

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

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This paper studies the impact of public audit oversight on financial reporting credibility. We analyze changes in market responses to earnings news after public audit oversight is introduced, exploiting that the regime onset depends on fiscal year-ends, auditors, and the rollout of auditor inspections. We find that investors respond more strongly to earnings news following public audit oversight. Corroborating these findings, we find an increase in volume responses to 10-K filings after the new regime. Our results show that public audit oversight can enhance reporting credibility and that this credibility is priced in capital markets. (*JEL* G14, G38, K22, M41, M42, M48)

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Introduction

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

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

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

¹ See, for example, [Wall Street Journal \(2010\)](#) and [Hilzenrath \(2010\)](#). Consistent with these concerns, [Hilzenrath \(2010\)](#) states that "the [PCAOB] looks a lot like the system it was designed to replace: slow to act, veiled in secrecy and weak—or weak willed," and [Glover, Prawitt, and Taylor \(2009\)](#) characterize the PCAOB's inspection model as "inefficient and dysfunctional."

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

Our identification strategy exploits the staggered rollout of the PCAOB regime, which affects firms at different points in time depending on their auditors, fiscal year-ends, and the timing of the new auditor inspections. The PCAOB regime was rolled out in three phases: (1) one-time limited-scope inspections for the U.S. Big Four auditors (Deloitte & Touche, Ernst & Young, KPMG, and PricewaterhouseCoopers) in 2003; (2) annual full inspections (starting in 2004) for all auditors with more than 100 issuers (hereafter, "large auditors"); and (3) triennial, full inspections (starting in 2004) for auditors headquartered in the United States that issued a report for one to no more than 100 issuers (hereafter, "small auditors"). Because the first two phases are clustered in time, we use firms with non-U.S. auditors as a control group. For the third phase, we exploit the staggering of the 3-year rollout and use other triennially inspected auditors as a control group. For all phases, we use a generalized difference-in-differences design that compares earnings response coefficients (ERCs) before and after the respective inspections have taken place and firms' auditors have been treated.

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

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

prior literature and use ERCs to study the effects of public audit oversight on financial reporting credibility.³

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

In our empirical analysis, we first examine changes in reporting credibility for U.S. firms with large auditors after limited and full inspections, relative to non-U.S. firms traded on U.S. exchanges (i.e., cross-listed firms) with large non-U.S. auditors. These cross-listed firms are subject to U.S. market events and other aspects of the U.S. regulatory regime, but their non-U.S. auditors are outside the scope of the PCAOB's initial inspections. Consistent with public oversight increasing investors' assessments of reporting credibility, we find that the ERCs of firms with auditors that are subject to the new PCAOB regime increase significantly compared to the ERCs of the control group. The effect becomes statistically significant after the PCAOB releases the reports from its 2003 limited inspections and strengthens even more after the PCAOB conducts the full inspections in 2004. We also find that the ERC effects are concentrated in profitable firms, which is expected based on prior work showing that loss firms have minimal ERCs because of the transitory nature of losses (e.g., Hayn 1995). The estimated credibility effects are economically meaningful, suggesting an almost 20% increase in ERCs. They are also comparable in magnitude to ERC declines around major credibility shocks.

Our key empirical challenge is to isolate the effects of the PCAOB regime from other events, including (a) any other contemporaneous macroeconomic or capital market changes, (b) other SOX provisions unrelated to audit oversight, and (c) market responses to the accounting scandals. For instance, regulatory changes for firms' internal controls (stipulated by SOX Section 404[b]) could improve reporting credibility independent of public audit oversight. Similarly, after the Enron scandal, investors likely expected firms (especially former

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

Arthur Andersen clients) to provide more assurance about their financial reporting, even in the absence of a regulatory response (e.g., [Leuz and Schrand 2009](#)). We perform a number of additional analyses to address this challenge and corroborate our inferences. First, we provide evidence that concurrent changes in firms' information environments are unlikely to drive our results.⁴ Second, we show that the increase in ERCs after the regime change is *not* concentrated in former Arthur Andersen clients, where firm responses should be most pronounced. Third, our results are even stronger for firms that were exempt from Section 404[b] compliance, and are also robust to separately controlling for the onset of SOX Sections 302[a] and 404[b].

Our second set of analyses focuses on changes in reporting credibility for firms with small auditors, for which the new regime was phased in over 3 years. This staggered implementation allows us to estimate ERC changes within small auditors using only the variation in the timing of initial treatment, eliminating the need to use cross-listed foreign firms as a control group. Importantly, for small auditors, there is very little overlap between the introduction of PCAOB inspections and the effective dates of other SOX provisions. Thus, this setting allows us to more cleanly disentangle the impact of the PCAOB regime from other SOX provisions as well as other concurrent events. Similar to our findings for large auditors, our results indicate a significant increase in ERCs as the new regime is rolled out; again, the effects are concentrated in profit firms.

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

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

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

major shock like the accounting scandals of the early 2000s. As such, our study also adds to the literature on the merits of private versus public enforcement of regulation (La Porta, Lopez-De-Silanes, and Shleifer 2006; Jackson and Roe 2009), which generally focuses on the activities of securities regulators. For example, Duro, Heese, and Ormazabal (2018) examine the effects of disclosing SEC oversight activities on investor responses to earnings news. Prior auditing studies have focused on litigation and reputation as mechanisms that discipline or incentivize auditors (DeFond 2010, 2012). In contrast, we provide evidence on auditors and on the public regulatory oversight of auditing.

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

A few studies examine the PCAOB regime change overall. Specifically, our paper complements Shroff (2019) and Aobdia and Shroff (2017), who focus on real and audit market effects. Perhaps closest to our paper is that of Krishnan, Krishnan, and Song (2017), who examine the effects of first-time PCAOB inspections of foreign auditors of U.S. cross-listed firms on the value relevance

of earnings and book value.⁵ They find that the value relevance of accounting numbers increases in the post-inspection period for clients of inspected auditors. However, this effect is estimated relative to other clients of the *same* auditor and hence exists only for inspected-client engagements, and not the audit firm. Our analysis provides evidence of an increase in reporting credibility for all U.S. firms after their auditors are subject to the PCAOB regime.

1. Empirical Approach, Institutional Setting, and Presumed Mechanism

Our empirical analysis connects key dates for the rollout of the PCAOB oversight regime to subsequent changes in the market's assessment of reporting credibility for U.S. firms overall. We deliberately take the approach of studying marketwide shifts in investors' assessments of reporting credibility. An alternative approach would be to examine specific audit process outcomes (e.g., inspection findings, audit hours, audit opinions). While studying such outcomes is clearly important, these outcomes do not indicate whether public audit oversight enhances reporting credibility in capital markets. It is conceivable that auditors spend more time on documentation in the new regime but that investors do not value this increase in audit hours. For this reason, we do not focus on inspection reports or specific audit outcomes, but instead examine investor responses to earnings news in capital markets.

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

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

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

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

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

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

To corroborate the mechanism’s second element, we provide descriptive evidence that (a) PCAOB inspections identify meaningful weaknesses and deficiencies in auditing procedures (see IA§1 Part 3) and (b) that these findings lead to subsequent changes in auditing and financial reporting that last beyond the respective engagement (see IA§1 Part 4). Conceptually, lasting improvements in audit procedures beyond a single engagement are critical for the market to increase its overall assessment of credibility, as the mere revelation of previously unidentified deficiencies will likely lower investors’ credibility assessments. Although stricter oversight could initially reveal more audit failures (to which investors presumably respond negatively), a stricter regime should ultimately enhance credibility if it leads to broader improvements in audit quality that spillover to other engagements and auditors.⁷ Toward this end, the PCAOB regime not only identifies deficiencies but also requires *subsequent* changes in audit procedures (known as “remediation”). If, during

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

their fieldwork, inspectors identify potential deficiencies in one or multiple engagements, the PCAOB gives the auditor the opportunity to respond. If the response is not satisfactory, the deficiency is included in the inspection report as a “Part I finding.” While the inspection report does not reveal which engagements were inspected or had Part I findings, auditors are *required* under PCAOB rules to remediate Part I findings, both contemporaneously, by performing additional audit work to validate the issued audit opinion, and prospectively, in future audit engagements. Consistent with this, [DeFond and Lennox \(2017\)](#) provide large sample evidence that PCAOB inspections improve internal control audits.

