 2007 enforcement action against Deloitte & Touche, which was the first Big-Four auditor to be subject to a settled enforcement action (Dee et al, 2010; Boone et al, 2015).[1] We expect that this enforcement event is likely to reduce the reporting credibility of Deloitte & Touche clients, at least temporarily, and hence we expect ERCs to respond accordingly.[2] For this test, we can 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 of this section reports regression results that show the average F-Score (calculated following Dechow et al., 2011) for each Big-Four audit firm relative to PwC. We use observations from fiscal years 2003 through 2013. These regressions include firm characteristic controls from the paper's main analysis. Results in Column (1) indicate that the clients of PwC have the lowest F-Scores. KPMG clients have slightly higher F-Scores than PwC, but the difference is not statistically significant. The clients of Deloitte & Touche and Ernst & Young both have significantly higher F-Scores than PwC. 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 in F-Scores between the clients of Deloitte & Touche, Ernst & Young, and KPMG are not statistically significant).

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

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[1] The PCAOB commences an enforcement action in cases similar to a Part I finding, though generally where the violation is much more severe, as determined by the Board (PCAOB, 2004d).

[2] This analysis is similar in spirit to prior studies by Wilson (2008) and Chen et al. (2014), which document declines in ERCs subsequent to firm-specific restatements.

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indicators 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 using PwC clients as the benchmark. We find that the ERC differences between PwC clients and Deloitte & Touche and Ernst & Young clients are statistically significant. ERC differences between PwC clients 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 *cause* 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 partly reflect client composition).

For our purposes, the main take away 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 should be viewed as descriptive, it nonetheless consistent with the notion that ERCs reflect differences in reporting credibility. The next test is more stringent in that it exploits a shock to reporting credibility and examines whether ERCs respond accordingly.

ERCs around the Deloitte and Touche Enforcement Event:
For our analysis of the Deloitte & Touche enforcement event, we use a specification similar to that described in Eq. (1). We include all issuers of Big-Four auditors with year-end dates between December 2005 and November 2009. *Post* equals one for all issuer-years with fiscal year-ends following 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 concludes after the dates of the PCAOB enforcement announcement in December 2007.[3] Following our primary analyses, we interact *Loss* and *UE×Loss* with the treatment indicators to measure the treatment effect for profitable firm years, for which 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*, of -1.177 is 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 those variables for which we observe significant differences between Deloitte & Touch clients and other Big 4 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 in a predictable way to a negative shock to auditor reputation and thus provide added support for our use of ERCs as a proxy for reporting credibility.

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[3] Using the earnings announcement date instead of fiscal year-end date leads to the reclassification of only 38 firm years from *Post* = 0 to *Post* = 1 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 with F-Scores PwC as the baseline but also include industry-year fixed effects using Fama-French 48 industry definitions. We cluster all t-statistics, included in parentheses, at the industry level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

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. We generally 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 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 these 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 Column (1), (2), (3), and (4), we examine ERCs for Deloitte, E&Y, KPMG, and PwC, respectively, in separately estimated regressions for the set of 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 ERC estimate for E&Y but not different from ERC estimate for Deloitte. The ERC estimate for E&Y is not significantly different from the ERC estimate for Deloitte. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Table IA9 - Changes in Reporting Credibility around the First Deloitte Enforcement Event of December 2007

Dependent Variable: CAR	(1) Baseline	(2) Matching
UE×Post×Deloitte	-1.177**	-1.131**
	(-2.173)	(-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 changes in ERCs in response to the Deloitte enforcement action that occurred on December 10, 2007. Consistent with the main design from the paper analyzing the PCAOB regime, we use two year windows surrounding this event date and code *Post* to equal one for observations (fiscal year ends) following this date and zero otherwise. We use a modified Eq. (1) with indicator variables adjusted to examine 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). Also, we interact the treatment indicators with the *Loss* and the *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 of the respective fiscal year end, plus 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 the Deloitte enforcement event for profitable firm clients of Deloitte. In Column (2), we examine the same except use (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 4 auditors; unmatched bins result in 372 fewer observations. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Section 3: Stylized Timeline and *Post* Variation

In this Section, we present and discuss a stylized timeline to map out the timing of events and its relation to our research design. This timeline illustrates our research design but also helps us determine when to measure changes in reporting credibility. We discuss this timing in a less formal way 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 shocked reporting credibility, leading 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. Next, SOX passes and the PCAOB is established (at t-3). At that point, investors form expectations about the new regime, i.e., the expected treatment $E(T)$, and the ensuing regime effects on reporting credibility, $C[E(T)]$.

The market response to earnings surprises should not change until auditors are actually treated by the new regime and have had an opportunity to adjust their audit procedures, as indicated by $ERC_{t-2} = ERC_{t-4}$. We assume that the earliest possible date this could occur is 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 exactly the market assumes that treatment has taken place, and hence when ERCs respond, we use both dates as alternative cutoffs, and estimate treatment effects based on ERCs 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 depends on firms' fiscal year-ends, providing additional staggering in the rollout of the regime that we can exploit for identification.

Using the fieldwork-end date as the cutoff, we define an issuer as treated when its fiscal year-end occurs in, or after, the month inspection fieldwork ends for its auditor. By that time, the auditor can use information gathered from its PCAOB inspection to improve other audits that have not advanced out of the planning stage. If the inspection leads to improvements in audit quality beyond the inspected engagements, and investors learn about these improvements (or expect them to have taken place), reporting credibility should increase shortly after the completion of the 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 a considerable amount of time between the completion of the fieldwork and the time we measure the ERC effect, giving auditors time to adjust their audit procedures and for the market to become aware of these changes.
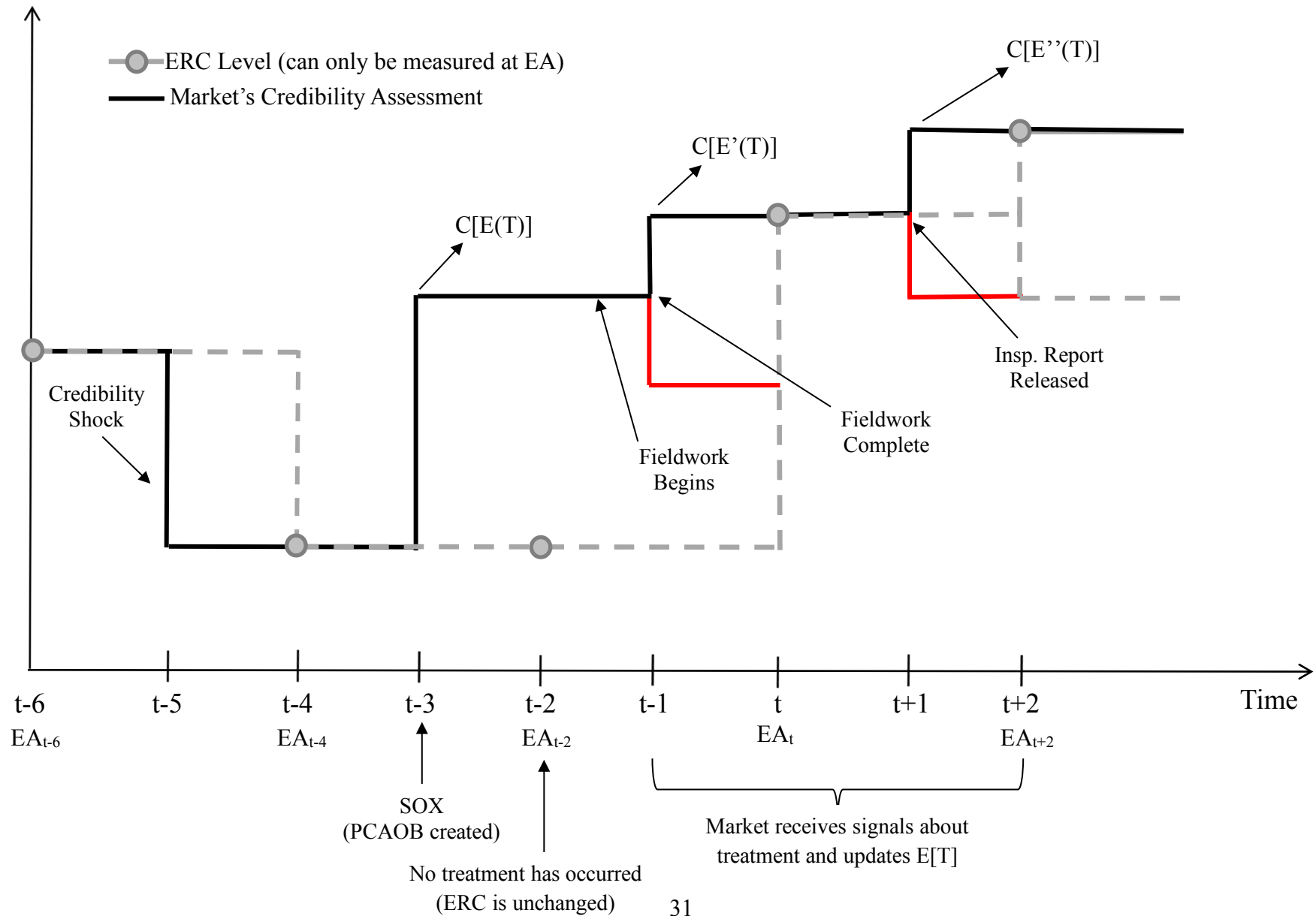
If instead we use the release date of the PCAOB inspection report as an alternative cutoff date (t+1), then there is an even longer period during which the auditor can adjust procedures and investors can learn about these changes. Using the report release as the cutoff date, we define an

