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ABSTRACT

We investigate changes in the quality and cost of audit services surrounding PCAOB Rule 3211, which requires disclosure of audit partner names in Form AP. To isolate changes due to Rule 3211 from other confounding factors, we use difference-in-differences analyses with separate control groups, including a group of companies that disclosed partner identities prior to Rule 3211. Our study also incorporates several measures from prior literature to proxy for various dimensions of audit quality. Evidence from the difference-in-differences analyses reveals that any immediate impact of Rule 3211 on audit quality or fees is limited to specific dimensions of audit quality, specific control groups, and/or specific company characteristics. We reach this conclusion after considering alternative research designs and evaluating confidence intervals for statistically insignificant coefficients. We caution that our findings only provide initial evidence and further research is necessary to evaluate other potential impacts of Rule 3211.

Keywords: PCAOB; Rule 3211; Form AP; audit partner; audit quality; audit fees; difference-in-differences

For me, this issue has always been more about improving audit quality, which is not where it should be, by enhancing and influencing a leader's sense of individual accountability and acceptance of responsibility for a team effort he or she has led by signing his or her name to a most commonly reviewed report, as opposed to simply being identified in a newly developed form. However, I understand that reasonable people may agree to disagree, which is why I support today's compromise which will result in the creation of a new standardized form—the Form AP.

—Steven B. Harris, 2015 PCAOB Open Board Meeting

I. INTRODUCTION

The PCAOB recently adopted Rule 3211 (*Auditor Reporting of Certain Audit Participants*) that requires disclosure of audit engagement partners in Form AP. Registered public accounting firms must comply with this new rule for audit reports issued on or after January 31, 2017 (PCAOB 2015b). The PCAOB motivated this new disclosure requirement by the need to improve transparency of the audit process and to increase the engagement partner's sense of accountability. Partner "identifiability" links the audit outcome to the individual partner's performance and is expected to enhance accountability by motivating partners to avoid negative consequences (Lerner and Tetlock 1999; DeZoort et al. 2006). The underlying assumption of this new rule—as reflected in the PCAOB's standard-setting process—is that audit quality will improve following mandatory disclosure of partner names for each audit (PCAOB 2009, 2015b). However, opponents of this regulation argue that engagement partners already have a strong sense of accountability that is supported by the accounting firm's quality control system as well as PCAOB oversight (e.g., Deloitte 2009; EY 2009; CAQ 2012; KPMG 2012; PwC 2012). Based on these arguments, mandatory identification of partners would not result in improvements to audit quality. The goal of this paper is to examine whether adoption of Rule 3211 affects the quality of audit services in the U.S. Such analysis enhances our understanding of this important regulatory rule and its impact on practice.

Given the importance of assessing costs of regulatory changes, our study also examines

whether adoption of Rule 3211 affects the cost of audit services. Opponents of the ruling argue that disclosing partner names will increase audit costs since partners may perceive an increase in legal liability, making them less willing to develop independent professional judgments and instead perform unnecessary procedures or engage in unnecessary consultations to mitigate increased personal risk (e.g., Grant Thornton 2009; PwC 2009; EY 2014). However, the cost could remain unchanged if disclosure in Form AP does not increase partners' potential legal liability relative to the prior regime without mandatory partner disclosure.

While other countries such as China, Australia, United Kingdom (U.K.), Germany, and France already require audit partner identification, public accounting firms in the U.S. have only been required to disclose the name of the accounting *firm* in regulatory filings. In the introductory quote, Former Board Member Harris refers to disclosure of U.S. partner names in Form AP as a "compromise" since the PCAOB faced significant opposition to the inclusion of partner names in the audit report (refer to Section II for further details). There are two major distinctions between Rule 3211 in the U.S. and partner identification rules in other countries: location and timing of the disclosure. U.S. partners still sign only the *firm's* name in the audit opinion instead of signing their personal names as is required in several other countries. Engagement partner names are disclosed in Form AP, which is filed on the PCAOB's website no later than 35 days after the audit report is first included in a document filed with the SEC. While these requirements reduce accounting firms' concerns about potential increases to partners' legal liability (CAQ 2015; CII 2015; KPMG 2015; PwC 2015), they also remove psychological effects associated with signing the report. As a result, some investor groups argue that separate disclosure in Form AP may not provide the same sense of accountability as would have been achieved had the partner signed or disclosed her name in the opinion (CII 2015). Another important element to consider when evaluating the potential effect of partner disclosure on audit

quality and cost is that the U.S. has a more litigious environment and greater pressure via PCAOB oversight compared to other countries. Therefore, it is not clear whether the positive effect of partner name disclosure on audit quality and cost as documented in prior studies can be applied to the U.S. setting (Carcello and Li 2013).

To begin, we develop our baseline specification by comparing audit quality and audit fees surrounding the Rule 3211 disclosure date for a balanced panel of U.S. companies that engage U.S. auditors.[1] Specifically, for U.S. companies with audit opinions issued between January 31, 2017 and June 29, 2017, we compare their audit quality and audit fees in the last year before implementation of partner identification with the same companies in the first year of mandatory partner identification. Our post-disclosure sample begins on the effective date of Rule 3211 and ends on June 29, 2017 to avoid contemporaneous effects from the mandatory disclosure of "other participants" in Form AP and the expanded audit report disclosures.[2]

An important consideration when evaluating the initial effect of audit partner identification is that the baseline specification may be contaminated by temporal trends in audit quality and fees or by the effect of contemporaneous events (Abadie 2005; Roberts and Whited 2013). To minimize the threat of confounding factors, we perform difference-in-differences analyses with two primary control groups: 1) companies with engagement partner disclosure at the annual shareholder meeting before Rule 3211 implementation ("early disclosers") and 2) companies with audit reports signed immediately prior to the effective date of Rule 3211 ("pseudo adopters"). For each of the difference-in-differences samples, we empirically examine

[1] Roberts and Whited (2013) refer to this research design as the single time-series difference before and after treatment. We use this terminology interchangeably with "baseline specification" to refer to these initial tests.

[2] Audit firms are required to use Form AP for two primary purposes. Beginning on January 31, 2017, auditors must disclose the name of the lead audit partner for each engagement. Beginning on June 30, 2017, auditors must also disclose other public accounting firms that took part in the audit (PCAOB 2015b). Separately, the PCAOB finalized a ruling on the expanded audit report in June 2017, which required certain changes to the audit report effective for fiscal years ending on or after December 15, 2017 (PCAOB 2017). In Online Appendix 4, we report the sensitivity of our results to expanding our analyses to companies with fiscal year ends through December 15, 2017 that do not also disclose other participants (i.e., to isolate instances with only audit partner disclosure in Form AP).

changes in our dependent variables during the pre-Rule 3211 period and incorporate several features into our research design to provide evidence consistent with the parallel trends assumption (Kausar et al. 2016; Lennox 2016; Gipper et al. 2017). If results from our baseline specification are driven by increased accountability and transparency and are not due to omitted contemporaneous factors, we expect the difference-in-differences design will reveal changes in the mandatory disclosure period that differ between treatment and control companies.

Because audit quality is not directly observable, we follow the suggestion in DeFond and Zhang (2014) to consider several measures to triangulate our inferences about audit quality. We use discretionary accruals (Kothari et al. 2005), the propensity to misstate financial statements (Dechow et al. 2011), and the likelihood of issuing an incorrect opinion on internal controls over financial reporting (ICFR) (Ge et al. 2017). In robustness tests in Section V, we also consider asymmetric timely loss recognition (Basu 1997), the propensity to meet or just beat analyst forecasts (Lim and Tan 2008), the propensity to impair assets and report positive or negative special items (Laurion et al. 2017), and the magnitude of valuation allowances for deferred tax assets (Laurion et al. 2017).

The initial evidence from our baseline specification indicates that several of the proxies for audit quality and audit fees are higher for audit opinions filed after January 31, 2017 relative to the same companies' opinions for the preceding year. However, difference-in-differences coefficients for eight of nine dimensions of audit quality are not statistically significant, suggesting that trends in quality surrounding January 31, 2017 are not convincingly attributable to the adoption of Rule 3211. The exception rests with the analysis of asymmetric timely loss recognition, which suggests that "bad news" is recognized in earnings in a timelier manner than "good news" for treatment companies relative to control companies following Rule 3211. For each of the audit quality tests with insignificant difference-in-differences coefficients, we

examine the respective confidence interval and find that the effect size does not exceed one-third of one standard deviation in the dependent variable. Finally, in our difference-in-differences tests for audit fees, we observe that fees increased approximately four percent for treatment companies relative to early discloser control companies, but do not exhibit a statistically significant difference compared to pseudo adopter control companies.

We report a variety of other robustness and sensitivity tests in Section V, including consideration of whether auditors changed their behavior in *t*-1 in anticipation of Rule 3211 disclosure in year *t*, propensity score matching to ensure covariate balancing between treatment and control groups, the use of U.K. companies as a third control group, and cross-sectional tests to determine whether our findings vary with auditors' litigation or reputation risks. Collectively, our difference-in-differences analyses indicate that any immediate impact of Rule 3211 on audit quality or fees is limited to specific dimensions of audit quality, specific control groups, and/or specific company characteristics.

Our study provides early evidence on the effect of an important and heavily debated regulatory rule in the U.S.—audit partner identification. In contrast to research that relies on the baseline specification to conclude that Rule 3211 improved audit quality, our use of a difference-in-differences research design with multiple control samples reveals that trends in quality surrounding January 31, 2017 cannot confidently be attributed to the effects of Rule 3211 for the majority of audit quality proxies examined in this study.[3] Our findings are subject to important limitations and should be used collectively with future studies examining the long-term and other potential effects of Rule 3211, which we discuss further in Section VI.

[3] For example, Burke et al. (2018) also analyze the initial effect of audit partner identification in the U.S. Their study uses a single time-series difference as well as changes models to examine whether audit quality, cost, and delay differ in the first year of required Form AP disclosure relative to the prior year. Their findings reveal that discretionary accruals and audit report lags decreased significantly and audit fees increased significantly in the first year of Form AP disclosure, which they interpret as evidence "consistent with the PCAOB argument that requiring public identification of the audit partner will enhance accountability and motivate individual audit partners to avoid negative outcomes such as poor audit quality and audit delay" (Burke et al. 2018, 2).