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

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

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

2. Empirical Measures, Research Design, and Sample Selection

2.1 Defining and measuring reporting credibility

Theory suggests that credibility strengthens the response of investors to signals. More specifically, investors should respond more strongly to a given level of earnings surprise (relative to expectations) if they believe that reported earnings accurately reflect economic performance. [Holthausen and Verrecchia \(1988\)](#) formalize this prediction using a noisy-rational expectations model

with two consecutive information releases: an analyst forecast and an earnings announcement. Under fairly general conditions, they show that the variance of the price reaction to the earnings announcement (i.e., the second release) is unambiguously nondecreasing in the signal-to-noise ratio of the earnings surprise (see their proposition 1). An increase in credibility is tantamount to an increase in the signal-to-noise ratio and, more specifically, to an increase in investors' assessments of the precision of the earnings news (see also [Kim and Verrecchia 1991](#)). Thus, we use the strength (or sensitivity) of short-term market responses to earnings surprises as a proxy for credibility. This construct is commonly referred to as the earnings response coefficient (ERC) and is estimated by regressing abnormal returns at the earnings announcement on the respective earnings news (see Section 3.1 for notation and more details).

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

2.2 Research design and earnings response coefficients

From a research design perspective, ERCs are also well suited for our identification strategy, which exploits the staggered introduction of the PCAOB regime and variation in auditors' and firms' fiscal year-ends.⁹ ERCs should not change in anticipation of future improvements in audit quality and reporting credibility. That is, ERCs reflect the credibility of reported earnings at a specific point in time (i.e., the earnings announcement). This feature is critical when using a staggered design. In contrast, firms' stock returns, market values, or cost of capital would likely reflect the PCAOB regime's impact much earlier, and hence would not allow us to exploit the staggered introduction. In addition, ERCs are measured over short windows around firms' earnings announcements, which are spread out in calendar time and depend on predetermined fiscal year-ends. These are desirable features from an identification perspective.

However, there are also some assumptions and drawbacks. First, ERCs require a measure of expected earnings in order to determine earnings news. We use consensus analyst forecasts, which are known to exhibit biases and

⁸ In IA§2, we provide additional evidence to support our use of ERCs to capture audit-related differences in reporting credibility; this includes an examination of the ERCs and F-scores of Big Four auditor clients and an examination of ERC changes following the PCAOB's enforcement action against Deloitte & Touche in December 2007.

⁹ In IA§3, we present a stylized time line for the introduction of the PCAOB regime and the related changes in reporting credibility and ERCs. This time line provides the conceptual underpinnings for our research design.

imperfectly reflect investors' expectations. Second, ERCs are not directly observable for a given announcement but must be estimated from a sample of announcements. This likely introduces noise and reduces the power of the analyses. We use several approaches to deal with this noise and also consider abnormal volume reactions around the release of firms' 10-Ks as an alternative measure of reporting credibility (see Section 3.4). Third, ERCs change for reasons other than reporting credibility. We directly control for several known ERC determinants (see [Collins and Kothari 1989](#)) and employ a difference-in-differences design that strips out time-invariant biases in the ERC estimation in order to isolate credibility effects.

Nonetheless, we recognize that stricter audit oversight could have other effects beyond changes in reporting credibility that indirectly affect firms' disclosure and reporting. Thus, we acknowledge that our analysis does not capture all the reporting effects of public audit oversight; instead, it focuses on credibility changes as measured by ERCs. We also note that the potential effects of audit oversight on disclosure and reporting could potentially confound an ERC analysis. Given this concern, we gauge the extent to which changes in ERC components and/or disclosure and reporting affect our inferences (see Section 3.2 for details).

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

In June 2003, the PCAOB began limited inspections of U.S. Big Four auditors. The PCAOB conducted fieldwork and released inspection reports at approximately the same time for all limited inspections (see Table A.1 in Appendix A). In 2004, the PCAOB conducted full inspections of large U.S. auditors and the first round of triennial inspections of small U.S. auditors. We examine the effects of the new regime on reporting credibility for each of the three distinct phases of the PCAOB regime introduction (i.e., limited, full, and triennial inspections). For each phase, we use a difference-in-differences analysis. It is unclear *ex ante* when investors adjust their credibility assessment and hence which phase is most relevant. The initial 2003 inspections were limited in scope.¹⁰ Additionally, it takes time for auditors to adjust their audit procedures in meaningful ways and for investors to learn about these changes. Thus, the first full inspections are more likely to be the relevant treatment for large auditors.

¹⁰ Limited inspections involved all components of full inspections, but were scaled down in extent (e.g., a lower number of individual audit engagements inspected), because, at that time, the PCAOB was still in the process of staffing up and building out its inspection regime ([PCAOB 2004b](#)). In the United States, the Big Four voluntarily agreed to participate in the limited inspections as the official PCAOB registration process had not yet begun.

Furthermore, limited and initial full inspections are clustered in time. Therefore, the first set of analyses relies on non-U.S. firms that are cross-listed on U.S. exchanges. This control group has several desirable features as well as potential drawbacks. The first benefit is that cross-listed control firms are audited by *non-U.S.* Big Four and Grant Thornton affiliates that (in 2003 or 2004) are not subject to PCAOB inspections, but are still required to comply with other SOX provisions at the same time as U.S. issuers (with one exception discussed later).¹¹ This feature helps us separate the PCAOB regime and other SOX provisions. Second, these issuers are exposed to the U.S. market conditions and information environment, which makes it more likely that the treatment and control groups would have similar ERC trends in the absence of the PCAOB regime. However, we acknowledge that foreign, cross-listed issuers could be differentially affected by shocks in the United States (e.g., Bailey, Karolyi, and Salva 2006), which could bias our analysis. For this reason, we carefully examine the validity of the parallel trends assumption in our setting (see Section 3.1 and IA§4). Third, foreign cross-listed issuers could experience similar treatments at home if these countries implement audit oversight reforms similar to those in the United States.¹² Moreover, it is possible that non-U.S. auditors change their procedures because the PCAOB inspects their U.S. affiliates. Both spillover effects would lead us to underestimate the impact of the U.S. audit regime. Given these possibilities, it is useful to have a set of analyses that does not rely on foreign control firms.

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

¹¹ In IA§1 Table IA6A, we provide details on the timing of the adoption of other SOX provisions, broken down by U.S. versus foreign firms and accelerated filer status.

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

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

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

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

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

¹⁴ For the Big Four, fieldwork typically lasts between 5 to 7 months. For small auditors, inspections are shorter, so we add 30 days to fieldwork completion when defining the cutoff date to allow small auditors to adjust audit procedures. See Table A.1 and Figures A.1 and A.2 in Appendix A for more details on timing and an illustration of our research design for annually inspected auditors. See the Internet Appendix for an illustration for triennially inspected auditors.

¹⁵ Aobdia (2018) notes that PCAOB inspectors normally share feedback on auditing deficiencies during on-site inspections. For example, the substance of the inspection comment forms, which are a precursor to Part I findings, is shared on site during fieldwork (Riley et al. 2008).

report as an alternative cutoff, there is an even longer period during which the auditor can adjust procedures and investors can learn about these changes. When we use the report release date as the cutoff, we define an issuer as treated when it first announces earnings after the PCAOB posts the inspection report for the firm's auditor online.