issuer as treated when it announces its earnings after the date on which 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 rather provide investors with more general information about audit quality as well as 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 the inspection reports reveal information suggesting 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}). Rather, we estimate long-run changes in (short-window) ERCs relative to the pre-inspection-regime period. 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 that shows the variation for the limited and full inspections. As illustrated in this Figure, the design and PCAOB regime rollout provides substantial variation in the timing of the treatment across firms. For the triennially inspected auditors, we provide specific examples of how we code the *Post* indicator for a variety of fiscal year-end dates and inspection years in Figure IA3. Again, these examples show the variation that triennial inspections provide in this sample.

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Figure IA1 - Stylized Timeline

ERC Level (can only be measured at EA)

Market's Credibility Assessment

C[E''(T)]

C[E'(T)]

C[E(T)]

Credibility Shock

Insp. Report Released

Fieldwork Complete

Fieldwork Begins

t-6   t-5   t-4   t-3   t-2   t-1   t   t+1   t+2   Time

EA_{t-6} EA_{t-4} EA_{t-2} EA_t EA_{t+2}

SOX (PCAOB created)

No treatment has occurred (ERC is unchanged)

Market receives signals about treatment and updates E[T]

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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 regime has 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 changes in the market's assessment of credibility and the earnings response coefficient surrounding 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 state of the expectation about treatment.

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* Note that under the hypothesis that the regime has 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.

Figure IA2 - Breakdown of the *Post* variable for Annually-Inspected Auditor Fieldwork and Inspection Report Release Dates
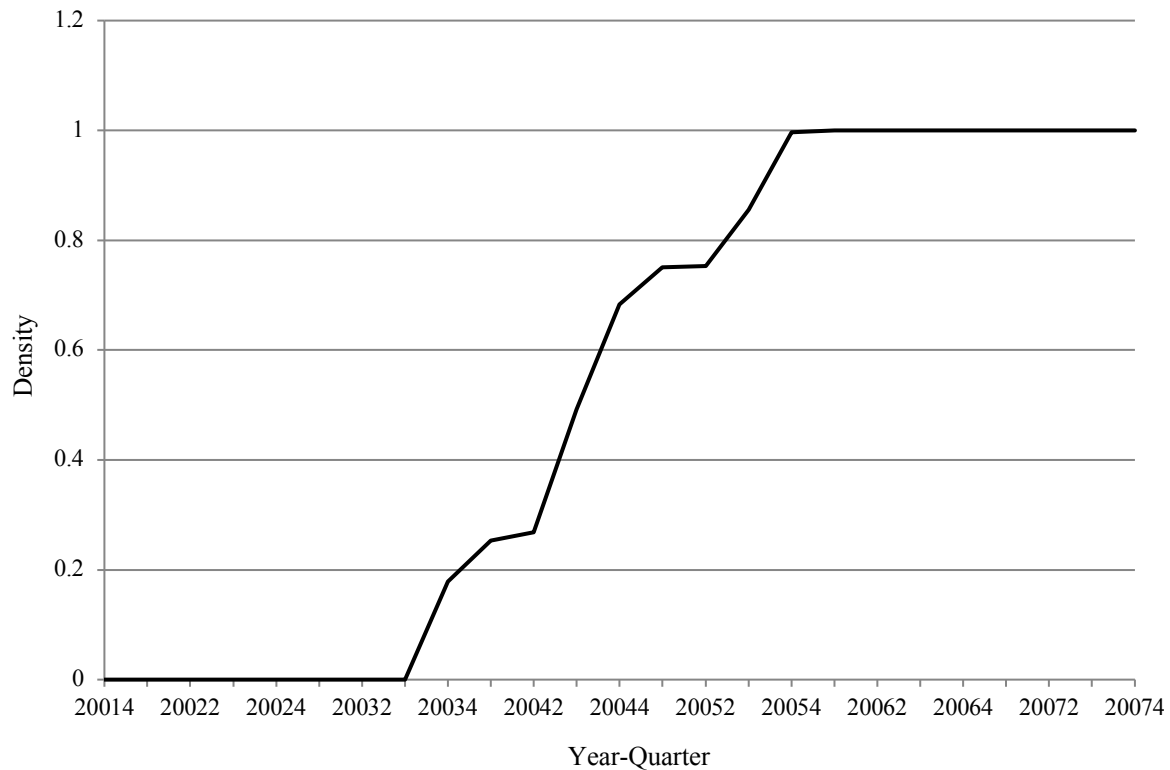

Figure IA2 provides a breakdown of the *Post* variable for annually-inspected auditor fieldwork and inspection report release dates, i.e. using the "combined" sample as described in Section 4.1. The y-axis is defined as the percentage of firms for which *Post*=1 (i.e., the density). The x-axis is defined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

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Figure IA3 - Examples illustrating our coding of the *Post* variable for triennially-inspected auditors

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

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 had similar trends in the absence of treatment, i.e., ERCs would not have evolved similarly in the absence of the PCAOB regime and hence the parallel-trends assumption is violated. Our analysis is conducted in event time using a relatively short pre-period only. Thus, it is difficult to assess pre-period trends. We therefore provide evidence on the historical trends of the ERCs for treatment and control firms for a longer time.

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 a similar fashion, suggesting that the parallel-trends assumption is likely to be valid for our sample.