II. BACKGROUND AND HYPOTHESES DEVELOPMENT

PCAOB Rule 3211 Standard-Setting Process

In 2009, the PCAOB issued *Concept Release on Requiring the Engagement Partner to Sign the Audit Report*. This concept release explored the requirement for engagement partners to sign the audit report in the U.S. The PCAOB stated that adopting a partner signature requirement could improve audit quality through two channels: 1) increasing the partner's own sense of accountability, and 2) increasing transparency about who is responsible for performing the audit. This release noted that "the act of signing itself" has important psychological effects and should increase accountability similar to CEO and CFO certifications under SOX (PCAOB 2009, 6). The PCAOB received 23 comment letters on this concept release with significantly different views. For example, the Council of Institutional Investors ("CII") supported the signature requirement and stated that it would enhance audit quality (CII 2009). Alternatively, public accounting firms expressed significant concerns. They stated that accountability—and, thus, audit quality—would not improve because partners already have a strong interest in maintaining their own reputation, which is supported by a firm's system of quality control and PCAOB oversight. Related, individual partners' legal liability exposure would likely increase, and, as a result, so would audit costs (e.g., Deloitte 2009; EY 2009; Grant Thornton 2009; KPMG 2009; PwC 2009).[4] Finally, accounting firms argued that financial statement users may be given a false impression that audits are the responsibility of one individual rather than a team effort.

After assessing these comments, the PCAOB released a proposed rule in October 2011 that partners continue signing the *firm* name but identify the partner name in a separate sentence

[4] Accounting firms noted that disclosure in the auditor's report could trigger the consent requirement of Section 7 and subject the named parties to potential liability under Section 11 of the Securities Act of 1933. Firms also raised concerns about increased risk of legal liability in private actions under Section 10(b) of the Securities Exchange Act of 1934 (PCAOB 2015a). PwC (2009, 7) discusses this outcome as a potential unintended consequence of partner disclosure: "Heightened concerns about personal liability may cause engagement partners to be less willing to make professional judgments imperative to the execution of timely and cost effective, high quality audits. Such hesitancy could result in increased audit costs without a corresponding improvement in audit quality."

within the audit report. The PCAOB also recommended that partner names be included in accounting firms' annual report on Form 2 so the public could easily access partner information in one place (PCAOB 2011). A reproposed rule, released in December 2013, retained partner identification in the report but removed the Form 2 reporting requirement since this disclosure would not be timely and would impose additional costs on accounting firms (PCAOB 2013). When commenting on both proposed rules, the CII continued to strongly support the partner signature requirement while public accounting firms remained opposed to the general idea of partner identification within the auditor's report (e.g., CII 2012; Deloitte 2012; EY 2014; McGladrey 2014; PwC 2012).

On June 30, 2015, the PCAOB issued a supplemental request for comment on the use of an alternative form filed with the PCAOB ("Form AP") to report the partner's identity, rather than in the audit report itself (PCAOB 2015a). The CII expressed its disappointment with the proposal, stating that disclosure in the audit report would be more transparent because the report is "the primary vehicle by which the auditor communicates with investors" and partner information "would be available immediately upon filing with the SEC" (CII 2015, 5). Public accounting firms were generally supportive of the PCAOB's objective to increase transparency by disclosing partner identities in a separate form, despite retaining the view that Form AP disclosure would not increase partner accountability (e.g., Deloitte 2015; Grant Thornton 2015; KPMG 2015).[5]

This extensive standard-setting process culminated in the PCAOB adopting the final rule (Rule 3211) on December 15, 2015, which was subsequently approved by the SEC on May 9,

[5] For example: "We agree that the proposed approach…accomplishes the Board's goal to increase transparency to investors. We do not believe, however, that the proposed disclosure of the name of the engagement partner will increase the engagement partner's sense of accountability, improve audit quality, or result in independent public accounting firms enhancing their system of quality control (e.g., through changes to the assignment protocols for an engagement partner)" (KPMG 2015, 1).

2016. The final rule requires disclosure of the audit partner's name in Form AP, which is filed directly with the PCAOB. Registered public accounting firms must complete Form AP within 35 days of the date that the auditor's report is first included in a document filed with the SEC. Accounting firms are required to comply with this rule for audit reports issued on or after January 31, 2017 (PCAOB 2015b). Upon filing, the PCAOB disseminates Form AP data on its website.

Prior Literature on Audit Partners

Prior to the recent focus on audit partners, archival researchers generally used the audit firm or audit office as the unit of analysis. Since audit partners are ultimately responsible for engagement performance, the characteristics and incentives of individual partners likely matter in the conduct of an audit; thus, partner quality would not be uniform within the firm or the office (Lennox and Wu 2018). Consistent with this notion, several studies provide evidence that factors such as partner expertise, tenure, and style exhibit an association with the quality and pricing of audits in other countries (Chen et al. 2008; Zerni 2012; Gul et al. 2013; Knechel et al. 2015).

Of most relevance to our setting are the studies examining mandatory disclosure of audit partners' names. Carcello and Li (2013) examine this issue in the U.K. based on the passage of the Companies Act, which requires the engagement partner to sign the audit report for financial years ending in April 2009 or later. To test audit quality and fee changes following the signature requirement, Carcello and Li (2013) use a pre-post design with and without control samples of companies from the U.S. and other European countries. Their findings generally indicate that audit quality and audit fees are higher for U.K. companies after the mandatory signature requirement. On the contrary, Blay et al. (2014) analyze the partner signature mandate in the Netherlands and detect no substantial change in audit quality. Institutional differences between

these countries and the U.S., especially related to the legal regime, make these results difficult to generalize to the U.S. setting. For instance, the U.K. prohibits class action lawsuits, suggesting that litigation risk is lower in the U.K. relative to the U.S. (Frost and Pownall 1994; Seetharaman et al. 2002).

Another complicating factor involves the difference in the partner disclosure requirement between the U.S. and other countries, such as the U.K. and Netherlands. The most significant difference is that U.S. partners disclose their names in a separate Form AP filed with the PCAOB, rather than sign their names in the audit report. Because audit reports are the primary communication channel between auditors and investors, disclosing partner names in Form AP may not achieve the same potential benefits of an increased sense of accountability and transparency as would be achieved by mandatory disclosure in the auditor's report (CII 2015).

Audit Partner Identification and Audit Quality

There are two main reasons behind the argument that publicly disclosing audit partner names will increase audit quality: transparency and accountability. First, transparency provides investors with more information about key participants in the audit, including the engagement partner "who is at the center of the effort" (PCAOB 2011, 2). Existing research documents that audit partners have a specific "style" that may carry over from one audit to another (Knechel et al. 2015; Wang et al. 2015). Greater transparency regarding the background and experience of the audit partner, along with historical trends in performance, will help investors better assess the rigor of the audit process and help audit committees make auditor selection and retention decisions (PCAOB 2009; CII 2015). Moreover, transparency may induce accounting firms to make more careful decisions when assigning partners to engagements and discourage firms from allowing partners without the appropriate competencies to play a significant role in audits. These factors could lead to improvements in the quality of all engagements (PCAOB 2011, 2015b).

Second, accountability refers to an "internalized expectation of being ready to provide justifications for decisions and behavior to a third party" (King et al. 2012, 547). An important feature of accountability is identifiability such that outcomes of a decision or process can be linked to the individual (Lerner and Tetlock 1999; DeZoort et al. 2006). As described previously, the PCAOB proposal process argues that partners will feel more accountable for the work performed and opinion expressed if they are identifiable to the public. The related expectation is that audit quality will improve following partner identification; for example, the final rule states that "public disclosure will create an additional reputation risk, which should provide additional incentive to maintain a good reputation, or at least avoid a bad one" (PCAOB 2015b, 5).

In contrast to the discussion above, a threshold may exist for the overall impact of accountability on audit quality, and the current level of accountability pressure in the U.S. may already be close to this threshold (King et al. 2012). Statements from accounting firms and other parties support the argument that accountability for audit partners is already high in the U.S. due to the existing regulatory environment and accounting firms' quality control systems (e.g., Deloitte 2009; EY 2009; CAQ 2012; KPMG 2012; PwC 2012). For instance, the PCAOB's current quality control standards expose audit partners to personal sanctions and penalties. Audit partners are also subject to the threat of private litigation (EY 2009; KPMG 2009). Thus, disclosing partner names may not increase partners' already strong sense of accountability.

In addition, some argue that separate disclosure in Form AP does not provide the same sense of accountability as would have been achieved had the PCAOB required the partner's signature in the audit opinion (CII 2015). Affordance theory relates to an individual's intrinsic motivation and examines behavior based on perceptions of self-identity as defined by the environment; however, King et al. (2012) argue that this theory only provides support for potential improvements in audit quality if a partner is required to sign her name in the opinion.

The original concept release emphasized the "act of signing itself" to induce partners' behavioral changes (PCAOB 2009, 6), while accounting firms expressed significantly less concern about partners' legal liability under Form AP disclosure relative to disclosure in the audit report (CAQ 2015; KPMG 2015; PwC 2015). If litigation risk does not change, then audit partners may not have additional incentives to enhance audit quality. Thus, even if there were room for improvement in accountability in the U.S. audit environment, partner identification in Form AP may not be effective at achieving this outcome.

Based on these arguments, it is unclear whether partner identification in Form AP will result in improved audit quality. However, given that the PCAOB expects disclosure to enhance quality through transparency and accountability, we state our hypothesis in alternative form:

H1: *Audit quality improved following mandatory audit partner identification in the U.S.*

Audit Partner Identification and Audit Cost

It is important to evaluate both benefits and costs of a new regulation. A common concern of the partner identification requirement is that audit costs will increase as auditors are likely to increase effort to mitigate perceived increases in personal and reputational risk (Grant Thornton 2009; PwC 2009). Evidence from prior accountability research suggests increased pressures in an audit context may lead to more conservative risk-related judgments and expansive audit procedures, which increases audit costs but not necessarily audit effectiveness (Hoffman and Patton 1997; DeZoort et al. 2006; King et al. 2012). For instance, when an audit partner's personal reputation is at stake, she will be less willing to make the professional judgments over certain issues, and may either collect more evidence or seek more consultations (PwC 2009). The analytical model in Carcello and Santore (2015) also suggests that partners will gather more evidence and report more conservatively than is optimal once their identities are disclosed. Therefore, our second hypothesis, stated in alternative form, is:

H2: *Audit cost increased following mandatory audit partner identification in the U.S.*

III. RESEARCH DESIGN AND SAMPLE

Baseline Specification

To begin our tests of H1 and H2, we examine the single time-series difference before and after the PCAOB Rule 3211 implementation date for U.S. companies. Specifically, we identify all opinions issued by U.S. audit firms with signature dates between January 31, 2017 and June 29, 2017 to compare audit quality and audit fees with the prior year's audit of the same companies. This approach using one year before and after mandatory adoption of engagement partner disclosure mirrors the design in Carcello and Li (2013). Using this balanced sample, we estimate the following equations to test our hypotheses:

$$AUDIT\_QUALITY_{it} = \alpha_0 + \alpha_1 POST_t + \sum \alpha_k Controls_{kit} + Company\ Fixed\ Effects_i + \varepsilon_{it} \quad (1)$$

$$AUDIT\_FEES_{it} = \beta_0 + \beta_1 POST_t + \sum \beta_k Controls_{kit} + Company\ Fixed\ Effects_i + \varepsilon_{it} \quad (2)$$

where *POST* is an indicator variable equal to one if the fiscal year is the first year that mandatory audit partner identification is required in Form AP (PCAOB Rule 3211), and zero otherwise. The various proxies for audit quality used in equation (1) and the control variables for each model are discussed below. Because each of these proxies represents actual or suspected *failures* in audit quality, a negative coefficient estimate for α_1 suggests the PCAOB's rule created the intended effect of increasing accountability of audit partners. For equation (2), the dependent variable (*AUDIT_FEES*) equals the natural logarithm of audit fees.