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

2.4 Sample selection and composition

We obtain (a) accounting, auditor, and market data from Compustat, (b) additional auditor data from Audit Analytics, (c) analyst forecasts and accounting data from I/B/E/S, (d) market data from CRSP, and (e) fieldwork and inspection dates from the PCAOB's Web site. All data are publicly available. For the analyses of annually inspected auditors, we define a roughly 4-year window around treatment such that we typically have 2 fiscal year-ends *before* and *after* the respective cutoff date. For the limited inspections, using the fieldwork (inspection report) cutoff date, the sample period is from December 2001 to November 2005 (June 2002 to May 2006) and the sample includes firms whose fiscal year-ends that fall into this window. For the limited inspections, we include the full sample of cross-listed control firms because (at that time) there were no formal cooperative agreements between the PCAOB and the home-country regulators of non-U.S. firms to conduct inspections in non-U.S. jurisdictions. For the full inspections of Big Four and Tier-Two auditors, using the fieldwork (inspection-report) cutoff date, the sample period is from June 2002 to December 2006 (July 2003 to November 2007) and the sample includes firms whose fiscal year-ends fall into this window. For the full inspection control sample, we exclude cross-listed firms from countries with an inspection agreement with the PCAOB during or before our analysis window.¹⁶ The control group also includes firms from countries unavailable for inspection.¹⁷

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

¹⁷ <http://pcaobus.org/International/Inspections/Pages/IssuerClientsWithoutAccess.aspx> (accessed January 2015).

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

Table 1
Sample composition

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

	Unique firms				Firm-years	
	Limited inspections		Full inspections		(5)	(6)
	(1)	(2)	(3)	(4)		
Treatment sample	Fieldwork	Reports	Fieldwork	Reports		
<i>Big Four auditors</i>						
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
<i>Tier-two auditors</i>						
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						
<i>Big Four auditors</i>						
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
<i>Tier-Two auditor</i>						
Grant Thornton	—	—	2	2	2	6
Subtotal	481	515	316	279	579	3,765
Total	3,806	3,917	4,319	4,018	4,868	40,766

Table 1 provides details on the sample composition for our limited, full, and triennial inspection analyses. Panel A describes the sample composition for the limited and full inspections by auditor, inspection type, and sample window cutoff date. Columns 1 through 4 report the count of unique firms with available data for each of the four separate sample windows (limited inspection fieldwork end, limited inspection report release, full inspection fieldwork end, and full inspection report release). We provide the dates (month and year) of these four events for annually inspected auditors in Table A.1 in Appendix A. In Column 5, we report the number of unique firms in the combined analysis in which we stack all inspections and sample windows. Thus, the combined analysis includes the same firm up to 4 times. In Column 6, we report the number of firm-years for the combined analysis. We include any firm fiscal year-end that falls into a sample window defined for the respective cutoff date (see Section 2.4 and Figures A.1 and A.2 in Appendix A for details). We require that a firm have available data on Audit Analytics, Compustat, CRSP, and I/B/E/S.

(Continued)

¹⁸ Non-U.S. Grant Thornton affiliates are included in the full inspection control sample. Other Tier-Two auditors are not included, because Audit Analytics does not identify foreign affiliates for these auditors. We do not include Grant Thornton in the control group for the limited inspections in order to provide a clean within-Big-Four comparison.

Table 1
(Continued)*B. Number of newly treated, triennially inspected auditors and firm-years*

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

Panel B provides a sample breakdown of the number of newly treated, triennially inspected auditors and the number of their unique client firms and firm-years. We include all firm-years on Compustat with fiscal years ending between Q2 2001 and Q2 2008 that meet the following requirements: (1) the firm has available data on Audit Analytics, Compustat, CRSP, and I/B/E/S and (2) the auditor has registered with the PCAOB. At the end of the sample period, all but 4 auditors in our sample have been inspected; 32 had not yet had an inspection report released. Column 1 (2) reports the number of newly treated auditors by calendar year, using the inspection fieldwork (report release) as the cutoff date. Column 3 (4) reports the number of unique client firms associated with the newly inspected auditors. In the last row, we report the number of firm-years contributed by these firms.

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

2.5 Descriptive statistics

Panels A–D of Table 2 present descriptive statistics for domestic issuers with large annually inspected auditors (i.e., the treatment group in the limited and full inspection analyses), cross-listed firms with non-U.S. auditors (i.e., the control group in the limited and full inspection analyses), a comparison of means between these subsamples, and issuers with triennially inspected auditors, respectively. The first two variables are the key inputs to estimate the ERC: the *CAR* at the earnings announcement and the earnings surprise or unexpected earnings (*UE*). Our primary control variables are *Loss*, *Size*, *Market-to-book*, *Leverage*, *Persistence*, and *Beta*. The other variables in Table 2 are used in additional sensitivity tests. We also count the number of days between the respective cutoff date for the auditor's initial treatment (i.e., either the end of fieldwork or the report release) and the firm's earnings announcement at which the first post-treatment ERC is measured (*Timing: Treatment to first EA (in days)*). The variable indicates that our design allows for a substantial time lag during which auditors could adjust their procedures and where investors could learn and price the effects of the regime change.

In panel C, we see that the control sample is similar to the treatment sample along most dimensions, including mean *Loss*, *Market-to-book*, *Leverage*, and *Persistence*. However, the two groups differ in terms of *Size* and *Beta*, which is unsurprising given that exchange-traded, cross-listed firms tend to be quite large. Thus, it is important to include these variables as controls (interacted with *UE*). In addition, we run analyses in which we explicitly match firms based on

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

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

Variable	N	Mean	SD	P25	Median	P75
<i>CAR</i>	37,001	0.002	0.064	-0.030	0.001	0.034
<i>UE</i>	37,001	0	0.008	-0.001	0	0.002
<i>Loss</i>	37,001	0.182	0.386	0	0	0
<i>Size</i>	37,001	7.018	1.518	5.948	6.926	7.990
<i>Market-to-book</i>	37,001	2.973	2.639	1.594	2.254	3.487
<i>Leverage</i>	37,001	2.654	3.991	0.506	1.153	2.643
<i>Persistence</i>	37,001	0.282	0.446	0	0.285	0.553
<i>Beta</i>	37,001	1.092	0.548	0.708	1.033	1.436
<i>Forecast</i>	36,659	0.032	0.074	0.025	0.046	0.063
<i>Timeliness</i>	36,596	-0.201	0.157	-0.256	-0.155	-0.093
<i>Relative information</i>	36,586	99.81	0.144	99.74	99.84	99.92
<i>Scaled raw accruals</i>	34,855	-0.055	0.076	-0.084	-0.046	-0.014
<i>Earnings guidance</i>	37,001	0.530	0.499	0	1	1
<i>Guidance bundle</i>	37,001	0.394	0.489	0	0	1
<i>Post</i>	37,001	0.504	0.500	0	1	1
<i>Timing: Treatment to first EA (in days)</i>	12,436	241.0	193.3	88.0	165.0	386.0

Table 2 presents descriptive statistics for the variables used in the limited, full, and triennial inspection analyses. Table B1 in Appendix B defines each variable in detail. We include observations from limited and full inspections for annually inspected auditors using both the end of fieldwork and the inspection report release as cutoff dates (i.e., the combined sample), so the same firm enters multiple times (see Table 1). We truncate all continuous variables (except *UE*) at 1% and 99% by fiscal year. *UE* is truncated at 2.5% and 97.5% by fiscal year. Panel A presents descriptive statistics for firms with U.S. annually inspected Big Four or Tier-Two auditors (i.e., the treatment group in the limited and full inspection analyses). The sample includes 37,001 firm-year observations. The last row in the panel provides the average number of days from the respective cutoff date (end of fieldwork or inspection report release) to the (treated) firm's first earnings announcement.

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

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

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

Table 2
(Continued)

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

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

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

D. Triennially inspected auditors

Variable	N	Mean	SD	P25	Median	P75
CAR	1,338	−0.005	0.070	−0.036	−0.003	0.027
UE	1,338	−0.009	0.033	−0.006	0	0.001
Loss	1,338	0.254	0.436	0	0	1
Size	1,338	4.800	0.890	4.309	4.831	5.405
Market-to-book	1,338	2.862	4.167	1.374	1.890	3.046
Leverage	1,338	4.983	5.133	0.431	2.167	9.576
Persistence	1,338	0.648	0.605	0.156	0.491	1.083
Beta	1,338	0.316	0.587	0	0.328	0.643
Fieldwork timing:						
Post	1,338	0.528	0.499	0	1	1
Timing: Treatment to first EA (in days)	706	543.3	343.8	245.0	505.5	677.0
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	130.0	335.0	581.0
Dropped observation timing:						
Post	1,013	0.392	0.488	0	0	1

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

these two characteristics. The final two columns of Table 2 show that *Size* and *Beta* are no longer statistically significantly different across the treatment and control groups when matched.