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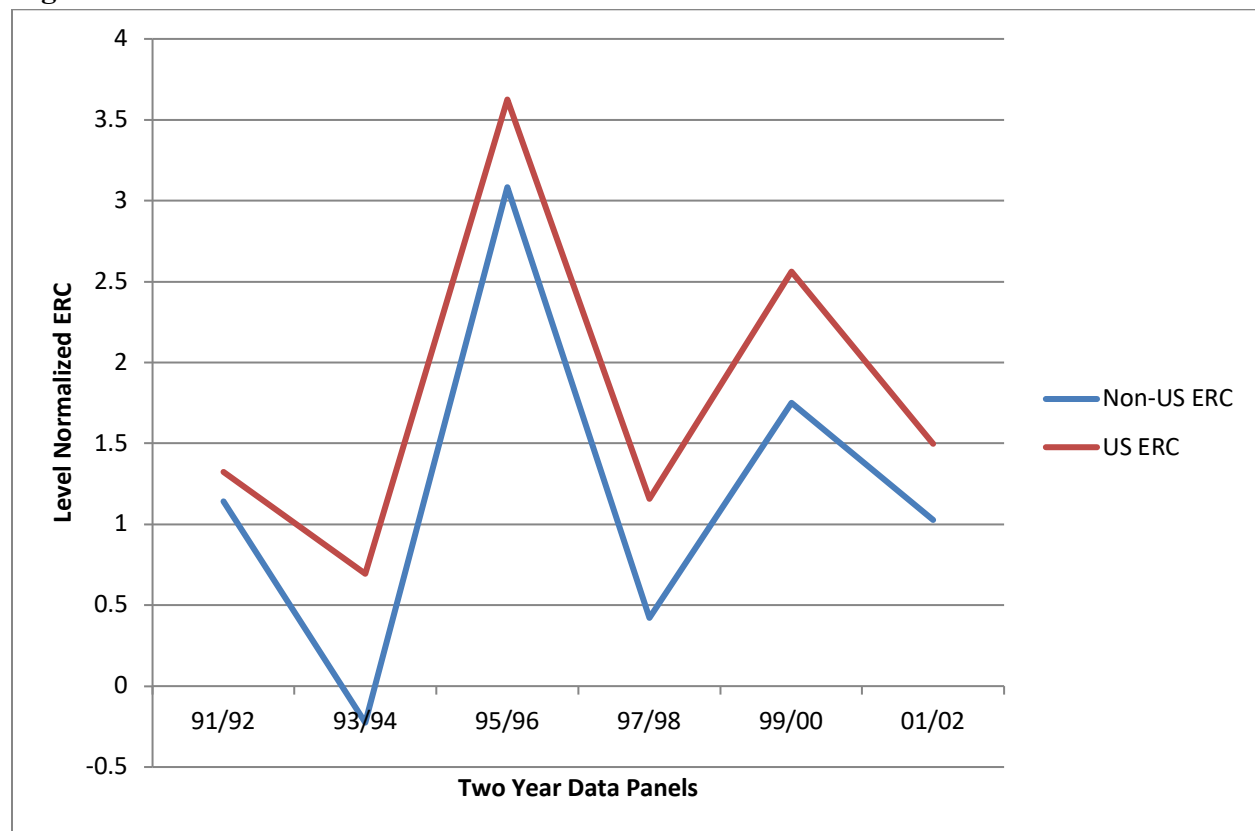
Figure IA4 presents historical trends in ERCs for U.S. firms audited by Big-Four and Tier-Two U.S. auditors and for non-U.S. firms audited by the Big Four and Grant Thornton from 1991-2002, estimating ERCs in two-year intervals in calendar time based on Eq. (1). The sample includes firms used in our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

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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 the location of the auditor. Table IA10 provides a breakdown of the treatment and control samples by the location of the auditor that signs the opinion letter. We consolidate the 19 countries with three or fewer unique firms into the category "Other." By design, all firms in the treatment sample have U.S.-domiciled auditors. Canadian and British auditors audit the most control firms.

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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. We include the count of unique firms with available data for each of the four separate measurement dates (limited inspection fieldwork end, limited inspection report release, full inspection fieldwork end, and full inspection report release) in the combined analysis in which we stack all inspections and measurement periods. We define the exact timing for each of these events in Appendix A, Panel A. The combined analysis therefore includes the same firm up to four times. We include any firm fiscal year-end that is within two years (before or after) of the respective cutoff date. We require that a firm have available data on Audit Analytics, Compustat, CRSP, and I/B/E/S. Here, we provide a breakdown of the treatment and control samples by auditor location. Column (1) reports the number of unique firms by country. Column (2) reports the number of firm-years by country for the combined analysis of all inspections and measurement periods.

Section 6: Dealing with Extreme Values of *UE*

In this Section, we present scatter plots for untrimmed and truncated data across a variety of truncation levels and discuss several additional analyses to assess the sensitivity of our results to truncation. Guided by prior research we include results based on: 1) a standard OLS regression, where UE is truncated at 2.5% and 97.5%; 2) an OLS regression using the percentile rank of UE, where we truncate UE only at 1% and 99%; 3) truncating UE at 1% and 99%; and 4) truncating UE within a specified percentage of price (e.g., 1% or 1.5%).

Figure IA5 provides a scatter plot of Cumulative Abnormal Returns (*CAR*) on Unexpected Earnings (*UE*) for both untrimmed *UE* and a variety of levels of trimming (1%, 2.5%, and 5% in each tail). These figures illustrate the extreme distribution of *UE*, especially for negative realizations. For instance, in the untrimmed scatter plot, some of the most extreme realizations of *UE* are between five and ten times as large as the firm's stock price. As we increase the level of trimming from 1% to 5% the distribution becomes significantly less skewed. In our primary analyses, we choose an intermediate trimming level of 2.5% at the top and the bottom.

Table IA11 presents additional results for our primary analysis (i.e., using a pooled 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 including all control variables, but excluding 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 pooled ERC test with robust regression estimation, the primary regression method from the manuscript. Columns (3)-(6) present the primary tests of our main empirical prediction using two alternative specifications. 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) including 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) including 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.

Figure IA5 - Scatterplot of Cumulative Abnormal Return on Unexpected Earnings

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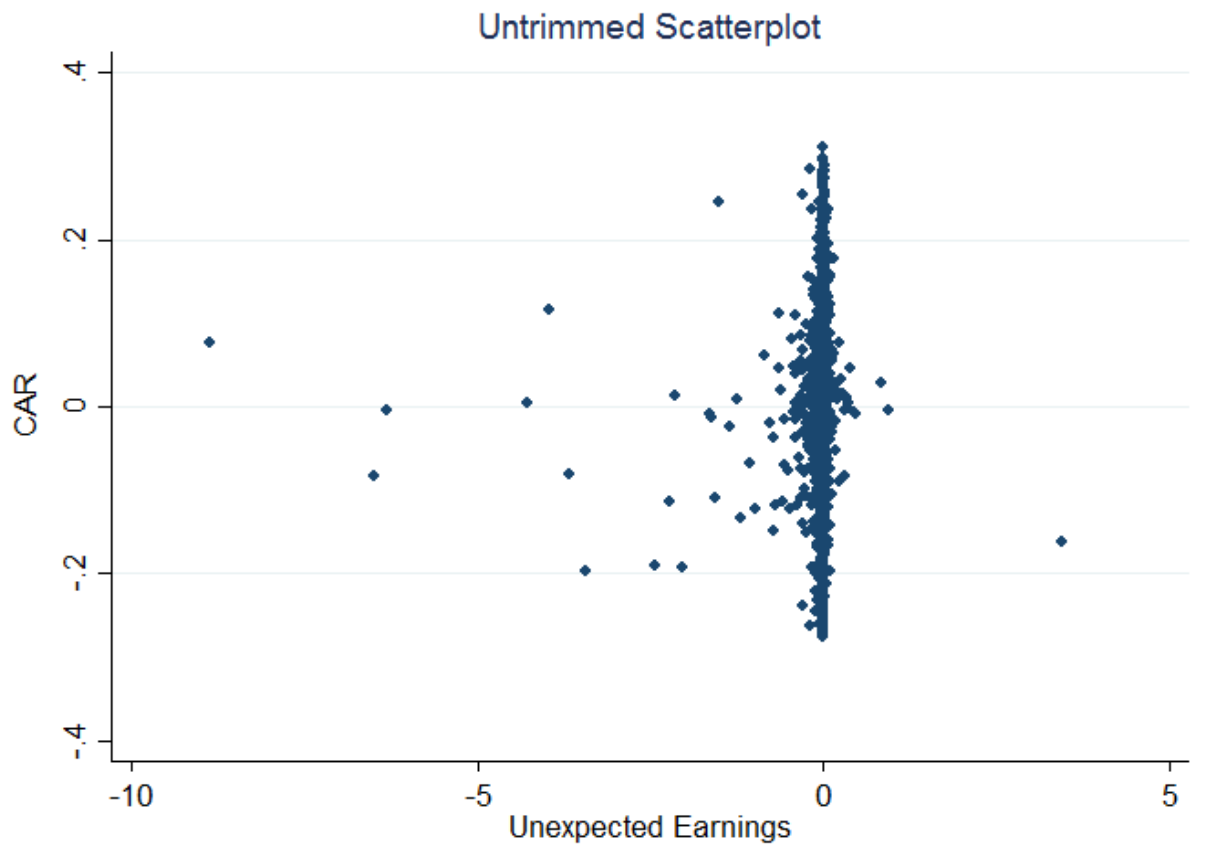