Difference-in-Differences Design

A significant concern about the analysis in equations (1) and (2) is that our baseline specification may be contaminated by temporal trends in audit quality and fees or by the effect of other events that occurred between the pre- and post-periods (Abadie 2005; Atanasov and Black 2016). An alternative approach to mitigate this concern is to compare U.S. audit engagements

subjected to partner disclosure for the first time to a control group of audit engagements that do not experience the same treatment. To do so, we employ a difference-in-differences design using two control groups discussed below. Each control sample has its own strengths and weaknesses; however, we expect the collective findings will allow us to separate the effects of Rule 3211 from other confounding factors.

Early Discloser Sample

In the first analysis, we identify a control group that allows for a comparison of two sets of U.S.-adopting firms over the same post-implementation period, alleviating concerns about comparing macroeconomic changes across different periods. We exploit a unique setting in which companies disclose the name of the audit engagement partner at the annual shareholders' meeting and publish this information on their company websites. Since ratification of the external audit firm is a topic at this meeting, the engagement partner may choose to attend and is available to answer shareholder questions (PCAOB 2009). This "early discloser" control sample is motivated by comments from respondents to the initial PCAOB proposal: "Some have noted that the identity of the engagement partner generally is not a secret and that regulators and others may easily determine who served in that role on a given audit" (PCAOB 2009, 6).[6] We argue that companies disclosing the partner identity in this manner offer a within-period control sample because, similar to Form AP, the disclosure of the audit partner occurs after the audit report date and the information is accessible for those seeking to find it (company website versus PCAOB website). Thus, the behavior of these partners should already reflect the effect of being publicly disclosed. We can then compare changes pre- and post-adoption of Rule 3211 between early adopters and non-early adopters to control for contemporaneous macroeconomic factors that

[6] A retired Deloitte partner reiterated this point: "Finally, having been an audit partner on public companies I attended many annual shareholder meetings where I was introduced; attending shareholders not only had my name but could see my face." (Fuehrmeyer 2011, 2).

might be influencing changes in audit quality and audit fees at the same time as the Rule 3211 adoption.[7]

We collect data from company websites to identify companies that disclose the identity of audit engagement partners during the period prior to PCAOB Rule 3211. Since our approach requires significant hand collection, we limit our sample to S&P 1500 companies. For each company in this sample, we examine the Investors/Investor Relations website to locate information related to the annual meeting of shareholders. If a company provides detailed annual meeting materials on its website (often an audio or video webcast and/or a detailed transcript), we review all available materials to determine whether the audit engagement partner is identified. Appendix B provides a few examples of these disclosures. Approximately 15 percent of S&P 1500 companies disclose archived information about the annual meeting on their websites, and 76 percent of these companies identify the audit partner in the archived materials.[8] Our review of the data over two years suggests that the decision to disclose the partner at the annual meeting is relatively sticky (i.e., 89 percent continue between 2015 and 2016); thus, it is reasonable to expect that partners anticipate future disclosure at the annual meeting at the time they work on the audit.

If the PCAOB rule achieves its intended purpose, we expect the rule to have a greater accountability impact on the group of partners that were *not* previously identified to the public in annual meeting archives on companies' websites. Therefore, audit partners identified before (in annual meeting archives) *and* after (in Form AP) Rule 3211 represent our control group whereas

[7] Another avenue for publicly identifying audit partners in the pre-Rule 3211 period is through the SEC's comment letter correspondence. We choose not to use this group of early disclosers because companies are only required to be reviewed at least once every three years and only six percent of companies receiving comment letters copy the audit partner in their response letters to the SEC (Laurion et al. 2017).

[8] For the 2016 annual meetings, we identify 218 companies that disclose archived information, 167 of which name the audit partner (untabulated). We began this data collection effort in 2015 for the 2015 annual meetings and learned that companies provide these materials for varying lengths of time on their websites (e.g., one month, one quarter, one year); as a result, we tracked when the annual meeting would likely occur for each company for the 2016 annual meetings and collected the data within a short period of time following the annual meeting date.

partners identified only after Rule 3211 (in Form AP) represent our treatment group. The treatment and control samples are limited to S&P 1500 companies with audit reports issued between January 31 and June 29, 2017 for the post-period with a corresponding observation from the pre-period. Using this balanced panel, we estimate the following difference-in-differences regressions:

$$AUDIT\_QUALITY_{it} = \alpha_0 + \alpha_1 POST_t + \alpha_2 UNIDENTIFIED_i + \alpha_3 POST_t * UNIDENTIFIED_i + \sum \alpha_k Controls_{kit} + Company\ Fixed\ Effects_i + \varepsilon_{it} \quad (3)$$

$$AUDIT\_FEES_{it} = \beta_0 + \beta_1 POST_t + \beta_2 UNIDENTIFIED_i + \beta_3 POST_t * UNIDENTIFIED_i + \sum \beta_k Controls_{kit} + Company\ Fixed\ Effects_i + \varepsilon_{it} \quad (4)$$

where *POST* is an indicator variable equal to one if the fiscal year is the first year that mandatory audit partner identification is required in Form AP (PCAOB Rule 3211), and zero otherwise. *UNIDENTIFIED* is an indicator variable equal to one if the audit partner identity was *not* disclosed prior to PCAOB Rule 3211, and zero otherwise. The coefficient estimates for α_3 and β_3 capture whether audit quality and costs increased following adoption of Rule 3211 for partners that were not already identified to the public, relative to the change over the same period for partners that were already identified.

Pseudo Adopter Sample

To relax our assumption that audit partner disclosure in the annual meeting is as effective as audit partner disclosure in Form AP, we also perform a difference-in-differences analysis using a control group of companies with an audit report issued immediately prior to January 31, 2017 (a pseudo-post period). This test is motivated by regression discontinuity design, which examines the effect of a shock that creates a discontinuity in the data and separates companies into treatment and control groups (Atanasov and Black 2016). Consistent with the design in Christensen et al. (2013), we create a "pseudo adopter" control sample using companies that file immediately before Rule 3211 implementation. Because these companies lack Form AP

disclosures based on the report issuance dates but still have a report issued within a similar time period as the treatment group, they can be used to control for general macroeconomic changes pre- and post-Rule 3211. This control sample also has the benefit of being U.S. companies, avoiding concerns about cross-country comparisons.[9]

Similar to equations (3) and (4), we perform a difference-in-differences analysis between our treatment group (i.e., U.S. audit opinions issued between January 31, 2017 and June 29, 2017) and the pseudo adopter control group (i.e., U.S. audit opinions issued between July 1, 2016 and January 30, 2017). For each treatment and control group, we use balanced panels where we retain the "post" period and "pre" period for each audited company. We estimate the following difference-in-differences regressions using these samples:

$$AUDIT\_QUALITY_{it} = \alpha_0 + \alpha_1 POST_t + \alpha_2 TREATMENT_i + \alpha_3 POST_t * TREATMENT_i + \sum \alpha_k Controls_{kit} + Company\ Fixed\ Effects_i + \varepsilon_{it} \quad (5)$$

$$AUDIT\_FEES_{it} = \beta_0 + \beta_1 POST_t + \beta_2 TREATMENT_i + \beta_3 POST_t * TREATMENT_i + \sum \beta_k Controls_{kit} + Company\ Fixed\ Effects_i + \varepsilon_{it} \quad (6)$$

where *POST* is an indicator variable equal to one if the fiscal year is the first year that mandatory audit partner identification is required in Rule 3211 for the treatment group, or the audit report issuance is in the immediate period prior to January 31, 2017 for the control group, and zero otherwise. *TREATMENT* is an indicator variable equal to one for the group subject to Rule 3211, and zero otherwise. The coefficient estimates for α_3 and β_3 capture whether audit quality and costs increased following adoption of Rule 3211, relative to the general trends in U.S. audit quality and fees that can be observed in the pseudo adopter sample.

Proxies for Audit Quality

[9] We recognize that there could be systematic industry differences between our pseudo adopter and Form AP adopter companies due to the differences in fiscal year-ends. Upon examining the distribution of two-digit SIC codes for each group (untabulated), we identify that the significant industry differences only relate to regulated industries. As a result, we evaluate our results after dropping these regulated industries as discussed in Online Appendix 4.

Since actual audit quality is unobservable, we use several different proxies based on the recommendations in DeFond and Zhang (2014). First, we estimate performance-matched discretionary accruals from a cross-sectional modified Jones model (Dechow et al. 1995; Kothari et al. 2005; Carcello and Li 2013; Gutierrez et al. 2018). We calculate the difference between each company's residual from this model and the residual for a company with the closest return on assets in the same two-digit SIC industry and year. The absolute value of this difference represents our measure of discretionary accruals (*ABSDA*), which captures income-increasing and income-decreasing earnings manipulation. This proxy is supported by research showing that discretionary accruals are associated with fraud and AAERs (Jones et al. 2008; Dechow et al. 2011).

Ideally, we would use misstatements, as revealed through subsequent restatement announcements, as our second proxy for audit quality because it represents a clear measure of the auditor's failure to detect and report a material misstatement during the examined periods (DeFond and Zhang 2014); however, on average, two to three years after a misstatement are necessary to reveal the misstated period.[10] Instead, we use the F-score (*FSCORE*) developed in Dechow et al. (2011) as our second proxy for audit quality because it provides a "signal of the likelihood of earnings management or misstatement" (p. 18) and can be used as a "supplementary measure to discretionary accruals for identifying low quality-earnings firms" (p. 77). *FSCORE* is the predicted probability of accounting misstatements from model (1) in Dechow et al. (2011), scaled by the unconditional probability of having a misstatement. Several recent studies employ the F-score in a similar manner (Ge et al. 2011; McGuire et al. 2012; Fang et al. 2016; Bradley et al. 2017).

[10] Using observations in the *ABSDA* sample, the mean (75th percentile) for the number of days between the restatement announcement date and the "Restatement Period Begin" date in Audit Analytics is 786 days (1,156 days).