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

3. Empirical Results

3.1 Analysis of large, annually inspected auditors

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

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

CAR is the 3-day ($t-1$, $t=0$, and $t+1$) cumulative abnormal return for firm i , centered on the earnings announcement date and market-adjusted by the CRSP value-weighted index. UE is the difference between the actual, annual EPS and the median analyst forecast for annual EPS, both from I/B/E/S. $Treated$ is an indicator that equals one when a firm's auditor is a U.S. Big Four or Tier-Two auditor, and zero otherwise. $Post$ is an indicator that equals one after the treatment by the new regime, and zero otherwise. As discussed in Section 2.3, we use two alternative cutoffs: the fieldwork end date and inspection report release date (see Appendix A for details). For analyses using the fieldwork end date, $Post$ equals one if a firm's fiscal year ends in the same month as the fieldwork or later. For analyses using the inspection report date, $Post$ equals one if a firm's fourth quarter earnings announcement falls on or after the release of the inspection report. While auditors' fieldwork end and inspection report release dates are clustered in time, the $Post$ variable is coded based on clients' fiscal year-ends or earnings-announcement dates, respectively, which provides more variation. In Equation (1), our primary coefficient of interest is β_7 , which measures the incremental change in the ERC for firms whose auditors have been treated by the PCAOB regime. A positive coefficient indicates an increased response to earnings news under the new regime, which we interpret as an increase in reporting credibility.

We include controls for a variety of firm characteristics shown by prior research to be important determinants of a firm's ERC. First, we include *Loss*, an indicator variable that equals one if a firm reports negative earnings, and zero otherwise; we also include $UE \times Loss$. As losses are less persistent than profits, the response to negative earnings is likely lower than the response to positive earnings (Hayn 1995). Second, we include *Size*, *Market-to-book*, *Leverage*, *Persistence*, and *Beta* as well as the interaction of these variables with UE , given that prior work shows that ERCs are a function of the riskiness, growth, and persistence in earnings (e.g., Collins and Kothari 1989; Easton and Zmijewski 1989; Dhaliwal, Lee, and Fargher 1991).

We include *Fixed effects* for the auditor's global network, country of domicile, the year-quarter of the firm's fiscal year-end, and interactions of

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

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

In Table 3, panel B, we present results from an alternative design that reduces potential contamination effects from the overlap in the pre- and post-periods

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

²⁰ Prior studies use a variety of approaches to deal with extreme *UE* observations, including deleting *UE* observations that exceed a specified percentage of price (e.g., 100%) and deleting observations with large standardized residuals (e.g., Collins and Kothari 1989; Teoh and Wong 1993; Francis and Ke 2006; Chen, Cheng, and Lo 2014). In IA§6, we present scatterplots for untrimmed and truncated data across a variety of truncation levels and provide several additional analyses to assess the sensitivity of our results to extreme *UE* observations. These analyses suggest that the method for handling extreme *UE* observations can affect the strength of our inferences.

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

when using alternative cutoff dates (e.g., in the primary design, the pre-period for the report release overlaps with the post-period for fieldwork). In this design, we exclude pre-period fiscal year-ends that occur during fieldwork and prior to the release of the inspection report. To reduce sample attrition, we extend the sample window to maintain pre and post periods that are of similar length to the main analyses. Figures A.1 and A.2 in Appendix A illustrate the limited and full inspection designs without this overlap (which we call the “dropped observations” design). Results with this design are stronger, particularly in

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

A. Analyses using the primary design

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

Table 3 presents separate analyses for each inspection event (limited and full) and each sample window cutoff date (end of fieldwork and report release). Panel A reports results for our analysis using the primary design as described in Figures A.1 and A.2 in Appendix A. Following Equation (1), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). For brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or most of the interactions among these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Table B.1 in Appendix B defines each variable in detail. We include fixed effects for the auditor (defined at the global network level), the auditor’s country of domicile, and the respective fiscal year-end, as well as the interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Column 1, we examine the changes in ERCs following fieldwork completion for limited inspections. In Column 2, we examine the changes in ERCs following inspection report releases for limited inspections. In Column 3, we examine the changes in ERCs following fieldwork completion for full inspections. In Column 4, we examine the changes in ERCs following inspection report releases for full inspections. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. For all robust regressions, we calculate firm-level clustered standard errors using a weighted least squares regression based on the weights (and coefficients) generated by the *reg* command. **p* < .1; ***p* < .05; ****p* < .01 (two sided).

(Continued)

Table 3
(Continued)*B. Analyses for the dropped observations design*

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

Panel B reports results for our analysis using the dropped observations design as described in Figures A.1 and A.2 in Appendix A. Following Equation (1), we regress cumulative abnormal returns (*CAR*) on unexpected earnings (*UE*), indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). For brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or most of the interactions among these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Table B.1 in Appendix B defines each variable in detail. We include fixed effects for the auditor (defined at the global network level), the auditor's country of domicile, and the respective fiscal year-end, as well as the interactions of these fixed effects with *UE*. In all columns, we estimate a robust regression. In Column 1, we examine the changes in ERCs following fieldwork completion for limited inspections. In Column 2, we examine the changes in ERCs following inspection report releases for limited inspections. In Column 3, we examine the changes in ERCs following fieldwork completion for full inspections. In Column 4, we examine the changes in ERCs following inspection report releases for full inspections. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. For all robust regressions, we calculate firm-level clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

Column 4, which is where the contamination effects from overlap are likely the most severe. As expected, the described overlap biases against our results. To be conservative, we use the design without the dropped observations as our primary specification.

Table 4, panel A, presents results when stacking the samples for the limited and full inspections and the fieldwork end and inspection report release dates (hereafter, the “combined” sample), which effectively provides the average change in ERC across the four alternative sample windows. This presentation is parsimonious without favoring a particular date, and it also exploits the variation in firms' fiscal year-ends more effectively, which is why we use it for

the subsequent analyses. In this specification, $UE \times Post \times Treated$ is positive and significant at the 1% level (Column 1).²² This specification should deliver a conservative estimate of the treatment effect because it pools the relatively small response to the limited inspections with the larger response to the full inspections. We report results for the combined sample using the dropped observation design in Column 2; the coefficient magnitude is similar and is also significant at the 1% level.

The key assumption underlying our identification strategy is that the treatment and control firms would have had similar trends in their ERCs absent the introduction of public audit oversight (i.e., the parallel trends assumption). To assess the reasonableness of this assumption, in Figure 1, we replace the single $Post \times Treated \times UE$ interaction term with separate interactions for each of the years in our sample, except for the year immediately before the introduction of the PCAOB regime, and map out the treatment effect in event time. In the pre-period, the coefficients for the incremental ERC are small

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

A. Main results combining inspection events and cutoff dates

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

(Continued)

²² For completeness, in IA§7, we report a full tabulation of all the coefficient estimates, excluding the coefficient estimates for the fixed effects and fixed effects interacted with UE .

Table 4
(Continued)*B. Main results, including additional firm-level fixed effects*

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

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

and statistically insignificant, which supports the parallel trends assumption.²³ The treatment effect is positive but statistically insignificant in period *T* and

²³ In IA§4, we examine past trends in ERCs for our treatment and control firms over an extended period and again find no evidence that calls into question the validity of the parallel trends assumption.