Figure IA5 includes scatterplots across various trimming thresholds for *Unexpected Earnings* (*UE*) along the x-axis and *Cumulative Abnormal Returns* (*CAR*) along the y-axis. Panel A presents a scatterplot of *CAR* on untrimmed *UE*. The sample comprises 42,544 observations from limited inspections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

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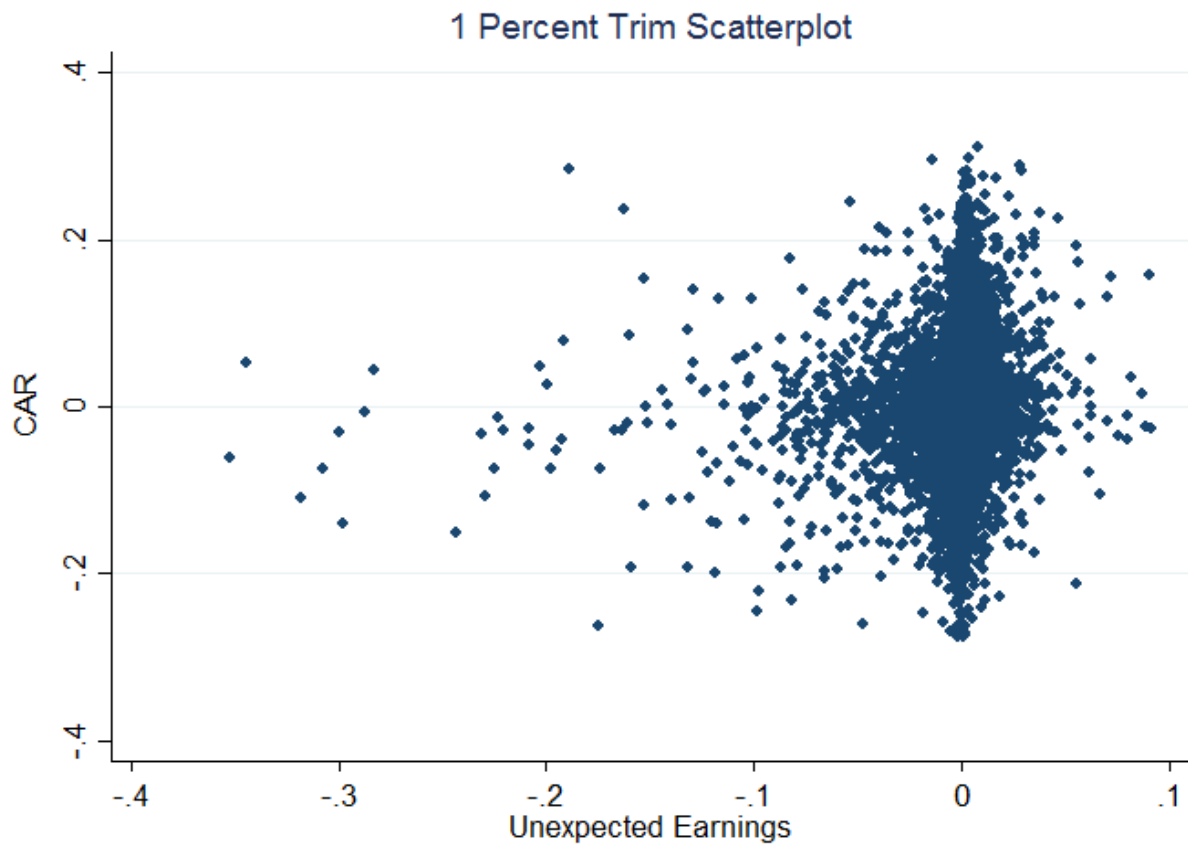
Panel B presents a scatterplot of *CAR* on 1% and 99% trimmed *UE*. The sample comprises 41,882 observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample).

Figure IA5 - Scatterplot of Cumulative Abnormal Return on Unexpected Earnings (continued)

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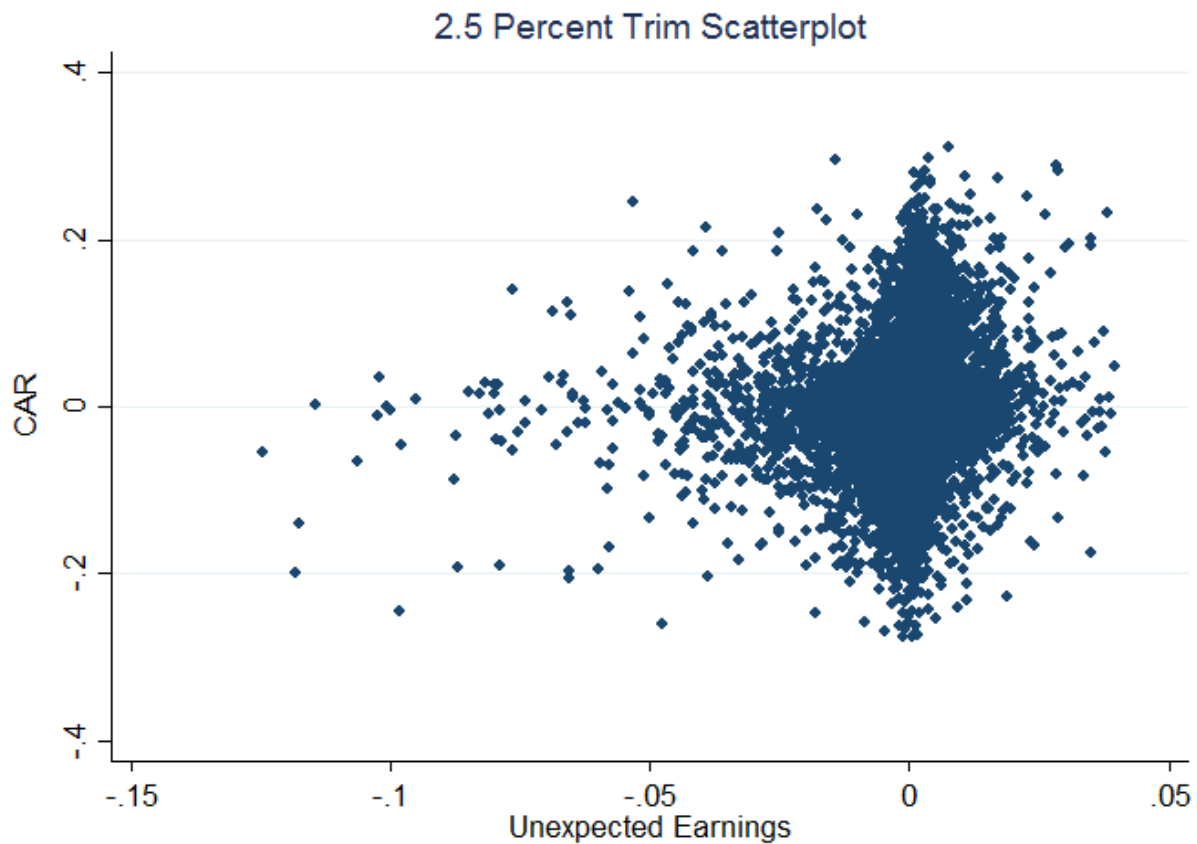
Panel C presents a scatterplot of *CAR* on 2.5% and 97.5% trimmed *UE*. The sample comprises 40,766 observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample).