As a third proxy, we consider recent literature that uses predictive models for material weaknesses to measure the quality of the ICFR audit opinion. Similar to *FSCORE*, we use the coefficients from equation (2) in Ge et al. (2017) to estimate the probability of a material weakness. Following Ge et al. (2017) and Bhaskar et al. (2018), we consider the unconditional mean of material weaknesses in our sample of audit opinions on ICFR and classify observations as 'suspect' material weakness observations if they are in the top decile of predicted probabilities for a material weakness in a given fiscal year. The resulting measure, *INCORRECT_MW*, is an indicator variable set equal to one if the suspect classification and actual audit opinion disclosure for material weaknesses are not equal, and zero otherwise.[11]

Control Variables

We follow prior research to determine the relevant control variables for our models. For models using *ABSDA* and *FSCORE*, we follow Reichelt and Wang (2010) and Carcello and Li (2013) and include control variables that capture company size (*SIZE*), financial condition of the company (*ROA, LEVERAGE, LOSS, MB*), the company's capacity to manage earnings (*LAG_ACCR, CFO, VOLATILITY*), the size of the audit firm (*BIG4*), whether the company changed auditors (*AUDITOR_CHG*), restatements announced during the fiscal year (*RESTATE*), material weaknesses (*MW*), and going concern opinions (*GC*). For models using *INCORRECT_MW* as the dependent variable, we include the same control variables as the *ABDSA* tests except that we omit *MW.* Finally, we follow Carcello and Li (2013) for our audit fee model and include the same controls from the *ABSDA* model as well as additional variables that capture accounts associated with more auditor effort (*RECEIVABLE, INVENTORY,*

[11] Ge et al. (2017) examine ICFR quality for both 404(a) and 404(b) reporters with samples that experience up to 20 percent material weakness rates in their sample period; as a result, they use the top quintile of the predicted probability of a material weakness to identify suspect observations. We limit our tests to 404(b) audit opinions to draw inferences about audit quality, as opposed to including 404(a) opinions that are issued by management and not subject to audit procedures. We use the top decile since our sample period experience up to 10 percent material weakness rates. In Online Appendix 4, we discuss alternative approaches to identify 'suspect' observations.

FOREIGN) and audits performed during peak busy season (*BUSY*). Variable definitions are included in Appendix A.

We estimate equations (1) through (6) using a company fixed-effect estimator.[12] Company fixed effect models control for potential time-invariant omitted variables specific to the company being audited (Hope et al. 2017). All models are estimated using robust standard errors clustered by company. We perform a variety of sensitivity tests using alternative measures and research designs, which we discuss in Section V.

Sample Selection

Table 1 summarizes our sample selection process. To construct our primary balanced sample, we begin with Audit Analytics data for U.S. companies engaging U.S. auditors with audit report signature dates between January 31, 2017 and June 29, 2017 (inclusive). We restrict our sample to companies with total assets greater than or equal to $1 million in years *t*-2, *t*-1, and *t*, where *t* represents the first year of mandatory partner disclosure. We then merge with Compustat and impose the requirement that data for all variables in each model are available for *t*-2, *t*-1, and *t*. Requiring data for three periods allows us to provide a consistent set of inferences among our baseline specification, a strict changes analysis (discussed in Section IV), and our difference-in-differences specification. These sample selection procedures result in samples ranging from 2,203 to 3,226 companies for our balanced panels with pre- and post-Rule 3211 data.

The sample size for the baseline specification is double the number of companies in each sample (i.e., one pre- and one post-year). When estimating the difference-in-differences tests for

[12] For binary dependent variables, we estimate a linear probability model (LPM) as opposed to a non-linear model because the latter would drop observations for all companies where the binary variable is equal to one or equal to zero in both years. LPM is effective in estimating average partial effects for dichotomous outcome variables (Wooldridge 2010, 562). Interactions also have a more intuitive interpretation in linear models, which is important for the interpretation for our analyses using difference-in-differences regressions.

our hand-collected sample of early disclosers, we limit these samples to only those companies included in the S&P 1500 in 2015 with available data for our models. For tests using the pseudo adopter sample, we supplement the baseline samples with a control sample of companies constructed using the same method as above, but where the post-period is based on signature dates between July 1, 2016 and January 30, 2017 (inclusive).

IV. BASELINE SPECIFICATION RESULTS

Descriptive Statistics

Table 2 reports descriptive statistics for the full sample based on the baseline specification in equations (1) and (2). We report mean values for each variable in the pre-disclosure period (*POST* = 0) and the post-disclosure period (*POST* = 1) along with a test of differences in means. The mean values are presented separately for each balanced panel with available data for the tests related to the respective audit quality proxies and audit fees. We do not find a significant univariate difference between periods for any of our proxies of audit quality ($p > 0.10$).

Regression Results

In Table 3, regression results for the baseline specification show that *POST* is significantly associated with several dimensions of audit quality. The negative associations with *ABSDA* and *FSCORE* suggest lower discretionary accruals and a reduction in the propensity to misstate, respectively. The positive and significant associations with *INCORRECT_MW* and *AUDIT_FEES* suggest an increased propensity to issue an incorrect 404(b) opinion and higher audit fees, respectively.[13] Consistent with Burke et al. (2018), these results indicate that several of the proxies for audit quality and cost of audit services increased in the first year of Rule 3211

[13] The adjusted R-squared value for the audit fee model is high due to the inclusion of company fixed effects. When we replace company fixed effects with industry fixed effects in the audit fee model, we find the adjusted R-squared declines to 0.870, which is comparable to prior studies using a similar specification (e.g. Carcello and Li 2013).

relative to the period just prior to this disclosure requirement.

We also perform a strict change analysis using the baseline sample that compares changes in the audit quality proxies and audit fees from *t*-2 to *t*-1 with changes from *t*-1 to *t*. All other variables in the model are also transformed to change variables, and company fixed effects are excluded. Contrary to the baseline results, we find that *POST* is insignificant in three of the four regressions (untabulated), suggesting that collectively, changes in audit quality and fees between *t*-1 and *t* are not statistically different from temporal trends captured in the change from *t*-2 to *t*-1.

V. DIFFERENCE-IN-DIFFERENCES RESULTS

Descriptive Statistics

Due to the large volume of data, we present descriptive statistics for the final samples used in each difference-in-difference analysis in Online Appendix 1 (early disclosers) and Online Appendix 2 (pseudo adopters).

Parallel Trends Assumption

The identifying assumption in our difference-in-differences regressions is that treatment and control groups exhibit parallel trends in audit quality and cost in the period leading up to the treatment effect, and, in the absence of treatment, the average outcomes for these two groups would continue following parallel trends (Abadie 2005). We follow prior research and examine trends in our proxies for audit quality and audit fees in the period prior to Rule 3211 (Kausar et al. 2016; Lennox 2016; Gipper et al. 2017). Specifically, we calculate changes in each dependent variable from year *t-2* to *t-1* (where *t* is the first post-Rule 3211 period) for each treatment and control group and then test whether a significant difference exists between groups. The results (untabulated) show that the pre-period trends in audit quality and cost are indistinguishable between each treatment and control sample (i.e., none of the univariate differences have a p-

value lower than 0.20).

The remaining question, which we cannot observe, is whether *post*-treatment trends in the outcome variables would have continued to be parallel for the treatment and control groups in absence of Rule 3211. The concern is that unobserved confounders may cause trends in the outcome variables to diverge in the post-period irrespective of the new regulation. We incorporate several features in our research design to reduce this concern. For example, we use balanced panels so that changes in sample composition do not affect our findings. Our models also include company fixed effects and time-varying controls to reduce the likelihood that our inferences are confounded by company-specific characteristics. In addition, we perform robustness tests based on propensity score matched samples (see Online Appendix 4).

Finally, we consider whether other auditing or accounting standards became effective in the U.S. at the same time as PCAOB Rule 3211 (or during our "post" period for our pseudo adopter sample) and, if so, whether these events might have led to differential changes in audit quality or fees between our treatment and control groups. While we did not identify other PCAOB auditing standards that were adopted at the same time, several accounting standard updates promulgated by the FASB became effective around our sample period. Upon review of these standards, we identify the concurrent adoption of ASU No. 2014-15 related to management's disclosure of going concern uncertainties. Based on this standard's potential impact on auditors' assessment of going concern, we do not examine going concern opinions as a proxy for audit quality. The remaining standards relate to specific accounts and transactions that would not be expected to cause widespread differences in audit quality or fees between our treatment and control groups.

Regression Results

Table 4 presents our difference-in-differences regression results. In each test, the main

effect for the treatment group (*UNIDENTIFIED* or *TREATMENT*) is subsumed by the inclusion of company fixed effects in the model. Panel A (B) of Table 4 reports our findings using the control group of early disclosers (pseudo adopters). The coefficients on the difference-in-differences interaction term in *ABSDA, FSCORE,* and *INCORRECT_MW* are statistically insignificant in both Panels A and B ($p > 0.10$). Related to the cost of audits *(AUDIT_FEES*), the coefficients on the difference-in-differences interaction term is positive and statistically significant in Panel A ($p < 0.01$) but insignificant in Panel B ($p > 0.10$). Collectively, these results raise questions as to whether the changes observed in the baseline specification can be attributed to Rule 3211. In Section VI, we discuss the implications of these findings when taken together with the description of confidence intervals and a series of robustness and cross-sectional analyses included below.

Hoenig and Heisey (2001) suggest that authors with insignificant results also consider confidence intervals when interpreting a failure to reject the null hypothesis.[14] Therefore, we examine the magnitude of the confidence intervals for the statistically insignificant coefficients in our difference-in-differences tests. In all cases, we find (untabulated) that the upper and lower bounds of these confidence intervals indicate that the potential effect size does not exceed one-third of one standard deviation in the respective dependent variable.[15] For example, the 95% confidence interval for the interaction term in *ABSDA* in Table 4, Panel A, ranges from -0.019 to

[14] "It is well known among applied scientists that a lack of impact or effect is not sufficiently established by a failure to demonstrate statistical significance. A failure to reject the null hypothesis of no effect may be the result of low statistical power when an important effect actually exists and the null hypothesis of no effect is in fact false" (Hoenig and Heisey 2001, 1). Hoenig and Heisey (2001) describe that there is no appropriate statistical test to demonstrate sufficient power to reject a null hypothesis. Rather, researchers should consider "the breadth of the [confidence] interval," which "tells us how confident we can be of the true state of nature being close to the null" (Hoenig and Heisey 2001, 4). Cready et al. (2018) and Stone (2018) also emphasize that authors should examine standard errors or confidence intervals when a study is based on null hypothesis significance testing.