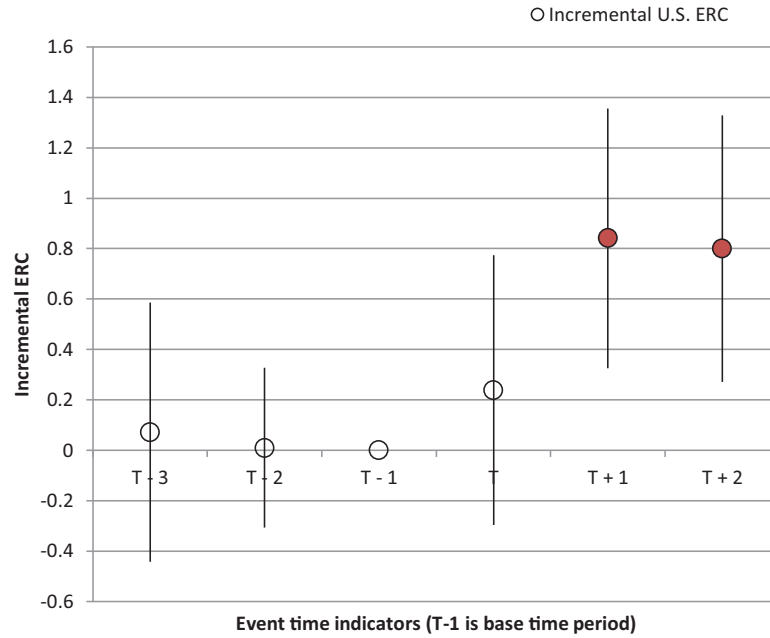


Figure 1
Mapping the estimated treatment effect by event time

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

becomes economically and statistically significant in periods $T+1$ and $T+2$, consistent with the coefficient pattern in Table 3 that shows stronger results after the first full inspections.

Although the evidence regarding the parallel trends assumption is reassuring, recall that our treatment and control firms differ along two observable dimensions, *Size* and *Beta*. For this reason, we also conduct an analysis using coarsened exact matching (CEM) (see Blackwell et al. 2009) based on both these firm characteristics. CEM relies on covariate weighting to construct a synthetic control sample, allowing us to preserve sample size. We coarsen our sample into 20 CEM bins (per matching variable), which reflects a trade-off between preserving observations and the ex post similarity of the matching variables for the treatment and control groups. We then use the weights from this coarsening in estimations of Equation (1). After applying the CEM weights,

average *Size* and *Beta* are very similar for the treatment and the control samples (see Table 2, panel C). Table 4, Column 3, presents the regression results with the CEM weights. They are consistent with the results in Column 1, which do not use the CEM weights.

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

In Table 4, Column 4, when we separately estimate the treatment effect for profit and loss firms, we find that the results are stronger and that the credibility effects of the new regime are concentrated in profitable firms, which is consistent with our expectations and corroborates our interpretation (i.e., the $UE \times Post \times Treated$ coefficient is 0.942).²⁵

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

Next, we present several additional tests to gauge the influence of firm-level heterogeneity on our results. To address this concern, it is common to

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

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

include firm fixed effects in a generalized difference-in-differences estimation. However, this approach poses several issues in our analyses. First, our main analysis is based on a short 4-year window, centered on the onset of the regime. This means that we typically have only two firm-year observations before and after an inspection, which makes the estimation of firm fixed effects quite demanding and noisy. Second, including firm fixed effects in an ERC regression is different from a standard difference-in-differences setting where the left-hand-side variable is the outcome of interest. In an ERC regression, the outcome of interest is an interaction on the right-hand side of the model. Therefore, including firm fixed effects (without interacting them with *UE*) does not control for firm-level differences in the ERC. It controls for a firm's average CAR at the earnings announcement (and hence can help with sample composition changes over time of firms with different CARs). But in order to have a within-firm ERC analysis, it is necessary to interact *UE* with firm fixed effects. However, such a fully interacted model is not feasible with only four observations per firm. Given these issues, we use three alternative approaches to assess the impact of firm-level heterogeneity on our results.

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

Third, instead of estimating the ERC in a regression, we define it as *CAR* divided by *UE* and then use this proxy as the dependent variable. With this specification, we can control for time-invariant firm-level heterogeneity with firm fixed effects without using interactions. However, our sample period is still short, so the downside of this approach is that the “ERC” proxy is quite

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

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

noisy. Nevertheless, using this approach, we obtain similar results and draw essentially the same inferences as in our primary analyses (see IA§9). Thus, based on all three approaches, we conclude that firm-level heterogeneity is unlikely to drive our results.

3.2 Sensitivity analyses: Changes in information environment and concurrent events

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

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

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

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
<i>Post</i> × <i>Treated</i>	-0.001** (-2.285)	0.000 (0.227)	0.003 (0.366)	0.026*** (5.253)	0.003 (0.876)	-0.021 (-1.234)	-0.010 (-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
<i>Loss</i> 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 <i>R</i> -squared	.032	.410	.249	.494	.147	.137	.114

Table 5 presents tests for other concurrent changes in the information environment around the introduction of the PCAOB inspection regime. We estimate the treatment effects separately for profit and loss firms, but only report the effects for profit firms. In Columns 1 (2, 3, 4, 5, 6, 7), we regress *UE* (*Forecast*, *Timeliness*, *Relative information*, *Scaled raw accruals*, *Earnings guidance*, and *Guidance bundle*) on indicators for PCAOB inspection (i.e., *Post* and *Treated*), controls, and fixed effects. In all columns, for brevity, we do not report coefficients for the control variables, fixed effects, and treatment indicator main effects. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Table B.1 in Appendix B defines each variable in detail. We include fixed effects for the auditor (at the global network level), the firm's country of domicile, and the year-quarter of the respective fiscal year-end. In all columns, we report OLS regressions. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

The observed increase in *Relative information* suggests that in the new regime, treated firms release more of the year's total information before the earnings announcement. The increase of 0.026 is about 18% of 1 standard deviation (0.144). Again, this effect is relatively small. More importantly, if firms release more of the year's total information before the earnings announcement, this should decrease the relative importance of the earnings announcement. Thus, the change in *Relative information* likely works against finding an increase in the ERC. In an untabulated test, we confirm that our results are essentially unchanged when we include *Relative information* as an additional control variable (interacted with *UE*). Consistent with a decrease in *Relative information* biasing against our results, the treatment effect for $UE \times Post \times Treated$ increases slightly (0.860) and is still significant at the 1% level. We also confirm that results do not change materially if we simultaneously include all seven information environment proxies as controls (untabulated). Overall, there is no evidence that our findings are explained by significant changes in pre-earnings-announcement disclosures, management guidance, earnings' properties, and/or analyst forecast behavior.²⁹

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

In the third set of analyses, we address the possibility that the observed ERC increase could reflect other SOX provisions. Three provisions stand out as possibilities: (1) rules regarding audit committee independence, (2) Section 302 rules regarding executive certification of financial statements, and (3)

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

Table 6
Are results driven by changes in reporting incentives or by other provisions of the Sarbanes-Oxley Act?

Dependent variable: <i>CAR</i>	(1) Reporting Incentives		(3)	(4)	(5) Sarbanes-Oxley Act		(6)	(7)
	Non-AA clients	Only AA clients			Excluding 404[b]	Only 404[b]		
<i>UE</i> × <i>Post</i> × <i>Treated</i>	1.030*** (3.662)	0.492 (1.415)	1.139** (2.570)	0.871*** (3.102)	0.923*** (3.153)	0.234+ (0.632)	0.921*** (3.306)	
<i>UE</i> × <i>SOX404b</i>	—	—	—	—	—	—	0.275 (1.375)	
<i>UE</i> × <i>SOX302a</i>	—	—	—	—	—	—	−0.900*** (−3.276)	
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
<i>UE</i> × Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Treatment indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
<i>UE</i> × 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	
<i>UE</i> × Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
<i>Loss</i> & <i>UE</i> × <i>Loss</i> 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 <i>R</i> -squared	.054	.075	.091	.050	.057	.062	.053	

Table 6 presents sensitivity analyses that examine the role of changes in market-based reporting incentives and other provisions of SOX. We separately estimate the treatment effects for loss and profit firms, though we only report the effects for profit firms. Following Equation (1), we regress cumulative abnormal returns (CAR) on *UE*, indicators for PCAOB inspection (i.e., *Post* and *Treated*), control variables, fixed effects, the interactions of *UE* with control variables and fixed effects, and the interactions of the treatment indicators with *UE* (as noted in the table footer). In all columns, for brevity, we do not report coefficients for the control variables, fixed effects, treatment indicator main effects, or the interactions among these variables. Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Table B.1 in Appendix B defines each variable in detail. We include fixed effects for the auditor (at the global network level), the firm's country of domicile, and the year-quarter of the respective fiscal year-end. In all columns, we estimate a robust regression. In Columns 1 and 2, we partition the treatment sample based on whether the firm was audited by Arthur Andersen in 2000 or 2001 (as indicated by the column headings). In Columns 3 and 4, we partition the treatment sample based on whether a firm-year observation is classified as an accelerated filer in Audit Analytics (as indicated by the column headings). In Columns 5 and 6, we partition the treatment sample based on whether a firm-year has an auditor internal-control opinion (effective, adverse, or disclaimer) in Compustat (as indicated by the column headings). As additional controls, in Column 7, we include the indicator variables *SOX404b* and *SOX302a* and their interactions with *UE*. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. + indicates significance (two-sided) at the 10% level for tests of the coefficient magnitudes, relative to the adjacent column on the left. For all robust regressions, we calculate firm-level clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression. * $p < .1$; ** $p < .05$; *** $p < .01$ (two sided).