Figure IA5 - Scatterplot of Cumulative Abnormal Return on Unexpected Earnings (continued)

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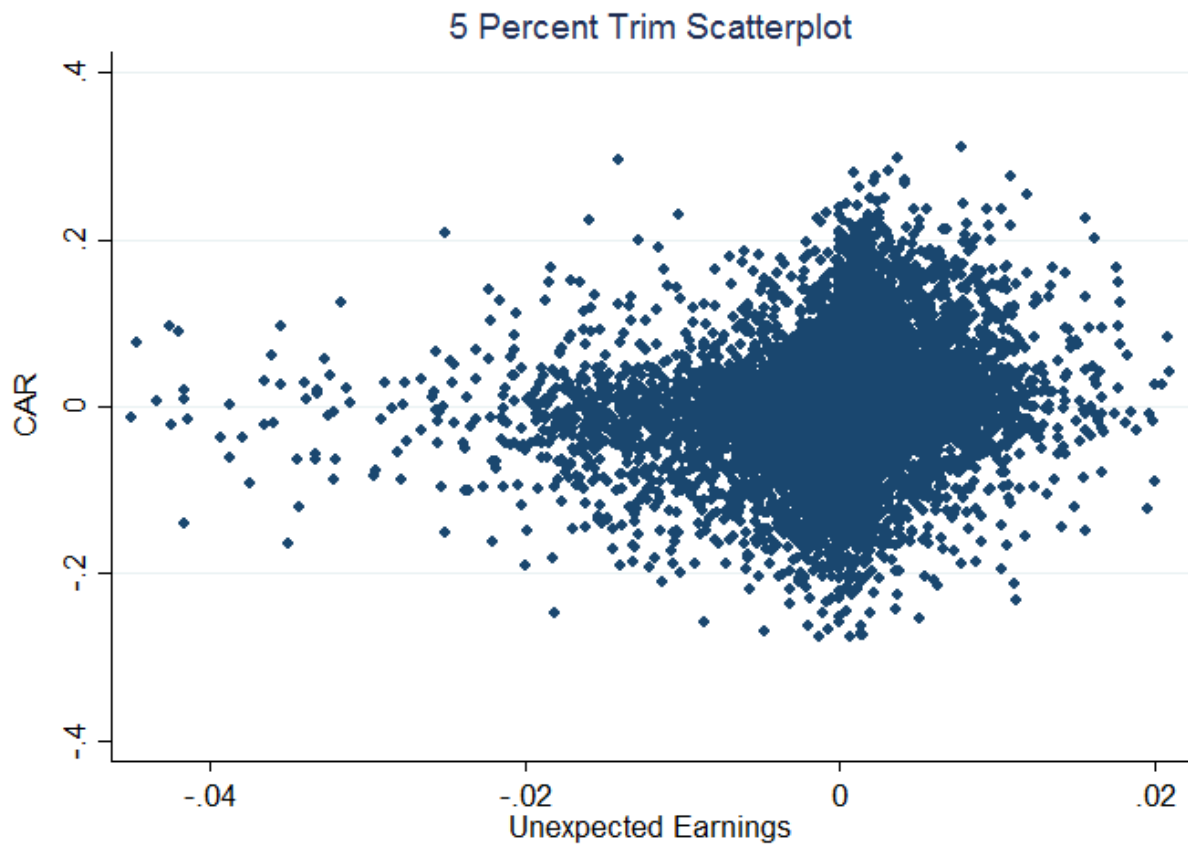
Panel D presents a scatterplot of *CAR* on 5% and 95% trimmed *UE*. The sample comprises 38,766 observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample).

Table IA11 - Baseline Analyses with Control Coefficient Estimates and Non-Robust 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

Notes on next page.

Table IA11 reports an analysis of changes 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 inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). In Columns (3)-(6), we do not report the coefficients for fixed effects, for *UE* interactions with the fixed effects, or for variables that pertain only to the benchmark category (e.g., omitted 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 country of domicile, and the year-quarter of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In Column (1), we use Ordinary Least Squares (OLS) and exclude the PCAOB regime treatment indicators to provide a benchmark ERC regression. In Column (2), we estimate a robust regression similar to Column (1) excluding the PCAOB regime treatment indicators to provide an alternative benchmark ERC regression using the primary regression strategy from the manuscript. In Column (3) and (4), we introduce the regime change indicators and use OLS estimation; Column (4) includes the fixed effects from the manuscript. In Column (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 these Columns, the sample size is larger because we truncate *UE* only at the 1% and 99% levels (like all other variables) prior to applying percentile rank. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Section 7: Tabulation of Additional Coefficient Estimates for Control Variables

In this Section, we present a full tabulation of all coefficient estimates, excluding fixed effects and fixed effects interacted with *UE*. Table IA12 reports this additional output from models tabulated in the Manuscript in Table 4. Importantly, we caution that many of the main effects are challenging to interpret given 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, say Argentina), which is not particularly meaningful and, by itself, the coefficient on *UE* is also not sufficient to infer the total effect.

Similar interpretation issues arise for other variables and their interactions (e.g., *Treated* and *Post*) given we have country and year-quarter fixed effects, and so 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 country ERC estimates for other countries (i.e., not Argentina) are incremental estimates relative to this omitted country and hence by themselves not particularly meaningful.

Similarly, *UE × Post* measures the change in the ERC for the control group during the periods of time after PCAOB inspections that are not collinear with the year-quarter interacted effects. So, 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). Thus, because of the collinearity between the main effect and the fixed effects, the point estimate on *UE × Post* is again not particularly meaningful.

One might naturally wonder why the *UE × Post × Treated* is not the average change in the ERC for U.S. firms relative to Argentinian firms in the post period. The reason is that we have neither interacted *Post* with all of the country effects nor interacted *Post* with the country effects × *UE*. Had we done so, this would allow each country to be separately "tested" for post-PCAOB changes in ERCs against Argentina. The same applies for the year-quarter effects and interactions: why doesn't the variable of interest only measure the effects for the "non-collinear" periods in the post-PCAOB period? The reasoning is similar, the year-quarter effects and interactions with *UE* have not been interacted with *Treated*. So, the variable of interest measures the average, incremental change in ERCs for U.S. audited firms after PCAOB inspections.

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

Notes on next page.

Table IA12 reports full model estimates for our analysis using the "primary design" as described in Appendix A Panel B/C. Following Eq. (1), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). For brevity, we do not report coefficients for the 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 country of domicile, and of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Column (1), we examine the combined analysis, using 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. In Column (2), is the same as Column (1) except we combine the robust regression weights with weights from a coarsened exact matching procedure using 20 bins for control variables *Size* and *Beta*; unmatched bins result in 923 fewer observations. All t-statistics, included 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.

Section 8: Considering and Plotting Nonlinearities

In this Section, we consider nonlinearities in the ERC relation driven by limited market responses to extreme values of *UE*. Freeman and Tse (1992) demonstrate that, because extreme realizations of unexpected earnings are likely to be 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 the same reason, any credibility effect for extreme values of *UE* is likely to be smaller. Moreover, changes in the fraction of extreme and non-extreme values of *UE* across time, i.e., from the pre- to the post-period, could bias the estimated treatment effect. Aside from macroeconomic shocks and the business cycle, such changes could also arise in our setting if auditors are more likely to force firms to recognize impairments in the post-inspection regime. While the latter could be the result of stricter audit oversight, it would contaminate the ERC analysis, which aims to estimate changes in reporting credibility from the pre- to the post-period for otherwise comparable earnings surprises. We present results that confirm the existence of nonlinearities, including a plot of our estimated ERC function including and excluding the nonlinear term in a simple scatterplot of *CAR* and *UE* as well as a fractional polynomial regression. Using the fractional polynomial regression, we show that the s-shaped ERC relation shifts upwards consistent with a credibility effect. Consistent with this finding, we confirm that our results do not change once 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 fall 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 that includes the nonlinear term fits the data better (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 nonlinear function that best fits the data using fractional powers from -2 to +3.[4] The regression results indicate that a fractional polynomial that includes cubic terms provides the best fit for the relation between *CAR* and *UE*, supporting our *Nonlinear* specification which essentially allows for a preservation of the sign of the variable 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

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

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 reporting credibility of earnings surprises following the introduction of the PCAOB regime. Loss firms are expected to have fundamentally different ERCs due to the lower persistence of losses. Thus, we do not expect loss firms to exhibit the same treatment effect as profit firms. This figure illustrates that loss firms have a fundamentally different ERC shape. In Figure IA5 Panel C, we plot fractional polynomial regressions of cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*) using Eq. (1) but including only non-profitable (loss) firms from the treatment group in the pre-inspection (the dashed line) and the post-inspection (the solid line) periods. As expected, the shape of the function for loss firms is generally flat, consistent with the low persistence of loss observations.