[15] We recognize that the interpretation of whether these confidence intervals contain potentially significant effects will vary with the prior opinions of the reader. Readers can use these confidence intervals when comparing our findings with future studies since Hoenig and Heisey (2001) and Stone (2018) suggest an evaluation across multiple studies should converge to the 'true' effect when there are concerns about power for insignificant results (e.g., through meta-analysis).

0.014. The standard deviation of *ABSDA* for the same sample, reported in Online Appendix 1, is 0.086. This comparison suggests that the potential effect size ranges from a 0.221 standard deviation decrease to a 0.163 standard deviation increase in *ABSDA*.[16]

To provide another perspective, the coefficient on the difference-in-differences test involving discretionary accruals following U.K. partner identification in the Carcello and Li (2013, Table 4) is -0.041 ($p < 0.01$, $N = 2{,}104$). Thus, our confidence intervals, derived from similar or larger sample sizes, suggest that if an effect does exist though not statistically significant in our sample, the potential difference-in-differences effect of mandatory partner identification on audit quality is likely substantially lower in the U.S. relative to the documented U.K. effect.[17]

Alternative Control Sample

Because each of our control groups has its own strengths and weaknesses, we perform our difference-in-differences tests using an alternative control sample of U.K. companies. Because auditors in the U.K. have been reporting partner identities since 2009, any changes in audit quality or fees surrounding the Rule 3211 implementation date should not be related to partner disclosure for this control sample. As noted previously, the legal regime and regulatory environment for auditors differs between the U.K. and the U.S. However, as long as the U.K. does not experience a separate shock to litigation/regulatory risk within our sample period, the level of litigation/regulatory risk in the U.K. is constant over the sample period and would not affect the difference-in-differences test.

To perform our tests, we create a balanced sample of U.K. companies that are listed on

[16] Following Gutierrez et al. (2018), to address concerns about small sample sizes, we also consider confidence intervals based on standard errors estimated with bootstrapping with 1,000 replications. We find nearly identical 95% confidence intervals.

[17] We also note that our sample sizes are similar to, or larger than, the sample used in Ahmed et al. (2013), who provides preliminary evidence on the significant effect of IFRS adoption using difference-in-differences analyses with a sample size of 2,180 in a discretionary accruals test.

the London Stock Exchange (LSE) using data from Worldscope and Compustat Global. Our variables for the U.K. sample are constructed using the same conventions as those in the U.S. data, except for audit fees and incorrect material weaknesses. Since the separation of audit and non-audit fees is not readily available for U.K. companies in our sample period, we substitute *AUDIT_FEES* with the natural logarithm of total fees (*TOTAL_FEES*) for both the U.S. and U.K. companies. We drop *RESTATE*, *MW*, and *GC* from the models and omit tests for *INCORRECT_MW* since these variables represent rare events in the U.K. and are not readily available in the datasets. After combining the U.S. and U.K. samples, we create an indicator variable (*US*) equal to one for U.S. companies whose auditors are subject to PCAOB Rule 3211, and zero otherwise. The final sample sizes for each multivariate difference-in-differences test are comparable to the pseudo adopter sample sizes: 7,196 (*ABSDA*), 7,240 (*FSCORE*), and 7,274 (*TOTAL_FEES*).[18] We consider the parallel trends assumption before estimating the regressions and find that all pre-period changes between U.S. and U.K. companies are indistinguishable, with the exception of *ABSDA*. Because *ABSDA* fails the parallel trends test, we instead use *ABSDA* (*ROA Control*), as defined in Online Appendix 4, since changes in this variable in the pre-period are insignificantly different between the treatment and control samples. The regression results reveal (untabulated) that the coefficient on *POST*US* is insignificant in all models, suggesting that we fail to reject the null hypothesis in H1 and H2 when using U.K. firms as the control sample.

Alternative Proxies for Audit Quality

In this section, we consider whether our difference-in-differences results are sensitive to

[18] Our sample begins with 1,339 companies listed in Worldscope on Datastream as being active on the LSE that are primary quotes and are classified as "major" securities. We obtain financial information from Compustat Global and total fees paid to the auditor and foreign sales data from Worldscope (retrieved in May 2018). Because these databases lack audit opinion dates, we assign observations with a "datadate" on or after December 31, 2016 (but on or before April 30, 2017) to the post-period and the prior year's observation as the pre-period. We assume that reports on or before April 30, 2017 will be issued by June 29, 2017. Once we require a lagged audit opinion for each observation, this restricts our U.K. sample to 831 potential companies.

the choice of audit quality proxies. First, we consider asymmetric timely loss recognition as measured using the Basu (1997) model, which indicates whether "bad news" is recognized in earnings in a timelier manner than "good news." Similar to the design in Ahmed et al. (2013), we extend Basu's (1997) model to include the appropriate main and interactive effects for *POST.* The dependent variable, *EARN*, is earnings divided by the lagged market value of equity. *RET* is the 12-month cumulative return ending three months after the fiscal year-end. *NEG* is an indicator variable equal to one if *RET* is negative, and zero otherwise. We also include the control variables and interaction terms (*SIZE_LAG, MB_LAG, LEVERAGE_LAG*) commonly used in related research designs (Ettredge et al. 2012; Ahmed et al. 2013). A company is considered to exhibit greater conditional conservatism if its earnings are more sensitive to bad news than to good news (*RET*NEG*). Thus, our variable of interest is *POST*UNIDENTIFIED(TREATMENT)*RET*NEG* in the difference-in-differences specification. A positive coefficient suggests greater conservatism following Rule 3211 relative to each control group in the difference-in-differences analyses.[19]

Second, we measure the likelihood of a company meeting or just beating its earnings target. A large body of evidence documents discontinuities in earnings distributions, suggesting that earnings are managed to meet prominent benchmarks (Graham et al. 2005; Burgstahler and Chuk 2017). We create an indicator variable, *MEET*, equal to one if a company's actual earnings per share minus the latest analysts' earnings per share forecast is within zero to one cent (both inclusive), and zero otherwise (Lim and Tan 2008; Reichelt and Wang 2010; Reid et al. 2018). When estimating this model, we include the same control variables as the *ABSDA* model but also control for analyst coverage (*NUM_ANALYST*) and forecast dispersion (*DISPERSION*).

[19] A large body of research uses the Basu (1997) model but substantial debate exists as to whether this model adequately captures asymmetric timely loss recognition (Dietrich et al. 2007; Givoly et al. 2007; Patatoukas and Thomas 2011; Ball et al. 2013a, 2013b; Dutta and Patatoukas 2017). Following Ball et al. (2013a), we include company fixed effects to reduce the likelihood of spurious inferences when using the Basu (1997) approach.

Finally, following Laurion et al. (2017), we consider impairments, special items, and valuation allowances as alternative dependent variables to capture audit areas that are subject to greater professional judgment. *IMPAIR* is an indicator variable set equal to one if the company discloses an impairment or write-down, and zero otherwise. *SPI_POS* (*SPI_NEG*) is an indicator variable set equal to one if the company discloses positive (negative) special items, and zero otherwise. *DTA_ALLOW* is the percentage of gross deferred tax assets (DTA) with a valuation allowance. For our models, we rely on the significant control variables established in Laurion et al. (2017), which are detailed in Online Appendix 3. In the *DTA_ALLOW* model, we also control for the deferred tax liability to deferred tax asset ratio *(DTA/DTL*), which proxies for the future realizability of DTAs (Schrand and Wong 2003; Christensen et al. 2008).

Prior to performing the regressions, we consider the parallel trends assumption (untabulated) and find that the changes in these dependent variables from *t*-2 to *t*-1 are not statistically different between treatment and control groups with the exception of *SPI_NEG*.[20] Therefore, the findings for this variable should be interpreted with caution. We report the results for each difference-in-differences test in Table 5. For brevity, we suppress the coefficients on control variables in Table 5 but report full results in Online Appendix 3. As shown in Panels A and B, the difference-in-differences coefficient (*POST*UNIDENTIFIED* or *POST*TREATMENT*) is insignificant in all regressions, with the exception of the interaction on the Basu coefficient, indicating that for the majority of audit quality proxies examined in this study, we fail to reject the null hypothesis in H1. The positive coefficient on *POST*UNIDENTIFIED*RET*NEG (POST*TREATMENT*RET*NEG*) seems to suggest that Rule 3211 contributed to more timely loss recognition relative to the early discloser (pseudo

[20] The Basu (1997) timely loss recognition model is excluded from the parallel trends analysis since inferences for audit quality are inferred from variation in the coefficient on *RET*NEG*, which is not observable in univariate statistics for changes.

adopter) control group.

Sensitivity Tests

To assess the robustness of our difference-in-differences findings, we employ 22 different research design modifications (including propensity score matching) and alternative variable measurements. We summarize the evidence from these tests in Online Appendix 4 for the early discloser and pseudo adopter samples. The collective results from these alternative specifications are consistent with the originally tabulated results from Table 4.

Cross-Sectional Tests

To further understand the sensitivity of our results, we conduct cross-sectional tests for equations (3) through (6). To develop predictable and plausible cross-sectional tests, we identify client and auditor characteristics associated with litigation and reputation risk since auditors are incentivized to provide higher quality audits in settings with a greater likelihood of litigation or reputational damage (DeFond and Zhang 2014). For client characteristics, we consider client size (*SIZE*), clients reporting a material weakness (*MW*) which increases the risk of material misstatement, clients operating in litigious industries or exhibiting greater *ex ante* litigation risk following Francis et al. (1994) and Kim and Skinner (2012) (*LIT_SCORE*), client financial distress (*LOSS, BANKRUPTCY_RISK, GC*), and outside monitoring and potential scrutiny on the audit via analyst coverage (*NUM_ANALYST*). For auditor characteristics, we consider the size of the audit firm (*BIG4*), auditor industry expertise following Reichelt and Wang (2010) (*INDUSTRY_EXPERT*), auditor tenure (*TENURE*), and audit fee pressure following Ettredge et al. (2014) (*FEE_PRESSURE*). All variables are defined in Appendix A.

We modify each of our difference-in-differences tests to include an additional interaction with each of these cross-sectional variables (*VARIABLE*), making sure to include all main effects

and lower-order interactions.[21] For brevity, we only report the coefficient on our variable of interest (i.e., the additional interaction with *VARIABLE*) in Table 6, Panel A (early disclosers) and Panel B (pseudo adopters), based on separately estimated regressions for each cross-sectional variable. As reported in Table 6, we find a few instances of statistically significant relations in the cross-sectional tests. However, the results are not consistent across all proxies for audit quality and thus, only provide some evidence that partners respond to Rule 3211 with lower discretionary accruals when the risk of material misstatement is higher (*MW*) or litigation risk is higher (*LIT_SCORE*). The results suggest that the coefficients in our primary difference-in-differences analyses appear to be representative of the sample across many potential dimensions of client and auditor characteristics, and only differ within the sample in a few specific situations.