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

In contrast, the adoption of Section 404[b] was staggered based on issuer size and domicile. For U.S. accelerated filers (i.e., firms with market capitalizations greater than \$75 million), Section 404[b] became effective for fiscal year-ends on or after November 15, 2004. The SEC deferred implementation for nonaccelerated filers because of cost concerns. In 2010, the Dodd-Frank Act made this exemption permanent. Foreign accelerated filers were not subject to Section 404[b] until July 15, 2006 or July 15, 2007, depending on their size. Prior research documents that the market responds negatively to the disclosure of 404[b] internal control weaknesses (e.g., Hammersley, Myers, and Shakespeare 2008). Thus, if firms improve their internal controls, and if better controls lead to more credible reporting, it is possible that the effects documented in Table 4 are attributable to the implementation of SOX 404[b] rather than the new public oversight regime for auditors.

We conduct two analyses to separate the PCAOB regime from other SOX provisions. First, in an approach similar to Iliev (2010), we separately examine ERC changes for accelerated and nonaccelerated filers. If the documented increase in credibility is attributable to the new oversight regime (instead of 404[b]), we expect similar effects for accelerated and nonaccelerated filers. Results in Columns 3 and 4 of Table 6 are consistent with this prediction. The treatment effect for nonaccelerated filers is 1.139 as compared to 0.871 for accelerated filers. These coefficients are not significantly different and, if anything, indicate a larger ERC change for nonaccelerated filers—a result that goes against the alternative explanation.

Second, we separately examine ERC changes within treatment firms based on whether or not a firm has an internal control opinion from its auditor—be it an effective, adverse, or disclaimer opinion (i.e., we estimate our effects for firms outside and within the SOX 404[b] regime). If the internal control opinions required under SOX 404[b] made earnings more credible, we would expect a larger treatment effect for firms with such opinions. The results, presented in Columns 5 and 6 of Table 6, do not support this conjecture. The estimated treatment effect for firms *without* a SOX 404[b] internal control opinion (0.923) is larger than for firms with an opinion (0.234); the difference in the coefficients is statistically significant at the 10% level, suggesting that the effects we document are distinct from the potential impact of SOX 404[b].

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

To be sure, in Column 7, we simultaneously include both indicators in our model to control for the effects of SOX 404[b] and SOX 302[a]. The estimated treatment effect is similar to that in Table 4, which again provides assurance that the documented increase in reporting credibility is not attributable to other key SOX provisions.³¹ The next section provides further support for this conclusion.

3.3 Analysis of small, triennially inspected auditors

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

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

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

Table 7 presents results for this analysis. In Column 1, we estimate a robust WLS regression of Equation (2), where $Post$ is based on the fieldwork end date. The estimated treatment effect of 0.789 is positive and significant at the 5% level. In Column 2, $Post$ is based on the report release date. $UE \times Post$ is positive (1.063) and statistically significant at the 5% level. The larger

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

³² There are two exceptions. First, for triennially inspected auditors, fieldwork is shorter, and it is less clear that the market is aware of its timing. Thus, we code the $Post$ variable as equal to one for any earnings announcement occurring 30 days after the end of the PCAOB's inspection fieldwork (or alternatively, the day following the inspection report release). Second, because small firms have less analyst coverage, we extend our window for measuring the median analyst forecast (from which UE is computed) from 95 to 360 days.

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

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

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

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

Table 7 presents results for the analysis of the change in reporting credibility around the introduction of triennial PCAOB inspections. Following Equation (2), we regress cumulative abnormal returns (CAR) on *UE*, an indicator for PCAOB inspections (i.e., *Post*), control variables, fixed effects, the interactions of *UE* with the control variables, the interacted fixed effects, and the treatment indicator (as shown in the table footer). Controls include *Loss*, *Size*, *M2B*, *Leverage*, *Persistence*, and *Beta*. Table B.1 in Appendix B defines each variable in detail. In all columns, we include fixed effects for the auditor and the year-quarter of the respective fiscal year-end. We estimate robust regressions. We separately estimate the treatment effect for profit and loss firms by including additional interactions. We report the coefficient of interest for profit firms only. In Column 1, we examine changes in ERCs using the fieldwork cutoff date (i.e., *Post* equals 1 if the firm's fiscal year-end is at least 30 days after fieldwork completion). In Column 2, we examine changes in ERCs using the report release as the cutoff date (i.e., *Post* equals 1 if the firm's earnings announcement is after the report release date). In Columns 3 through 6, we perform tests similar to the specifications for annually inspected auditors. In Columns 3, 5, and 6, we use the report release design from Column 2. As additional controls, in Column 3, we add the indicator variables *SOX404b* and *SOX302a* and their interactions with *UE*. In Column 4, we examine the dropped observation design for triennially inspected auditors. In Column 5, we include firm-level main effects that are not interacted with *UE*. In Column 6, we include firm-group fixed effects interacted with *UE*; these are generated by forming 16 portfolios (4-by-4) using *Size* and *Beta* from the first year that a firm enters the sample (see Internet Appendix §9 for more details on groups). The firm-group fixed effects are included in the model as main effects and with their respective interactions, including *UE*. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. We calculate firm-level clustered standard errors using a weighted least squares regression based on the weights (and coefficients) from the robust regression. ***, **, * $p < .01$, $p < .05$, $p < .1$, respectively (two sided).

coefficient magnitude for the inspection report release is consistent with less publicized fieldwork dates for triennial firms. In Column 3, we include additional controls for SOX 404[b] and 302[a] and (using the report release date) find similar results, which indicates that the increase in reporting credibility is not attributable to other SOX provisions. Column 4 reports results for the “dropped observations” design, which excludes the post-fieldwork period from the sample to avoid overlap and contamination. The treatment effect (1.022) is similar to the other specifications. Again, assuming a pre-period benchmark return response for a 1% earnings surprise of 3.7%, in the post-PCAOB regime period, an earnings surprise of the same magnitude leads to a price change of about 4.7%, an increase of about 28% for clients of triennially inspected auditors.

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

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

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

In this section, we examine abnormal trading volume around the SEC filing of firms’ annual financial statements (10-Ks) as an alternative measure of reporting credibility. While prior empirical studies generally interpret abnormal trading volume as a measure of the information content of firm disclosures (e.g., [Asthana and Balsam 2001](#); [Asthana, Balsam, and Sankaraguruswamy 2004](#); [Leuz and Schrand 2009](#)), it likely also reflects the credibility of the information released. [Kim and Verrecchia \(1991\)](#) model the relation for abnormal trading volume and show that the results for price reactions in [Holthausen and Verrecchia \(1988\)](#), on which the ERCs rely, extend to trading volume even when investors are diversely informed. Thus, the conceptual underpinnings discussed in Section 2.1 still apply. If the new audit oversight regime increases reporting credibility, we predict a stronger volume reaction.

The abnormal trading volume proxy also has some empirically desirable properties. Like ERCs, abnormal trading volume around an information event

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

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

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

$$\begin{aligned} \text{Abnormal 10-K Volume}_{i,t} = & \alpha + \beta_1 \text{Post}_t + \beta_2 \text{Treated}_i + \beta_3 \text{Post} \\ & \times \text{Treated}_{i,t} + \lambda_n \text{Controls}_{i,t} \\ & + \gamma_n \text{Fixed effects} + \varepsilon_{i,t} \end{aligned} \quad (3)$$

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 large auditors. Following [Leuz and Schrand \(2009\)](#), we include several controls from the ERC tests including *Size*, *Market-to-book*, *Leverage*, *Beta*, and *Loss*. We control for the number of days between a firm's fiscal year-end and the 10-K release (*Filing delay after FYE*) and between the earnings announcement and the 10-K release (*Filing delay after EA*), following [Asthana, Balsam, and Sankaraguruswamy \(2004\)](#). We also include *Analyst following*, since some sample firms do not have analyst coverage.