Figure IA8 provides the analogue of Figure IA7 Panel B for triennially inspected auditors. We plot fractional polynomial regressions of cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*) using Eq. (1) including only profitable, triennially-inspected firms in the pre-inspection (the dashed line) and the post-inspection (the solid line) periods. This figure illustrates a significant upward shift (or tilt) in the response to positive *UE*, similar to the plot for annually-inspected firms in Figure IA5 Panel B. The plot shows no increase in the response to negative values of *UE*. But note that the graphs are not benchmarked against control firms as 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 et al., 2014), we model the nonlinear relation using the interaction between *UE* and the absolute value of *UE* and include this variable along with losses interacted with *UE* in Column (1). Consistent with prior research, we find that these interactions are significantly negative. Moreover, the baseline ERC considerably increases, indicating that the low baseline ERC is in part attributable to not accounting for naturally less persistent surprises. We consider the effect of nonlinearity as an additional control (Columns (2)-(3)) and then the effect of nonlinearities on the estimated change in ERCs (Columns (4)-(5)). Across all columns, the estimated treatment effect is stronger, indicating that ERCs are nonlinear and that larger responses following treatment do not necessarily confer credibility linearly across the range of *UE* but is likely concentrated in small surprises.

Electronic copy available at: https://ssrn.com/abstract=2641211

Figure IA6 - Scatterplot and Fitted Values Including and Excluding *Nonlinear*
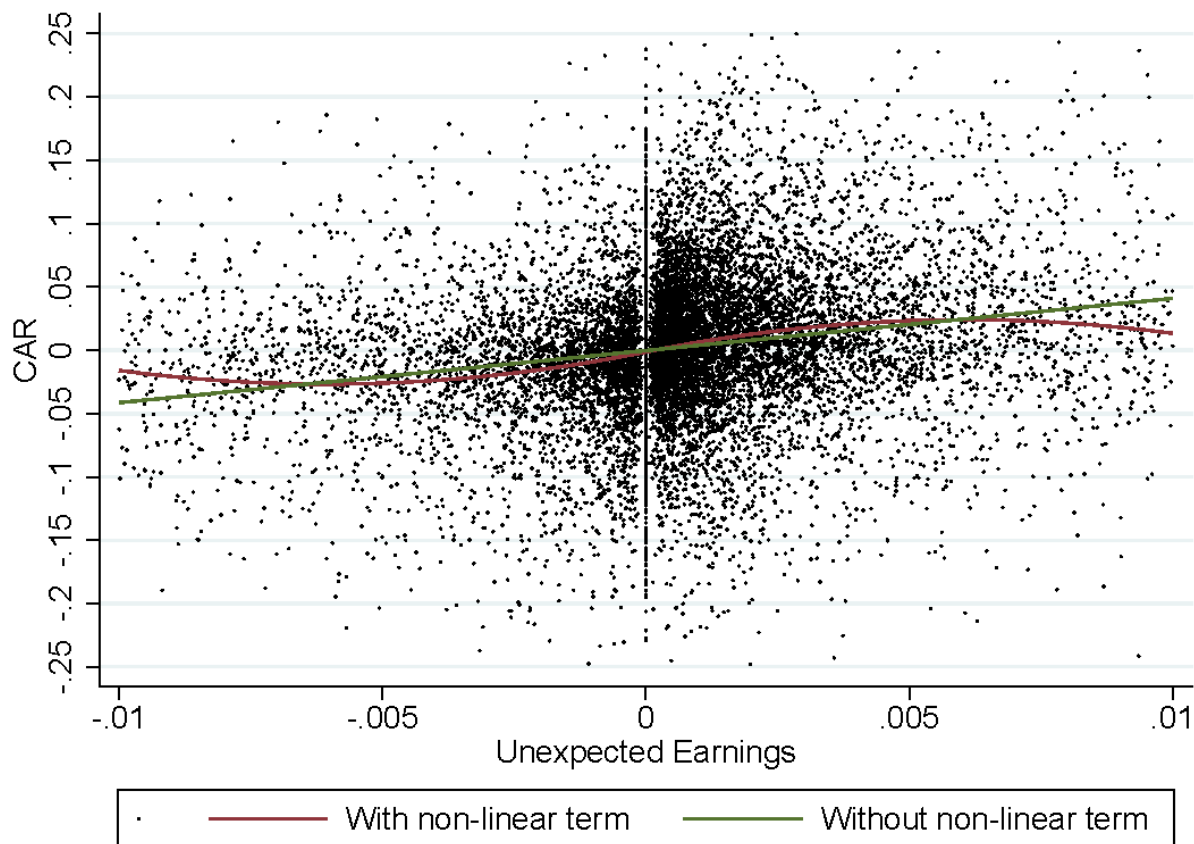

Figure IA6 presents a scatterplot and fitted values for cumulative abnormal returns (*CAR*) and unexpected earnings (*UE*). We plot all firm-year observations from the combined analysis (stacking limited and full inspections and using both alternative cutoff dates) provided unexpected earnings are within ±1% of price. This sample comprises 36,962 observations (or 90.7% of the combined sample). The green fitted-value plot is based on a (linear) regression of *CAR* on *UE*. The red-fitted value plot is based on a regression of *CAR* on *UE* and *Nonlinear*, essentially allowing for a preservation of the sign of the variable similar to using a cubic term. We provide detailed variable definitions in Appendix B of the manuscript.

Figure IA7 - Fractional Polynomial Regression Plots

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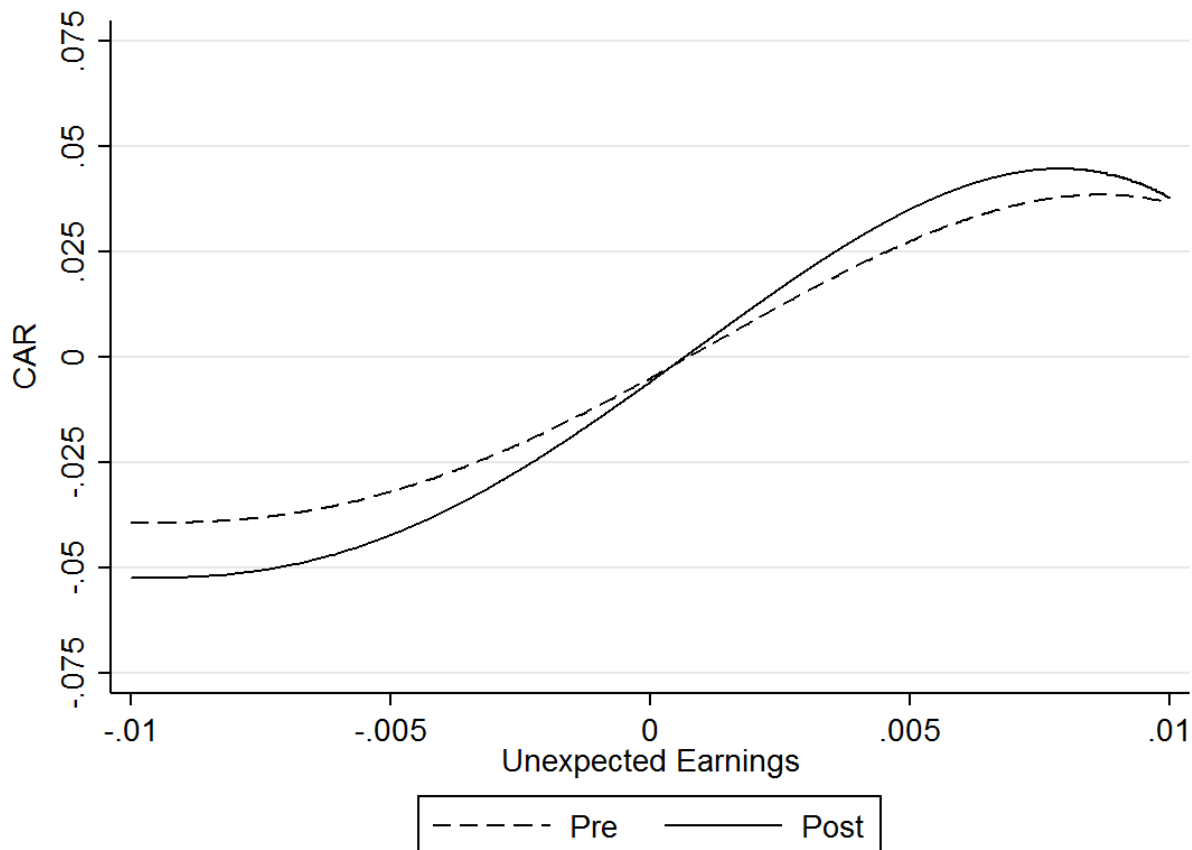
Figure IA7 presents plots of fractional polynomial regressions of cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*) using Eq. (1) of the manuscript. Fractional polynomial regressions provide flexible parameterization for continuous variables without predetermining the shape. The procedure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