VI. DISCUSSION AND CONCLUSION

We examine whether PCAOB Rule 3211 requiring audit partner identification in Form AP resulted in improvements to audit quality and increases in audit costs in the first year of adoption. To analyze the effects of this regulatory change, we develop a balanced panel research design using a single time-series difference before and after Rule 3211 as well as difference-in-differences analyses with early disclosers and pseudo adopters serving as separate control groups. Overall, we are unable to detect a significant change in audit quality attributable to Rule 3211 when evaluated along the dimensions of discretionary accruals (*ABSDA*), the propensity to misstate (*FSCORE*), and the likelihood of issuing an incorrect material weakness opinion (*INCORRECT_MW*). These results are consistent across additional proxies for audit quality as

[21] Because we use a firm fixed effect specification, it is important for both the pre-Rule 3211 observation and the post-Rule 3211 observation to be in the same *VARIABLE = 1* OR *VARIABLE* = 0 specification. Therefore, we measure these variables for the post-Rule 3211 period and impose that value on the same company's observation for the pre-Rule 3211 period. For continuous variables *SIZE*, *LIT_SCORE*, *TENURE*, and *NUM_ANALYST*, we create an indicator variable for whether the observation's value for the variable is above the respective sample's median value.

well as several alternative specifications, control groups, and company characteristics. However, it should be noted that we do observe an increase in timely loss recognition following Rule 3211, and while audit quality proxied for by *ABSDA* did not improve *on average*, it did improve for specific types of engagements with weak internal controls and higher risk of litigation.

Our results also indicate that audit fees increased approximately four percent following Rule 3211 when comparing S&P 1500 companies that did *not* previously disclose the partner name in publicly available annual meeting materials with our S&P 1500 early discloser control group. Although this finding is robust to several alternative specifications, we fail to find evidence of a fee increase when comparing treatment companies to the pseudo adopter or U.K. control groups. Collectively, many of our difference-in-differences tests contrast the findings in the baseline specification, suggesting that they represent macroeconomic trends not convincingly attributable to the adoption of Rule 3211.

Our failure to find robust and consistent evidence of an on-average immediate change in audit quality or fees following U.S. partner identification may be due to several factors, statistical and non-statistical. Statistically, our inability to detect a change post-Rule 3211 relative to control groups could be due to lack of statistical power. However, our statistically significant results in the baseline specification, in several tests for audit fees and timely loss recognition using similar difference-in-differences samples, and in specific cross-sectional tests help to alleviate the concern that our insignificant results are driven by small sample sizes. Nevertheless, we cannot fully eliminate this possibility, so we also discuss the potential impact from the perspective of confidence intervals. The upper and lower bounds of the 95 percent confidence intervals suggest that the potential effect sizes do not exceed one-third of a one standard deviation change in the respective dependent variables.

Unrelated to statistics, at least two factors may be driving our failure to find evidence of an increase in audit quality following partner identification. First, accounting firms argued that partner accountability was already sufficiently high prior to mandatory disclosure, such that partner identification would not induce additional improvements to audit quality. Second, the final adoption of Rule 3211 required audit partner disclosure in Form AP, which reduces partners' legal liability and removes psychological effects specific to the act of signing. As a result, disclosure in Form AP may not pervasively affect partners' sense of accountability as the PCAOB originally intended. We cannot disentangle or completely eliminate these alternative explanations using archival data.

Since our research focuses on the initial adoption of mandatory audit partner identification in the U.S. and because current data limitations prevent us from investigating restatements and PCAOB inspection results, we recognize that future research is necessary to investigate the long-term effects of this regulation. One of the biggest changes with Form AP is that each audit partner's entire public company portfolio will be easily accessible to all interested parties. Therefore, important unanswered questions relate to whether this increased transparency informs decisions by investors and audit committees or influences audit partners' behaviors on other clients.[22] For example, will audit committees seek out (or avoid) engagement partners who are viewed as performing consistently high-quality (low-quality) audits? Does increased transparency induce this partner to perform higher quality or more conservative audits for other public clients following negative events such as restatements? Will audit partner identification

[22] In a recent experimental study, Lambert et al. (2018) find that investors are less likely to invest in a company when the partner is linked to another client with a restatement. The PCAOB also highlights these potential long-term effects in the final rule: "As this information accumulates and is aggregated with other publicly available information…[t]his will allow interested parties to compile information about the engagement partner, such as whether the partner is associated with restatements of financial statements or has been the subject of public disciplinary proceedings, as well as whether he or she has experience as an engagement partner auditing issuers of a particular size or in a particular industry" (PCAOB 2015b, 3). However, early archival evidence finds no significant trading activity in the days surrounding PCAOB Form AP disclosure, even in cases of a change in audit partner in the second year of mandatory disclosure (Doxey et al. 2018).

affect public accounting firms' ability to retain high-performing employees in the profession? We leave these questions to future research. In the meantime, our paper provides initial evidence that any immediate changes in audit quality or audit fees attributable to Rule 3211 are limited to specific dimensions of audit quality, specific control groups, and/or specific company characteristics. As noted by Stone (2018, 108), "scientific 'truth' exists only in the accumulation of multiple investigations, ideally across methods and investigators." Our study—in conjunction with future research—should help inform regulators and other interested parties as they evaluate the impact of this ruling in the years to come.

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TABLE 1
Sample Selection

Audit opinions from Audit Analytics with a signature date between January 31, 2017 and June 29, 2017 (year *t*) for U.S. companies engaging U.S. auditors [a]	7,817
Less: When a CIK has more than one observation, keep only latest fiscal year end	(50)
Less: Missing Compustat identifiers and lagged audit opinions	(3,649)
Less: Total assets less than $1 million in *t*, *t*-1, or *t*-2	(182)
Potential companies in sample	3,936
Less: Missing data for variables in *ABSDA* models (require data for *t*, *t*-1, and *t*-2)	(751)
Companies in *ABSDA* sample	3,185
Less: Missing data for variables in *FSCORE* models (require data for *t*, *t*-1, and *t*-2)	(829)
Companies in *FSCORE* sample	3,107
Less: Missing data for variables in *INCORRECT_MW* models (require data for *t*, *t*-1, and *t*-2)	(1,733)
Companies in *INCORRECT_MW* sample	2,203
Less: Missing data for variables in *AUDIT_FEES* models (require data for *t*, *t*-1, and *t*-2)	(710)
Companies in *AUDIT_FEES* sample	3,226

[a] We require the signature date of the auditor's report to be within 365 days of the company's fiscal year end to avoid including observations related to restatements and other unusual activity in our sample. Our data are based on the May 2018 updates of Compustat, Audit Analytics, and I/B/E/S in WRDS.

TABLE 2
Univariate Statistics for the Baseline Specification

	Discretionary Accruals			**Propensity to Misstate**			**Incorrect Material Weakness**			**Audit Fees**		
Variables	*Pre* Mean	*Post* Mean	*Diff.*	*Pre* Mean	*Post* Mean	*Diff.*	*Pre* Mean	*Post* Mean	*Diff.*	*Pre* Mean	*Post* Mean	*Diff.*
ABSDA	0.106	0.100	0.125									
FSCORE				1.196	1.162	0.128						
INCORRECT_MW							0.083	0.094	0.203			
AUDIT_FEES										13.777	13.819	0.215
SIZE	6.769	6.789	0.736	6.880	6.909	0.625	7.697	7.732	0.522	6.753	6.774	0.730
ROA	-0.099	-0.100	0.911	-0.075	-0.069	0.516	-0.016	-0.013	0.548	-0.100	-0.102	0.884
LEVERAGE	0.277	0.286	0.224	0.284	0.292	0.255	0.286	0.293	0.395	0.277	0.286	0.227
LOSS	0.363	0.354	0.449	0.334	0.327	0.553	0.252	0.246	0.651	0.360	0.353	0.568
MB	2.018	2.053	0.426	1.951	2.002	0.233	1.924	1.975	0.275	2.030	2.063	0.449
LAG_ACCR	-0.030	-0.050	**0.000**	-0.025	-0.046	**0.000**	-0.012	-0.032	**0.000**			
CFO	-0.001	-0.012	**0.096**	0.019	0.014	0.380	0.056	0.048	0.119	-0.002	-0.013	0.113
VOLATILITY	0.065	0.067	0.566	0.058	0.058	0.991	0.039	0.041	0.541	0.067	0.068	0.842
BIG4	0.652	0.640	0.307	0.657	0.644	0.300	0.782	0.774	0.514	0.650	0.638	0.311
AUDITOR_CHG	0.061	0.057	0.559	0.060	0.055	0.445	0.039	0.039	1.000	0.061	0.057	0.492
RESTATE	0.078	0.075	0.706	0.078	0.075	0.668	0.076	0.078	0.865	0.079	0.074	0.483
MW	0.072	0.084	**0.076**	0.074	0.084	0.133				0.074	0.085	0.129
GC	0.057	0.068	**0.078**	0.050	0.057	0.237	0.009	0.017	**0.011**	0.058	0.069	**0.083**
RECEIVABLE										0.185	0.187	0.739
INVENTORY										0.072	0.071	0.699
FOREIGN										0.385	0.391	0.610
BUSY										0.909	0.911	0.828
N	**3,185**	**3,185**		**3,107**	**3,107**		**2,203**	**2,203**		**3,226**	**3,226**	

This table presents descriptive statistics for U.S. companies with available data for the respective analyses in the period before ('Pre') and after ('Post') mandatory partner identification in Form AP. All continuous variables are winsorized at the 1st and 99th percentiles to mitigate the influence of outliers. Appendix A provides the variable definitions. Reported p-values in the *Diff.* column are based on two-sample t-tests for tests of differences in the means. Values in bold represent statistical significance at 0.10 level or lower.