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

Table 8 presents the regression results. In Column 1, we estimate Equation (3) using ordinary least squares (OLS) and include auditor-, country-, and year-quarter fixed effects. In Column 2, we also include firm fixed effects. In both

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

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

Table 8 presents results for the analysis of changes in abnormal trading volume around 10-K filings after the introduction of the PCAOB regime. Following Equation (3), we regress *Abnormal 10-K volume* on indicators for PCAOB inspections (i.e., *Post* and *Treated*), control variables, and fixed effects (as indicated in the table footer). Table B.1 in Appendix B defines each variable in detail. We include fixed effects for the auditor (at the global network level), the firm's country of domicile, the year-quarter of the respective fiscal year-end, and the firm (as indicated in the table footer). In Column 1, we report the baseline specification estimated using OLS. We repeat this in Column 2, but substitute firm fixed effects for auditor and country fixed effects. In Column 3, we report the primary design with weighted least squares (WLS) using weights from a coarsened exact matching (CEM) procedure that uses 20 bins for the control variables *Size*, *Beta*, and *Loss*; unmatched bins result in 1,652 fewer observations. In Column 4, we use WLS with the CEM weights from Column 3 and include continuous variable *log(10-K file size)*. In Column 5, we use WLS with the CEM weights from Column 3 and include the indicator variables *SOX404b* and *SOX302a*. All *t*-statistics (in parentheses) are based on standard errors clustered at the firm level. **p* < .1; ***p* < .05; ****p* < .01 (two sided).

columns, the treatment effect, *Post* × *Treated*, is positive and significant (at the 5% level or greater). In Column 3, we use a similar approach as in Table 4 and employ CEM based on *Size*, *Beta*, and *Loss*.³⁵ In Column 3, the coefficient of interest has a magnitude of 0.136, which translates into a 14.6% increase in abnormal trading volume around the release of a firm's 10-K.

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

In Column 4, following [Loughran and McDonald \(2014\)](#), we include the log of the 10-K file size ($\log(10\text{-K file size})$) 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]. Although the magnitudes and standard errors differ slightly across specifications, the results and inferences are robust and similar to those in Column 3.

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

4. Conclusion

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

Despite many sensitivity analyses, our results should be interpreted cautiously as our study is subject to several limitations. First, although our analyses show sustained increases in reporting credibility for at least 2 years, ERCs are based on investor perceptions and can change as more information about the oversight regime (and reporting and audit quality) becomes publicly available. Second, attributing the credibility effect to public audit oversight depends critically on our ability to control for other concurrent changes in regulation and markets with our difference-in-differences analyses. Third, because ERCs are noisy and difficult to measure, the magnitude of our estimates should be interpreted carefully. Fourth, while we provide evidence that other

SOX provisions do not appear to drive our results, it is difficult to rule out the possibility that our results reflect the joint effect of these provisions and public audit oversight. Fifth, our results are relative to the prior peer review regime and do not rule out the possibility that a substantially reformed peer review system could also have increased reporting credibility. Sixth, our study focuses on the capital market benefits of public audit oversight, but does not examine the costs of the new regime. Thus, we do not show *net* benefits. Seventh and finally, our analysis is limited to equity investors. Given the role of auditing in debt contracting, it is conceivable that public audit oversight also provides benefits to (and has costs for) other stakeholders (e.g., [Costello and Wittenberg-Moerman 2011](#); [Minnis 2011](#)). We leave this question to future research.

Appendix A

Details on the Timing of the Introduction of the PCAOB Regime and Identification Strategy

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

Table A.1
Annually inspected auditor fieldwork and inspection report release dates

Auditor	Fieldwork		Report date
	Commences	Concludes	
Limited inspections			
<i>Big Four auditors</i>			
Deloitte & Touche	June 2003	December 2003	August 26, 2004
Ernst & Young	June 2003	December 2003	August 26, 2004
KPMG	June 2003	December 2003	August 26, 2004
PricewaterhouseCoopers	June 2003	January 2004	August 26, 2004
Full inspections			
<i>Big Four auditors</i>			
Deloitte & Touche	May 2004	November 2004	October 06, 2005
Ernst & Young	July 2004	December 2004	November 17, 2005
KPMG	June 2004	October 2004	September 29, 2005
PricewaterhouseCoopers	May 2004	January 2005	November 17, 2005
<i>Tier-Two auditors</i>			
BDO	May 2004	July 2004	November 17, 2005
Crowe Chizek	November 2004	December 2004	January 19, 2006
Grant Thornton	May 2004	March 2005	January 19, 2006
McGladrey & Pullen	October 2004	December 2004	November 30, 2005

This table provides the beginning and end dates for PCAOB fieldwork and the inspection report release dates for both limited and full inspections by auditor. We use these dates to define the sample window for each of the respective analyses. Whether a firm's fiscal year-end falls within a given sample window determines which earnings announcement observations are included. However, in setting the pre- and post-period windows, we also consider the earnings announcement timeline of a typical firm in determining which fiscal year-end months to include. We do not use the timing of earnings announcements to determine the sample window because the timing is not fixed, and thus a firm could enter or exit the sample for endogenous reasons related to that timing. Instead, we use fiscal year-end dates, which are fixed. See also Section 2.3 and Figures [A.1](#) and [A.2](#) for further details on the sample windows.

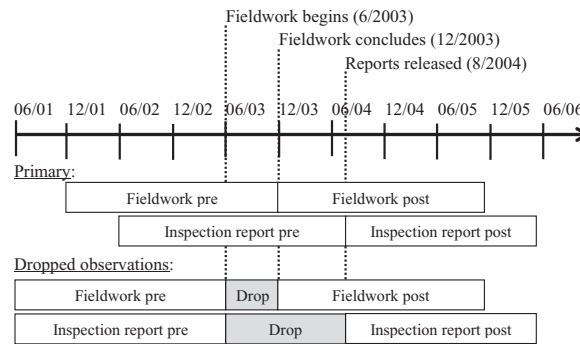
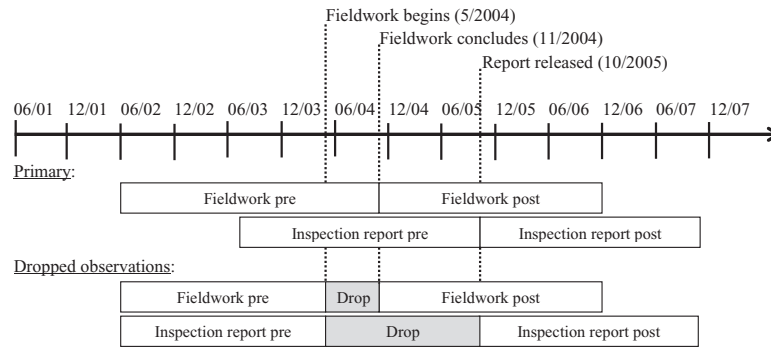


Figure A.1
Limited inspections: Treatment timing (specific dates from Deloitte & Touche are presented as an example)
 This figure describes the coding of the *Post* variable around the limited inspections. We use two different designs. In the primary design, we use the end of fieldwork across audit firms and the release of the inspection report as alternative cutoff dates to define the sample window. For the limited inspections, using the fieldwork (inspection report) cutoff date, the sample window is defined between December 2001 and November 2005 (June 2002 and May 2006). The sample includes firms whose fiscal year-ends fall into this window. For analyses using the end of fieldwork date, *Post* equals one if a firm's fiscal year ends in the final month of the fieldwork or later. For analyses using the inspection report date, *Post* equals one if a firm's fourth-quarter earnings announcement falls on or after the release of the inspection report. Typically, there are two firm-year observations before and after the relevant cutoff date. In the dropped observations design, to avoid overlapping pre and post periods, we exclude fiscal year-ends occurring during PCAOB fieldwork when the fieldwork end is the cutoff date and exclude fiscal year-ends occurring between the start of fieldwork and the release of the inspection report when the inspection report release is the cutoff date. To reduce sample attrition, we extend the sample window to maintain pre and post periods that are of similar length to the main analyses. Time line dates are presented in the following format: MM/YY.

