Do Investors Care Who Led the Audit in the U.S.?
Evidence from Announcements of Accounting Restatements

Daniel Aobdia
daniel_aobdia@psu.edu
Smeal College of Business
Pennsylvania State University

Vincent Castellani
vac35@psu.edu
Smeal College of Business
Pennsylvania State University

Paul Richardson
pzr41@psu.edu
Smeal College of Business
Pennsylvania State University

First Draft: March 2023
Current Draft: January 2024

Abstract

In 2015, the PCAOB mandated the disclosure of audit partner names on Form AP. Although Form AP aimed to help investors assess the quality of audit partners and their audits, prior studies indicate that investors did not immediately respond to the filing of the disclosure nor did it have an immediate impact on audit quality. In this study, we examine whether investors value audit partner disclosures using a high-power setting, whereby audit partners experience a shock to their track records due to clients' accounting restatements. We find that an audit partner's non-restating clients exhibit a significantly negative market reaction to the announcement of a client restatement for which the partner signed off on misstated financial statements. This effect is concentrated in more severe restatements and occurs even for Big 4 auditors. Our findings provide novel evidence that audit partner disclosures in the U.S. provide useful information to equity markets.

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I.   INTRODUCTION

In 2015, following a lengthy and contentious standard setting process, the Public Company Accounting Oversight Board (PCAOB) enacted Rule 3211 which established new reporting requirements for auditors. The rule included a mandate requiring that audit firms disclose the name of the lead audit partner for each of their public clients through the submission of Form AP. The PCAOB stated the rule would achieve "the objectives of enhanced transparency and accountability for the audit," while also giving investors "information about the engagement partner … [which can] be used along with other information, such as history on other issuer audits or disciplinary proceedings, in order to provide insights into audit quality" (PCAOB 2015). In particular, some at the PCAOB argued for an accountability channel (i.e., stakeholders' ability to identify audit partners and link audit outcomes to them motivating partners to exercise greater care in their audits (e.g., PCAOB 2009; Harris 2015). Proponents also argued that, beyond the accountability channel and as the information accumulated over time, investors could gauge a partner's audit quality track record (CII 2009). Alternatively, opponents questioned the accountability channel and maintained the additional disclosure would not alter the quality of provided audits as audits are continually subject to "engagement quality assurance reviews, internal inspections, performance reviews, and other monitoring, including … peer reviews" (Deloitte 2009). Consistent with these claims, subsequent academic literature on partner disclosure in the United States mainly focuses on the accountability channel surrounding the initial disclosure of Form AP and fails to find significant changes in audit quality (Cunningham, Li, Stein, and Wright 2019). The existing literature also fails to find a significant market reaction to the first filings of Form AP (Doxey, Lawson, Lopez, and Swanquist 2021), casting doubt on the informativeness of the audit partner disclosure mandate.

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However, no study to date utilizes the audit partner disclosure, in tandem with negative shocks to a partner's track record for providing quality audits, to investigate its informativeness.

We fill this gap in the literature by investigating whether the announcement of a restatement of a fiscal year-end audit for which the partner signed off on the misstated financial statements impacts the stock price of other clients in the partner's portfolio. This setting is particularly fruitful to investigate the informativeness of audit partner disclosures given that the primary responsibility of the auditor is to obtain reasonable assurance that their clients' financial statements are free of material misstatement (PCAOB AS 1001, para 2). In this case, the partner did not initially catch the misstatement, possibly casting doubt on the quality of audits conducted by the partner or suggesting this partner tends to audit riskier clients. Further, prior literature documents that contagion effects of restatements exist broadly across the restating client's industry as well as across audit firms and industry specialists (e.g., Francis and Michas 2013; Gleason, Jenkins, and Johnson 2008; Ji, Kumar, Pei, and Xue 2019; Weber, Willenborg, and Zhang 2008). Thus, if investors find audit partner disclosures informative, we expect that investors of the partner's non-restating clients react negatively to the announcement of a restatement for one of the partner's other clients.

However, audit firms have implemented robust quality control systems over the years to ensure high quality audits and the bulk of the audit work of U.S. public companies is delegated to middle managers (e.g., Aobdia 2020; Aobdia, Choudhary, and Newberger 2023; Aobdia, Siddiqui, and Vinelli 2021; Gipper, Hail, and Leuz 2021), leaving a more limited role for lead audit partners. Additionally, because an audit provides *reasonable*, not *absolute*, assurance that the financial statements are free of material defect, situations may exist in which a partner conducted a quality audit, but the financial statements are nevertheless misstated. Thus, investors may not consider

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audit partner disclosures to be a valuable signal of audit quality, indicating the mandate is not informative.