TABLE 3
Regression Results for the Baseline Specification

Dependent Variable:	*ABSDA*	*FSCORE*	*INCORRECT_MW*	*AUDIT_FEES*
Intercept	-0.205 *	-3.370 ***	0.613 *	11.126 ***
	(0.106)	(0.531)	(0.334)	(0.295)
POST	-0.005 *	-0.045 ***	0.015 **	0.038 ***
	(0.003)	(0.014)	(0.007)	(0.004)
SIZE	0.042 ***	0.655 ***	-0.074 *	0.318 ***
	(0.015)	(0.076)	(0.041)	(0.030)
ROA	-0.050 *	-0.131 *	-0.036	-0.123 ***
	(0.030)	(0.073)	(0.069)	(0.030)
LEVERAGE	-0.043	-0.393 ***	0.011	-0.056
	(0.036)	(0.113)	(0.076)	(0.051)
LOSS	0.008	0.016	0.009	-0.008
	(0.009)	(0.033)	(0.025)	(0.013)
MB	0.002	0.018	-0.027 **	0.001
	(0.005)	(0.016)	(0.011)	(0.004)
LAG_ACCR	0.000	0.380 ***	-0.011	
	(0.032)	(0.108)	(0.072)	
CFO	0.031	-0.900 ***	0.027	-0.145 ***
	(0.040)	(0.147)	(0.115)	(0.044)
VOLATILTY	0.181 **	0.416	-0.100	0.010
	(0.074)	(0.308)	(0.173)	(0.060)
BIG4	0.023	0.125	0.111	0.487 ***
	(0.030)	(0.093)	(0.098)	(0.053)
AUDITOR_CHG	-0.004	0.046	0.007	-0.011
	(0.012)	(0.051)	(0.041)	(0.020)
RESTATE	0.000	0.006	0.007	0.027 *
	(0.009)	(0.035)	(0.032)	(0.014)
MW	-0.011	0.072		0.114 ***
	(0.013)	(0.052)		(0.024)
GC	-0.027	0.036	0.063	-0.020
	(0.028)	(0.068)	(0.082)	(0.219)
RECEIVABLE				-0.065
				(0.112)
INVENTORY				-0.025
				(0.219)
FOREIGN				0.172 ***
				(0.049)
BUSY				0.139
				(0.212)
N	6,370	6,214	4,406	6,452
Company Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.351	0.626	0.362	0.985

This table presents the baseline regression results based on the single time-series difference analysis to examine the association between mandatory partner identification in the U.S. and audit quality and cost. Robust standards errors clustered by company are included in parentheses. ***, **, and * represent significance at the 0.01, 0.05, and 0.10 levels, respectively, based on two-tailed tests. Appendix A provides the variable definitions.

TABLE 4
Difference-in-Differences Regressions

Panel A: Results Using the Early Discloser Sample

Dependent Variable:	*ABSDA*	*FSCORE*	*INCORRECT_MW*	*AUDIT_FEES*
POST	-0.006	-0.083	0.007	-0.011
	(0.008)	(0.059)	(0.009)	(0.015)
*POST*UNIDENTIFIED*	-0.003	0.009	0.017	0.046 ***
	(0.008)	(0.062)	(0.011)	(0.016)
N	2,232	2,284	2,226	2,270
Control Variables	Yes	Yes	Yes	Yes
Company Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.264	0.707	0.309	0.986

This table presents the difference-in-differences regression results using the early discloser control sample to examine the association between mandatory partner identification in the U.S. and audit quality and cost. Robust standards errors clustered by company are included in parentheses. ***, **, and * represent significance at the 0.01, 0.05, and 0.10 levels, respectively, based on two-tailed tests. For brevity, coefficients on control variables are suppressed. Control variables are the same as those used in Table 3. Appendix A provides the variable definitions.

Panel B: Results Using the Pseudo Adopter Sample

Dependent Variable:	*ABSDA*	*FSCORE*	*INCORRECT_MW*	*AUDIT_FEES*
POST	-0.005	-0.065 **	0.035 *	0.047 ***
	(0.007)	(0.026)	(0.020)	(0.010)
*POST*TREATMENT*	-0.000	0.017	-0.019	-0.009
	(0.007)	(0.030)	(0.021)	(0.011)
N	7,486	7,320	5,120	7,584
Control Variables	Yes	Yes	Yes	Yes
Company Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.369	0.625	0.353	0.986

This table presents the difference-in-differences regression results using the pseudo adopter control sample to examine the association between mandatory partner identification in the U.S. and audit quality and cost. Robust standards errors clustered by company are included in parentheses. ***, **, and * represent significance at the 0.01, 0.05, and 0.10 levels, respectively, based on two-tailed tests. For brevity, coefficients on control variables are suppressed. Control variables are the same as those used in Table 3. Appendix A provides the variable definitions.

TABLE 5
Alternative Measures of Audit Quality

Panel A: Early Discloser Sample

Dependent Variable:	*BASU COEFF.*	*MEET*	*IMPAIR*	*SPI_NEG*	*SPI_POS*	*DTA_ALLOW*
POST	-0.101	0.006	0.005	-0.078	0.040	0.013
	(0.102)	(0.049)	(0.043)	(0.053)	(0.050)	(0.008)
*POST*UNIDENTIFIED*	0.581 ***	0.002	0.022	0.074	-0.015	-0.006
	(0.191)	(0.051)	(0.046)	(0.055)	(0.051)	(0.008)
N	2,254	1,734	2,252	2,252	2,252	2,264
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Company Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.775	0.053	0.329	0.371	0.176	0.887

This table presents the difference-in-differences regression results using the early discloser control sample to examine the association between mandatory partner identification in the U.S. and alternative proxies for audit quality. The coefficient listed for *POST* (*POST*UNIDENTIFIED)* in the *BASU COEFF.* column is the coefficient on *POST*RET*NEG* (*POST*UNIDENTIFIED*RET*NEG*). Robust standards errors clustered by company are included in parentheses. ***, **, and * represent significance at the 0.01, 0.05, and 0.10 levels, respectively, based on two-tailed tests. For brevity, coefficients on control variables, which are reported in Online Appendix 3, are suppressed. Appendix A provides the variable definitions.

Panel B: Pseudo Adopter Sample

Dependent Variable:	*BASU COEFF.*	*MEET*	*IMPAIR*	*SPI_NEG*	*SPI_POS*	*DTA_ALLOW*
POST	0.052	0.012	0.003	0.014	0.024	0.011
	(0.112)	(0.032)	(0.020)	(0.024)	(0.018)	(0.008)
*POST*TREATMENT*	0.288 **	-0.013	0.003	-0.010	-0.008	-0.004
	(0.135)	(0.034)	(0.022)	(0.026)	(0.020)	(0.008)
N	6,576	3,462	7,886	7,886	7,886	7,316
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Company Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.683	0.077	0.306	0.360	0.140	0.917

This table presents the difference-in-differences regression results using the pseudo adopter control sample to examine the association between mandatory partner identification in the U.S. and alternative proxies for audit quality. The coefficient listed for *POST* (*POST*TREATMENT*) in the *BASU COEFF.* column is the coefficient on *POST*RET*NEG* (*POST*TREATMENT*RET*NEG*). Robust standards errors clustered by company are included in parentheses. ***, **, and * represent significance at the 0.01, 0.05, and 0.10 levels, respectively, based on two-tailed tests. For brevity, coefficients on control variables, which are reported in Online Appendix 3, are suppressed. Appendix A provides the variable definitions.

TABLE 6
Cross-Sectional Analyses for the Difference-in-Differences Samples

Panel A: Early Discloser Sample

	Coefficient on difference-in-differences variable interacted with the cross-sectional measure listed below			
VARIABLE	*ABSDA*	*FSCORE*	*INCORRECT_MW*	*AUDIT_FEES*
SIZE ≥ Median	ns	ns	ns	ns
MW	-0.091 ***	ns		0.861 ***
LIT_SCORE ≥ Median	-0.030 *	ns	ns	ns
GC				
LOSS	ns	ns	ns	ns
BANKRUPTCY_RISK ≥ Median	ns	ns	ns	ns
NUM_ANALYST ≥ Median	ns	ns	ns	ns
BIG4	ns	ns	ns	ns
INDUSTRY_EXPERT	ns	ns	ns	-0.078 *
TENURE ≥ Median	ns	ns	ns	ns
FEE_PRESSURE	ns	ns	ns	

ns Coefficient is not statistically different from zero (p-value, two-sided > 0.10)

Each cell represents the coefficient on the variable of interest in a separately estimated regression. The blank cells in this table reflect either insufficient variation to perform the cross-sectional tests (*GC*) or the cross-sectional variable is based on the dependent variable (*INCORRECT_MW*, *AUDIT_FEES*). ***, **, and * represent significance at the 0.01, 0.05, and 0.10 levels, respectively, based on two-tailed tests.

Panel B: Pseudo Adopter Sample

	Coefficient on difference-in-differences variable interacted with the cross-sectional measure listed below			
VARIABLE	*ABSDA*	*FSCORE*	*INCORRECT_MW*	*AUDIT_FEES*
SIZE ≥ Median	ns	ns	ns	ns
MW	-0.071 ***	ns		ns
LIT_SCORE ≥ Median	-0.028 **	ns	ns	0.044 **
GC	ns		ns	ns
LOSS	ns	ns	ns	ns
BANKRUPTCY_RISK ≥ Median	ns	ns	ns	ns
NUM_ANALYST ≥ Median	ns	ns	ns	-0.070 ***
BIG4	ns	ns	ns	ns
INDUSTRY_EXPERT	ns	ns	ns	ns
TENURE ≥ Median	0.031 **	ns	ns	ns
FEE_PRESSURE	ns	ns	ns	

ns Coefficient is not statistically different from zero (p-value, two-sided > 0.10)

Each cell represents the coefficient on the variable of interest in a separately estimated regression. The blank cells in this table reflect either insufficient variation to perform the cross-sectional tests (*GC*) or the cross-sectional variable is based on the dependent variable (*INCORRECT_MW*, *AUDIT_FEES*). ***, **, and * represent significance at the 0.01, 0.05, and 0.10 levels, respectively, based on two-tailed tests.

APPENDIX A
Variable Definitions

Dependent Variables:

ABSDA — the absolute value of discretionary accruals in year *t* based on the modified Jones model using a cross-sectional regression for each two-digit SIC industry and year with at least 15 companies. Specifically, we estimate the following model and then difference each company's residual with the residual from a company with the closest *ROA* in the same two-digit industry and year:

$$TA_{it}/A_{it-1} = \beta_1(1/A_{it-1}) + \beta_2((\Delta S_{it} - \Delta AR_{it})/A_{it-1}) + \beta_3(PPE_{it}/A_{it-1}) + u_{it}$$

where *TA* equals total accruals using the indirect cash flow method (i.e., income before extraordinary items minus operating cash flow from continuing operations); *A* equals total assets; ΔS equals the change in total sales from the prior year; ΔAR equals the change in accounts receivable from the prior year; *PPE* equals net property, plant, and equipment; and *i* and *t* are company and year indicators, respectively. To mitigate the influence of outliers, we use all available observations in Compustat with total assets greater than $1 million and winsorize continuous variables at the 1st and 99th percentiles.

FSCORE — the predicted probability of accounting misstatements in year *t* from model (1) in Table 7, Panel A of Dechow et al. (2011), scaled by the unconditional probability of having accounting misstatements. Values greater than (less than) one indicate a higher (lower) probability of misstatement/manipulation than the unconditional expectation.