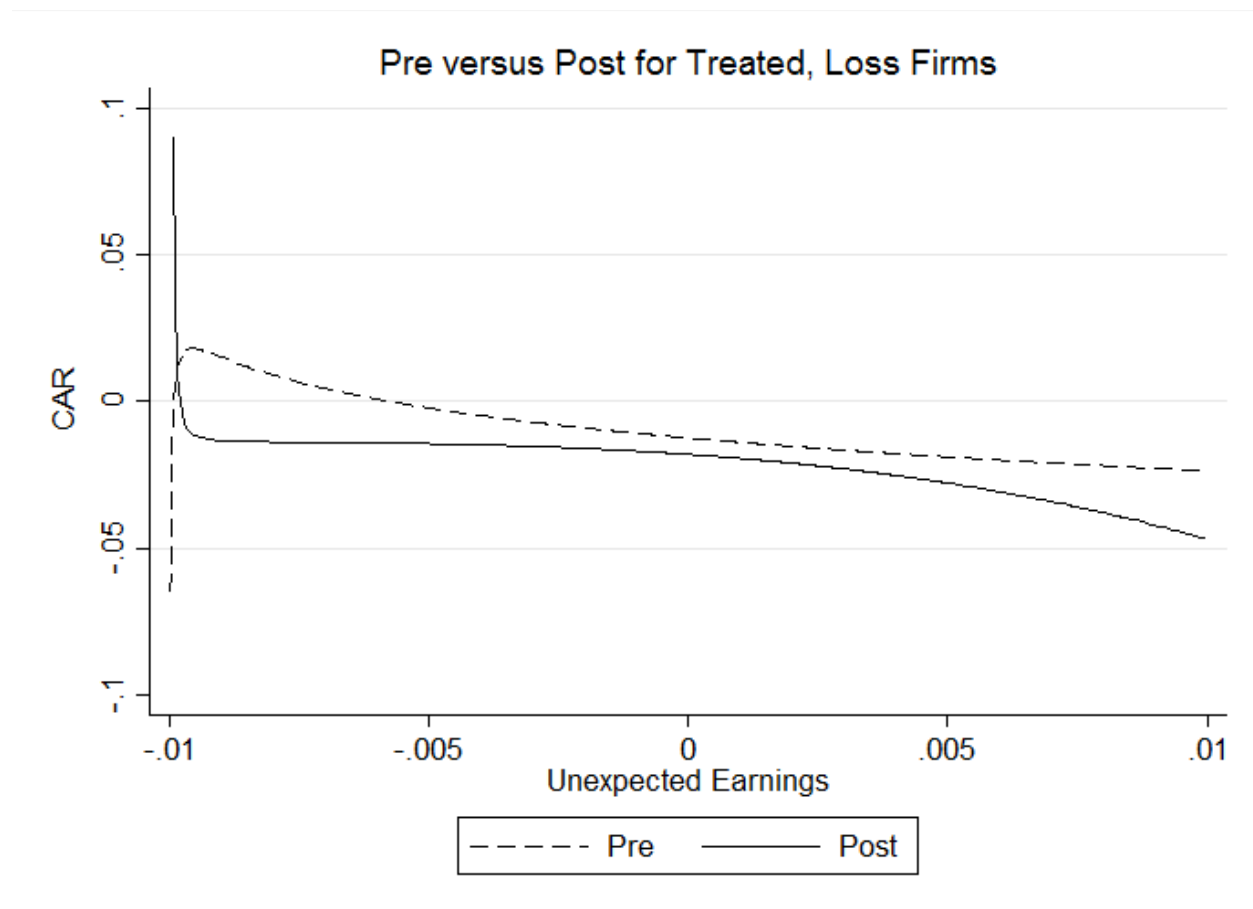**Figure IA7 - Fractional Polynomial Regression Plots** (continued)

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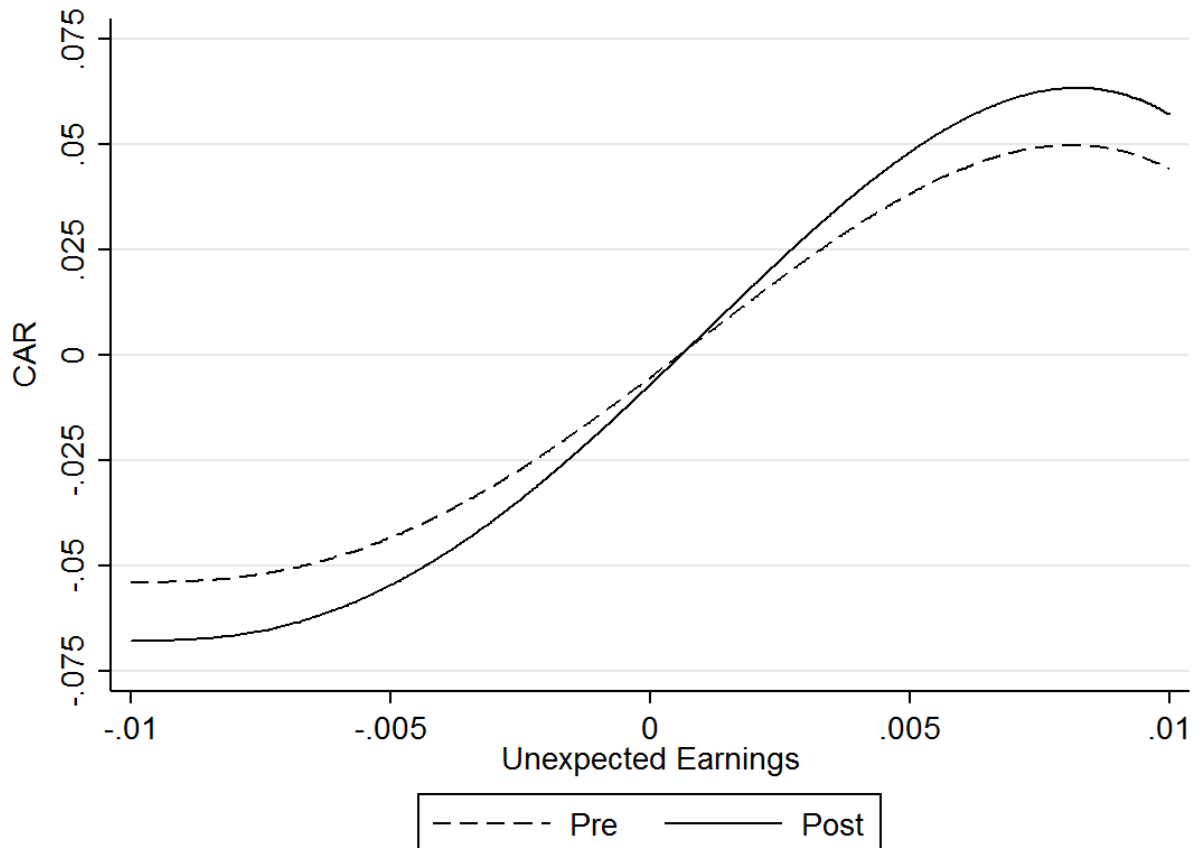
Panel B plots the fractional polynomial using only profitable treatment firms in the pre-inspection (the dashed line) and the post-inspection (the solid line) periods. This sample comprises 28,682 observations (or 97.4% of the combined treatment sample of profitable firms).

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 the 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 (continued)

Pre versus Post for Profitable Firms

Figure IA8 comprises only treated profitable firms (i.e., *Loss* = 0) from the triennial inspection analyses, provided unexpected earnings are within ±1% of price. The pre-inspection period is represented by the dashed line and the post-inspection period by the solid line. This sample comprises 1,242 observations, or 57.8% of the sample.