To examine the equity market reaction to a shock to a partner's track record, we use restatements announced in 2017 to 2022 related to the misapplication of accounting principles and fraud. Specifically, we examine the market reaction of non-restating clients to a client restatement at the audit (1) partner-, (2) office-, and (3) firm-level compared to non-restating industry peers employing an alternative audit firm.[1] Throughout our analyses, we eliminate all observations with contemporaneous filings (e.g., earnings announcements, 10-Qs, and 10-Ks) to avoid confounding events and ensure clean windows for analysis. Figure 1 graphically illustrates our research design.

We find that an audit partner's non-restating clients experience a significant negative market reaction in the days surrounding the announcement of another client's restatement. This is consistent with investors using audit partner disclosures to monitor partner track records and react to restatements, which are a shock to the partner's track record, leading investors to revise their expectations about the reliability of the non-restating clients' financial statements. Interestingly, while the effect is already present in the three-day window surrounding the restatement announcement, the effect grows considerably larger when analyzing longer windows. Specifically, we find cumulative abnormal returns that average -0.46 percent for the three-day period surrounding the restatement for a partner's non-restating clients, and we also find these abnormal returns grow to -1.46 (-2.09) percent when using a [-2,+5] ([-2,+10]) window. This price drift is consistent with increased disclosure processing costs due to the information being disclosed on Form AP, which is only available on the PCAOB's website and not directly in a company's

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[1] We use non-restating industry peers audited by a different audit firm as a control group to account for the industry contagion effect documented in Gleason et al. (2008). This also allows us to control for potential economic links among clients audited by the same audit partner.

financial reports.[2] Importantly, these results also hold in multivariate analyses, albeit with weaker economic magnitudes, after controlling for previously identified determinants of the variation in a restatement's spillover reaction (Gleason et al. 2008), along with the inclusion of announcement year, industry, and restatement fixed effects. Specifically, non-restating clients experience cumulative abnormal returns around the restatement announcement that are, on average, between -0.26 and -1.64 percentage points lower than other impacted clients and industry peers, depending on the window examined. We fail to find evidence of a significant market reaction to a client's restatement announcement when investigating the presence of an audit firm or office effect. Overall, these results indicate that investors scrutinize shocks to audit partners' track records and penalize clients of partners associated with poor audit quality.

We next investigate whether there is a differential effect depending on the severity of the restatement. As audits specifically aim to obtain assurance that the client's financial statements are free of *material* misstatement (PCAOB AS 1001, para 2), we predict investors will react more strongly when the partner signed off on materially misstated financial statements. Accordingly, we partition the restatements into two subsamples, material restatements (Big R) and revisions (little r), and find that the negative market reaction is generally stronger within the Big R sample. In contrast, the reaction is either muted or absent in the little r sample depending on the examined window. These results indicate investors are sensitive to restatement severity and adjust their views accordingly.

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[2] See Blankespoor, deHaan, and Marinovic (2020) for a discussion on this topic. For example, Christensen, Floyd, Liu, and Maffett (2017) find a stronger negative market reaction when a mine safety citation is reported in an 8-K filing *and* disclosed on another website, compared with when it was reported only on that website, and Kim and Kim (2023) find that the market reaction to insider-trading filings on FDICconnect is less timely than to those reported on the SEC EDGAR system. However, as we also observe a drift in our control groups, where information is readily available on the SEC EDGAR website, we cannot rule out other types of disclosure processing costs, such as frictions in understanding the link between restating observations and related companies, also influence our results.

Next, we investigate whether the effect varies with auditor size. Larger audit firms, particularly the Big 4, have extensive quality control systems, including comprehensive methodologies and careful partner assignment and rotation policies, and the time spent by lead audit partners on each of their audits represents a small proportion of the total time spent by the entire engagement team (e.g., Aobdia 2018, 2020; Aobdia et al. 2023). For these reasons, partners working for larger firms may have less influence on how audits are conducted and investors might not react as strongly. However, we find that the market values audit partner disclosures even for Big 4 auditors. We find similar results for non-Big 4 auditors, and fail to find evidence suggesting that a differential reaction exists between Big 4 and non-Big 4 partners, although the reaction appears to be slightly lower for Big 4 auditors. These results suggest that equity markets value audit partner disclosures, even for the Big 4 audit firms that have extensive quality control systems to ensure quality audits.