INCORRECT_MW — 1 if the suspect classification and actual audit opinion disclosure for material weaknesses are not equal, and 0 otherwise. We identify 'suspect' observations if the company's propensity to report a material weakness ('predicted value') is in the top ten percent of 404(b) filers by year, based on all companies with an auditor opinion on ICFR and available data in Compustat, using equation (2) in Ge et al. (2017) as follows:

$$PredictedValue_{it} = 0.301(AggLoss_{it-1,t}) + 0.940(Misstate_{it-2,t-1}) + 0.072(Seg_{it}) - 0.344(Age_{it}) - 0.714(BankInd_{it}) - 0.361(LogMVE_{it-1}) - 1.088(Cash_{it-1}) - 1.285(InstOwn_{it-1}) + 3.161(MW404b_{it-1})$$

where variables are as defined in Ge et al. (2017): *AggLoss* is an indicator variable equal to 1 if the sum of IB (in Compustat) across *t* and *t*-1 is less than zero, and 0 otherwise; *Misstate* is an indicator equal to 1 if the firm restates its year *t*-2 or *t*-1 financial statements, and 0 otherwise; *Seg* equals the number of business and geographic segments in year *t*; *Age* equals company age in year *t*; *BankInd* is an indicator variable equal to 1 if the company is in the banking industry in year *t*, and 0 otherwise; *LogMVE* is the natural logarithm of market capitalization in year *t*-1; *Cash* equals the proportion of assets that are cash and cash equivalents in year *t*-1; *InstOwn* equals the proportion of institutional investors in year *t*-1; *MW404b* is an indicator variable equal to 1 if the auditor identifies a material weakness in its auditor's report in year *t*-1, and 0 otherwise; and *i* and *t* are company and year indicators, respectively.

AUDIT_FEES — natural logarithm of audit fees in year *t* from Audit Analytics.

Test Variables:

POST	1 if the fiscal year is the first year that mandatory audit partner identification is required in Form AP (PCAOB Rule 3211), and 0 otherwise. In the "pseudo adopters" difference-in-differences model, this variable equals 1 if the fiscal year is the first year that mandatory audit partner identification is required in Form AP (PCAOB Rule 3211) for the treatment group, or the audit report issuance is in the six months prior to January 31, 2017 for the control group (i.e., July 1, 2016 to January 30, 2017 (inclusive)), and 0 otherwise.
UNIDENTIFIED	1 if the audit engagement partner's identity was not disclosed prior to the adoption of PCAOB Rule 3211 in the difference-in-differences model using "early disclosers" as the control group, and 0 otherwise.
TREATMENT	1 for the group subject to PCAOB Rule 3211 in the difference-in-differences model using "pseudo adopters" as the control group, and 0 otherwise.

Control Variables:

SIZE	natural logarithm of total assets (AT) at the end of year *t.*
ROA	earnings before extraordinary items (IB) in year *t* divided by total assets (AT) at the end of year *t.*
LEVERAGE	total debt (DLC + DLTT) divided by total assets (AT) at the end of year *t.*
LOSS	1 if net income (NI) in year *t* is less than zero, and 0 otherwise.
MB	market value of assets (AT + (PRCC_F*CSHO) - CEQ) divided by book value of assets (AT) at the end of year *t.*
LAG_ACCR	total current accruals (IB + DPC – OANCF) in year *t*-1 divided by total assets (AT) at the end of year *t*-1.
CFO	cash flow from operations (OANCF) in year *t* divided by total assets at the end of year *t.*
VOLATILITY	standard deviation of *CFO* over the prior three years (*t*-2 to *t*).
BIG4	1 if the auditor is a Big 4 firm in year *t*, and 0 otherwise.
AUDITOR_CHG	1 if the company changes auditors in year *t*, and 0 otherwise.
RESTATE	1 if the company announced a restatement during fiscal year *t* (data from Audit Analytics), and 0 otherwise.
MW	1 if the company or the auditor disclosed a material weakness under SOX 404a/404b in year *t* (data from Audit Analytics), and 0 otherwise.
GC	1 if the auditor issued a going concern opinion in year *t* (data from Audit Analytics), and 0 otherwise.
RECEIVABLE	total accounts receivable (RECT) divided by total assets (AT) at the end of year *t.*
INVENTORY	total inventory (INVT) divided by total assets (AT) at the end of year *t.*
FOREIGN	1 if the company has foreign sales in year *t* in the Compustat Segments file, and 0 otherwise.
BUSY	1 if the company's fiscal year-end in year *t* is in December, and 0 otherwise.

Alternative Proxies for Audit Quality:[a]

EARN	earnings before extraordinary items (IB) in year *t* divided by the market value of equity (PRCC_F*CSHO) at the end of year *t*-1.

RET	12-month cumulative returns compounded from monthly returns in CRSP ending three months after fiscal year end.
NEG	1 if *RET* is negative, and 0 otherwise.
MEET	1 if a company's actual EPS minus the latest analysts' EPS forecast in year *t* is within zero to one cent (both inclusive), and 0 otherwise. The latest analysts' forecast is the most recent EPS forecast prior to the date of the earnings announcement. Actual and forecasted EPS values are obtained from the I/B/E/S unadjusted detail file. When more than one analyst forecast is issued on the same last day, we use the median forecast. To avoid stale forecasts, they must be no older than 90 days before the earnings announcement date.
IMPAIR	1 if the company discloses an impairment or write-down (GDWLIP < 0 or WDP < 0) in year *t*, and 0 otherwise.
SPI_NEG	1 if the company discloses negative special items (SPI < 0) in year *t*, and 0 otherwise.
SPI_POS	1 if the company discloses positive special items (SPI > 0) in year *t*, and 0 otherwise.
DTA_ALLOW	valuation allowance (as a positive value) divided by the total gross deferred tax assets in year *t*, as provided by Audit Analytics.
Additional Cross-Sectional Variables:	
LIT_ SCORE	*ex-ante* client-specific litigation risk based on Model 3 in Table 7 of Kim and Skinner (2012).
BANKRUPTCY_RISK	the decile rank of Altman's bankruptcy score, calculated following DeFond and Hung (2003) and Altman (1968).
INDUSTRY_EXPERT	1 if the audit firm has the largest audit fees in an MSA-year for a two-digit SIC industry and is at least ten percent above the next closest competitor (following Reichelt and Wang 2010), and 0 otherwise.
TENURE	the consecutive number of years of the auditor-client relationship through year *t* in Audit Analytics, including pre-2000 years with available auditor information from the Compustat database.
FEE_PRESSURE	1 if actual audit fees are less than expected audit fees, and 0 otherwise. We calculate expected audit fees following the approach in Ettredge et al. (2014). We use the fiscal year 2014 audit fees (for all companies with Audit Analytics and Compustat data) and the control variables from equation (2) to estimate an audit fee regression by *SIZE* quintile. We then save the coefficient estimates from each quintile and calculate an 'expected' audit fee for the pre-3211 and post-3211 periods based on the respective *SIZE* quintile and values of control variables in those years.

[a] Control variables that are unique to these alternative audit quality models are defined in Online Appendix 3.

APPENDIX B
Examples of Audit Partner Identification in Annual Shareholder Meeting Materials

Yahoo! Inc. – Annual Meeting of Shareholders held on June 30, 2016

"Now I'd like to introduce our outside representatives who are here today…Mark McCaffrey, Yahoo!'s audit partner at PricewaterhouseCoopers LLP, Yahoo!'s independent registered public accounting firm."

Fiserv, Inc. – Annual Meeting of Shareholders held on May 5, 2016

"Also with us in the audience today representing Deloitte & Touche is Kathy Feucht, who will be available as needed during the Q&A segment of the meeting."

Facebook, Inc. – Annual Meeting of Shareholders held on June 20, 2016

"I would also like to introduce two more people who are in attendance, Alex Bender of Ernst & Young LLP, our independent registered public accounting firm, and Kris Veaco, who will act as Inspector of Elections for the meeting and will tabulate the results of the meeting."

Colgate-Palmolive Company – Annual Meeting of Shareholders held on May 6, 2016

"Mr. Chairman, on behalf of the audit committee, I move that the selection of PricewaterhouseCoopers LLP as the independent registered public accounting firm of the company for the year 2016 be ratified. We have with us Bill Brennan, Michael Nelson, and Susan Diaz of PricewaterhouseCoopers. Mr. Brennan is the Global Engagement Leader, Mr. Nelson is the Audit Partner, and Ms. Diaz is the Audit Senior Manager on the Colgate account. Would you please stand?"

Idacorp, Inc. – Annual Meeting of Shareholders held on May 19, 2016

"And third, the ratification of the appointment of the independent registered public accounting firm for the year 2016 has passed. Mr. Scott Loveless, a partner in Deloitte & Touche LLP, our independent auditor, is here to make any statement he desires regarding the independent auditor's audited books and records of Idacorp. Mr. Loveless, do you have any statements you wish to make? Turn the lights up, please." [Scott Loveless made the following statement, "We would just like to say we appreciate the support of the board and the shareholders and look forward to serving you as your independent auditor here in the upcoming year."]

Dean Foods Company – Annual Meeting of Shareholders held on May 11, 2016

"Mr. Sam Loughry, senior representative from Deloitte, the company's independent public accounting firm is here and is available to answer your questions regarding the audit of the company's financial statements and the audit of our internal controls over financial reporting. Sam – where is Sam? Are there any questions for Deloitte?" [None asked]

As noted above, in some cases, the company does not explicitly refer to the named representative as the "audit partner." Our conversations with audit partners indicate that the representative(s) at the meeting almost always includes the lead audit partner. When collecting the data for our "early discloser" sample, we searched for the representative name on LinkedIn and/or in subsequent Form AP disclosures to verify that the representative was in fact an audit partner.

SUPPLEMENTAL MATERIALS

We provide the following supplemental materials in a separate online appendix. Online Appendix 1 provides descriptive statistics for the final samples in our "early discloser" difference-in-differences analyses, while Online Appendix 2 provides descriptive statistics for the final samples in our "pseudo adopter" difference-in-differences analyses. Online Appendix 3 includes the fully tabulated results for the alternative audit quality measures in Table 5. Finally, Online Appendix 4 consists of several sensitivity tests performed to examine the robustness of our difference-in-differences tests.

Online Appendix 1 – Descriptive Statistics for the Difference-in-Differences Analyses ("Early Disclosers")

Online Appendix 2 – Descriptive Statistics for the Difference-in-Differences Analyses ("Pseudo Adopters")

Online Appendix 3 – Fully tabulated results for Table 5 (Alternative Measures of Audit Quality)

Online Appendix 4 – Sensitivity Tests Related to the Difference-in-Differences Samples