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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***	1.006***	2.012***	2.181***
		(4.142)	(3.865)	(4.724)	(5.099)
UE	2.304***				
	(9.245)				
Nonlinear: UE×|UE|	-24.463***	-29.302***	-30.169***		
	(-11.860)	(-10.725)	(-10.884)		
UE×Loss×Post×Treated			-0.490		-0.896
			(-0.901)		(-1.627)
Nonlinear×Post×Treated				-78.975***	-73.317***
				(-4.554)	(-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 next page.

Table IA13 reports results for the combined analysis allowing for nonlinearities in the estimation of the ERC. We add *Nonlinear* (i.e., $UE \times |UE|$) and its interactions to Eq. (1), which amounts to introducing a sign-preserving squared term. We regress *CAR* on *UE*, indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). We report only the coefficients of interest. Controls include *Nonlinear, Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Detailed variable definitions are in Appendix B. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and the year-quarter of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In all columns, we estimate robust regressions. In Column (1), we exclude the regime change indicators to provide a benchmark ERC regression including *Nonlinear*. In Column (2), we report the equivalent specification from the manuscript's Table 3 Panel A(1), except including control variable *Nonlinear*. In Column (3), we report the equivalent specification from the manuscript's Table 3 Panel B(1), except including control variable *Nonlinear*. In Column (4), we interact *Nonlinear* with the treatment indicators to allow nonlinearities to change as a result of the new regime. In Column (5), we include interactions between the treatment indicators and *Loss* and *UE×Loss* as well as *Nonlinear*. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors as described in Panels A and B of this table.

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Section 9: Including Additional Fixed Effects

In this Section, we present results for two additional analyses that confirm that our results are robust to the inclusion of fixed effects for the Fama and French 12 industries and "pseudo-firm fixed effects," which are based on firm-characteristic and industry groupings. This analysis is supplement to the residual approach in the Manuscript at Table 4.

Table IA14 includes analyses with various incremental fixed effects. We conduct an analysis including industry fixed effects (interacted with *UE*) as well as industry×time fixed effects (interacted with *UE*). The robustness of our results to the inclusion of these fixed effects should help to mitigate concerns about differences in industry or industry trends in our sample. We have also conducted an analysis including "pseudo firm fixed effects." Rather than introducing an indicator for each firm (for which we run into degrees of freedom constraints from interacting all fixed effects with *UE*), we first group firms into 100 portfolios based on firm characteristics (i.e., the ERC control variables *Size* and *Beta*). We then include indicators for each of the 100 firm groups and their interactions with *UE* as pseudo firm fixed effects. Results including these additional fixed effects are very similar to those in the paper. We conduct this additional analysis for both the annual (Panel A) and triennial inspections (Panel B).

Table IA14 Panel A - Main Test with Additional Fixed Effects (Industry and Pseudo Firm)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: *CAR*						
UE×Post×Treated	0.621***	0.614**	0.600**	0.732**	0.741***	0.784***
	(2.647)	(2.212)	(2.444)	(2.470)	(3.160)	(2.959)
UE×Loss×Post×Treated		-0.371		-0.633		-0.136
		(-0.708)		(-1.215)		(-0.249)
Firm Characteristics	Yes	Yes	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	Auditor & Country & YQ & Pseudo Firm	Auditor & Country & YQ & Pseudo Firm
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	No	Yes	No	Yes	No	Yes
Observations	40,285	40,285	40,766	40,766	40,766	40,766

Table IA14 presents analyses comparable to tests from the manuscript 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 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). 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 country of domicile, and the year-quarter of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Columns (1) and (2), we additionally include fixed effects to indicate the firm's Fama French 48 industry classification. In Columns (3) and (4), we additionally include fixed effects to indicate the firm's Fama French 12 industry classification interacted with the firm's year-quarter of the respective fiscal year end. In Column (5) and (6), we additionally include pseudo firm fixed effects; these are generated by forming 100 portfolios (10 by 10) using controls *Size* and *Beta* from the first year that a firm enters the sample. Columns (2), (4), and (6) replicate Columns (1), (3), and (5) respectively but allows for *Loss* and *UE×Loss* to vary with the introduction of the PCAOB. In all columns, the incremental fixed effects are also interacted with *UE*. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

Table IA14 Additional Effects (continued)

Panel B - Triennially Inspected Auditors with Additional Fixed Effects (Industry and Pseudo Firm)

	(1)	(2)
Dependent Variable: *CAR*		
UE×Post	1.048**	2.064***
	(2.494)	(4.413)
Firm Characteristics	Yes	Yes
Fixed Effects	Auditor & Country & Year-Quarter & FF48 Industry	Auditor & Country & Year-Quarter & Pseudo Firm
Treatment Indicators	Yes	Yes
*UE×*Firm Characteristics	Yes	Yes
*UE×*Fixed Effects	Yes	Yes
*UE×*Treatment Indicators	Yes	Yes
Loss & *UE×Loss* interacted with Treatment Indicators	Yes	Yes
Observations	1,338	1,338

Table IA14 presents analyses comparable to tests from the manuscript with expanded fixed effect specifications. In Panel B, we report results for the triennially inspected auditor sample using the report release. Following Eq. (2), we regress cumulative abnormal returns (CAR) on UE, an indicator for PCAOB inspection (i.e., Post), control variables, fixed effects, the interactions of UE with the control variables, the fixed effects, and the treatment indicator (as shown in the table footer). Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. We provide detailed variable definitions in Appendix B. We include at least fixed effects for the auditor and the year-quarter of the respective fiscal year end, plus interactions of these fixed effects with *UE*. In both columns, we estimate a robust regression. In Column (1), we additionally include fixed effects to indicate the firm's Fama French 48 industry classification. In Column (2), we additionally include pseudo firm fixed effects; these are generated by forming 25 portfolios (5 by 5) using controls *Size* and *Beta* from the first year that a firm enters the sample. In both columns, the incremental fixed effects are also interacted with *UE*. We cluster all t-statistics, included in parentheses, at the firm level. *, **, and *** indicate significance (two-sided) at the 10%, 5%, and 1% levels, respectively. For all robust regressions, we calculate firm-level-clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression.

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

In this Section, we discuss the estimation of a baseline pre-treatment period ERC, which we use as a benchmark against which to assess the economic magnitude of our estimated treatment effects in our primary analyses (calculated without fixed effects for the treated firms in the pre-Enron period). This should be a reasonable benchmark for the overall effect. We discuss the return response to an earnings surprise of 1% for these firms in the pre-PCAOB-inspection period relative to the same size effect in the post period (see pg. 27 of the paper). This pre-Enron benchmark model and our calculations document are Table IA15.

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Table IA15 - 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 IA15 presents measures to assess economic magnitudes. Panel A provides estimates of a baseline Earnings Response Coefficient (ERC) regression specification that follows a modified Eq. (1) without fixed effects or indicators for PCAOB inspections. We use a firm-year sample that extends for five years ending in November 2001, i.e., prior to the Enron and Arthur Andersen events. We limit the sample to cases where unexpected earnings is less than 1% of price. This causes the size of the sample to decline by 15.2%. With this 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, included 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.

Table IA15 - 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 by 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 that we perform. In Column (2), we give the magnitudes of interest.

Section 11: Descriptive Statistics for Abnormal Trading Volume Sample

Table IA16 presents descriptive statistics for the analysis of changes in abnormal trading volume around 10-K filings after the introduction of the PCAOB regime (Manuscript Table 8). The sample size is much larger than for the ERC analysis (68,830) because we do not require analyst coverage (which we need in 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. At the median, a firm files its 10-K 83 days after the fiscal year-end and 36 days after the earnings announcement. Four analysts follow the median firm. Across the other control variables, the sample is comparable to the main ERC tests.

Table IA16 - 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 IA16 presents descriptive statistics for the variables used in the abnormal trading volume analysis (i.e., Table 8 in the manuscript). We provide detailed variable definitions in Appendix B of the manuscript. We include observations from limited inspections and full inspections for annually-inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample), so the same firm enter multiple times (see Table 1 in the manuscript). We truncate all continuous variables at 1% and 99% by fiscal year. The sample comprises 68,830 firm-year observations from the treatment (i.e., firms with domestic Big-Four or Tier-Two auditors) and control samples (i.e., U.S. cross-listed firms with non-U.S. Big-Four or non-U.S. Grant Thornton auditors).

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