**Figure A.2****Full inspections: Treatment timing (specific dates from Deloitte & Touche are presented as an example)**

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

Appendix B

Table B.1
Variable definitions

Variables used in calculating earnings response coefficients

$CAR_{i,t}$	A firm's 3 trading day return, centered on the earnings announcement date, less the CRSP market return over the same period. The earnings announcement date is the earliest date available on Compustat or I/B/E/S
$UE_{i,t}$	The difference between the I/B/E/S annual EPS and the median I/B/E/S forecast of annual EPS from each analyst's most recent forecast in the window starting 95 calendar days before and ending 3 days before the earnings announcement, scaled by the CRSP price 2 days before the earnings announcement. We supplement the triennially inspected auditor analysis forecasts by including the difference between the I/B/E/S actual, annual EPS, and the median I/B/E/S forecast of annual EPS from each analyst's most recent forecast in the window starting 360 calendar days before and ending 3 days before the earnings announcement when the shorter window, detailed above, does not contain a forecast
PCAOB inspection indicators	
$Post_{i,t}$	An indicator variable (based on an auditor's global network) that equals one for all firm-years after a firm's auditor's U.S. affiliate's treatment through the PCAOB inspection process, defined for each event as follows: (1) Big Four limited and full inspection fieldwork and Tier-Two full inspection fieldwork: <i>Post</i> equals one if a firm's fiscal year-ends in the same month as the final month of fieldwork (as indicated in Table A.1 in Appendix A) or later, and zero otherwise; (2) triennially inspected auditor full inspection fieldwork: <i>Post</i> equals one if a firm's fiscal year-ends after the auditor-specific fieldwork end date plus 30 days, and zero otherwise; and (3) Big Four limited and full inspection report release, triennially inspected auditors' inspection report release, and Tier-Two full inspection report release: <i>Post</i> equals one if a firm's fourth-quarter earnings announcement falls on or after the release date of the inspection report (as indicated in Table A.1 in Appendix A), and zero otherwise
$Treated_{i,t}$	An indicator variable coded as one if a firm is audited by an auditor subject to a (limited or full) PCAOB inspection, and zero otherwise
Control variables	
$Analyst\ following_{i,t}$	The number of unique analysts who issue at least one forecast on I/B/E/S in a window starting 360 calendar days before and ending 3 days before the earnings announcement. When no forecasts are observed, we set this count to zero
$Beta_{i,t}$	The coefficient from regressing excess daily returns for firm <i>i</i> on excess market returns over one calendar year, ending on the fiscal year-end date. The risk-free rate is collected from Kenneth French's data library
$Filing\ delay\ after\ EA_{i,t}$	The number of days between the earnings announcement date (the earliest date available on Compustat or I/B/E/S) and the filing date of the 10-K (the earliest date reported by Audit Analytics or WRDS SEC Analytics). We use many variations of 10-K filings (e.g., 10-K405, 10-KSB, 20-F)
$Filing\ delay\ after\ FYE_{i,t}$	The number of days between the firm's fiscal year-end (from Compustat) and the filing of the 10-K (based on the earliest date reported by Audit Analytics or WRDS SEC Analytics). We use many variations of 10-K filings (e.g., 10-K405, 10-KSB, 20-F)
$Leverage_{i,t}$	The ratio of total liabilities to total equity, measured at the fiscal year-end, from Compustat

(Continued)

Table B.1
(Continued)

$\log(10\text{-K file size})$	The natural log of the file size of the firm's 10-K SEC filing, from WRDS SEC Analytics. We use many variations of 10-K filings (e.g., 10-K405, 10-KSB, 20-F)
$Loss_{i,t}$	An indicator variable coded as one when basic earnings per share excluding extraordinary items from Compustat (<i>epspx</i>) 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
$Persistence_{i,t}$	The coefficient from regressing basic EPS excluding extraordinary items from Compustat on lagged EPS using (where available) up to 10 years of data
$Size_{i,t}$	The natural log of the market value of equity measured at fiscal year-end, from Compustat
$SOX302a_{i,t}$	An indicator variable coded as one when the "IS EFFECTIVE" variable in the Audit Analytics SOX 302 data set is coded as a "0," "1," or "2," and zero otherwise. This variable is coded 1 only for domestic firms
$SOX404b_{i,t}$	An indicator variable coded as one when the auditor internal control opinion (AUOPIC) variable in Compustat shows an adverse, qualified, or unqualified indicator, and zero otherwise. This variable is only coded 1 for domestic firms

Alternative dependent variables

$Abnormal\ 10\text{-K}\ volume_{i,t}$	The mean abnormal trading volume in a window starting 1 day before the 10-K filing date and ending 3 days after. Abnormal trading volume is defined as raw volume less mean daily volume over a window starting 49 days before and ending 5 days before the annual financial statement report release (excluding any 3-day earnings announcement window days) divided by the standard deviation of daily volume over the same window. All volume data is from CRSP. The 10-K filing date is defined as the earlier of the date reported by Audit Analytics (as long as it is after the earnings announcement date) and the first observable 10-K date from WRDS SEC Analytics in a 180-calendar-day window beginning on the earnings announcement date
$Earnings\ guidance_{i,t}$	An indicator variable coded as one when a guidance observation (quarterly or annual) is available for the fiscal year-end date on either First Call or I/B/E/S, and zero otherwise
$Forecast_{i,t}$	The median I/B/E/S forecast of annual EPS, using each analyst's most recent forecast in a window beginning 95 days before and ending 3 days before the earnings announcement, scaled by the CRSP price from 2 days before the earnings announcement
$Guidance\ bundle_{i,t}$	An indicator variable coded as one when management provides earnings guidance for any fiscal period (quarterly or annual) within one calendar day of the earnings announcement on either First Call or I/B/E/S, and zero otherwise
$Relative\ information_{i,t}$	This variable captures the share of information arriving prior to the earnings announcement relative to the total amount of information reflected in equity prices over a firm's fiscal year, calculated as the sum of the absolute value of daily market-adjusted CRSP returns starting 345 calendar days before and ending the day before the earnings announcement window, divided by the same value plus predicted returns (based on the implied return to a given level of earnings surprise using the firm's estimated ERCs) for the 3-day earnings announcement window, scaled by 100

$$100 \cdot \frac{\sum_{d=-345}^0 |r_{i,d} - r_{M,d}|}{\left| \widehat{\alpha}_{lag2} + UE_i \cdot \widehat{ERC}_{lag2} + Loss_i \cdot \widehat{\beta}_{lag2}^{Loss} + UE_i \cdot Loss_i \cdot \widehat{\beta}_{lag2}^{LossERC} \right| + \sum_{d=-345}^0 |r_{i,d} - r_{M,d}|}$$

(Continued)

Table B.1
(Continued)

	<p>Returns are from CRSP, and d represents the number of calendar days relative to 2 trading days prior to the earnings announcement. To increase the precision of the measurement, we allow separate ERC coefficients for profits and losses estimated from cross-sectional regressions 2 years prior to t</p>
<i>Scaled raw accruals</i> $_{i,t}$	<p>The difference between net income and cash flow from operations scaled by average total assets, from Compustat</p>
<i>Timeliness</i> $_{i,t}$	<p>This variable captures how quickly market prices impound the information reflected in price at $p_{d=0}$, calculated following Beekes and Brown (2006) and given by the equation:</p> $-1 \times \frac{\sum_{d=-345}^0 \log(p_{d=0}) - \log(p_d) }{\sum_{d=-345}^0 1_d}.$ <p>We multiply by -1 so that the measure is increasing in timeliness. Prices are from CRSP, and d represents the number of calendar days relative to 2 trading days before the earnings announcement. The indicator function in the denominator turns on when d is a trading day</p>

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

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