Finally, we examine circumstances where we expect the market reaction to vary. First, as initial studies fail to find evidence of Form AP's informativeness (e.g., Cunningham et al. 2019; Doxey et al. 2021), we investigate whether the market immediately reacted to this information using our design. We do not find significant results when restricting our sample to the years immediately following the disclosure mandate. The results appear only once subsequent years are included. This suggests that the market either did not immediately recognize Form AP's informativeness or that it is empirically difficult to document the effect in the earlier years. Second, we investigate whether the effect differs based on characteristics of the non-restating client that likely increase information risk, which makes an audit a more valuable signal. We find evidence that the effect is more pronounced for less profitable, smaller, and riskier clients. Next, we investigate whether the effect differs when partners experience multiple restatements. We find that

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the effect from a partner's initial public restatement is not statistically different from the effect from a subsequent restatement. Finally, we investigate whether the effect is attenuated when a new partner (i.e., not the restating partner) takes over a non-restating client's engagement after the misstatement period ends but prior to the announcement of a restatement. We find that the effect among restating partners who are still on the engagement is not statistically different from the effect among non-restating clients with a new partner.

We conduct various sensitivity analyses to rule out alternative explanations and ensure our results are not subject to design choices. Specifically, we investigate and do not find evidence consistent with a stronger economic link existing between our non-restating clients and the restating client beyond that of the restating client's industry peers. Further, we perform within-firm, within-office, and within-CBSA analyses and find our results are robust. We also vary the composition of our control groups to examine whether our results are sensitive to the number of retained peers in our multivariate analyses and continue to find significant results. Given the nature of our design, we also ensure that the results are robust across a variety of return windows. Finally, we ensure that our results are not sensitive to the inclusion of alternative fixed effects or the method of standard error clustering. Overall, we continue to find evidence suggesting that equity markets value partner disclosure when a shock to their track record is made public.

Our findings contribute to several streams of literature. First, our evidence informs the debate of whether investors find the disclosure of engagement partner names valuable in the United States. Following an almost decade long rule making process, PCAOB Rule 3211 is one of the most significant changes to audit-related disclosure in recent years. Our study is the first to find direct evidence in the United States that investors use this disclosure in conjunction with

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restatements to form a signal of the audit quality provided by an engagement partner.[3] While our results are consistent with some experimental and international findings on audit partner disclosure, they contrast with prior archival studies in the United States that fail to find evidence that capital markets respond to audit partner identity or that audit quality changes surrounding implementation of the disclosure rule (Aobdia, Lin, and Petacchi 2015; Carcello and Li 2013; Cheng, Wang, Xu, and Zhang 2020; Cunningham et al. 2019; Doxey et al. 2021; Gul, Lim, Wang, and Xu 2019; Lambert, Luippold, and Stefaniak 2018). We note that prior archival studies in the U.S. primarily focus on the accountability effect of audit partner disclosures surrounding the initial implementation of the rule (e.g., Cunningham et al. 2019). In addition, Doxey et al. (2021) focuses on the date of the audit partner disclosure, which likely provides limited information in the absence of an understanding of an audit partner's track record, at least initially. Furthermore, investors likely do not monitor the disclosure of Form AP on the PCAOB's website in real-time. In contrast, we focus on a high-power setting whereby a partner's track record is negatively impacted by the announcement of a client's restatement and exploit this to understand whether investors consider audit partners' track record in their investment decisions.

Second, our evidence contributes to the disclosure literature by providing evidence that investors perceive an audit partner's track record as a signal of audit quality.[4] Theory and prior

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[3] Abbott, Barber, Buslepp, and Sapkota (2023) find weak indirect evidence that KPMG clients experienced negative abnormal returns at times of audit partner rotations following the "steal the exam" scandal. However, their results are based on a very limited number of mandatory and voluntary partner rotations and are present only on longer windows (30 and 60 days), and thus could be subject to confounding effects and alternative explanations. Abbott and Buslepp (2022) also find some evidence of equity market reaction at times of the PwC Oscars blunder, but this event, not related to a financial statement audit failure, preceded audit partner disclosures on Form AP.

[4] We note that our results are also consistent with an alternative explanation whereby the announcement of a client restatement signals that a partner tends to audit riskier clients, consistent with the analytical model of Titman and Trueman (1986) that shows a separating equilibrium can exist whereby "good clients" match with "good audit firms" and "bad clients" match with "bad audit firms." While the interpretation of the results is slightly different under this scenario, our results nevertheless provide evidence that equity markets value audit partner disclosures in the United States, which is our primary research question.

literature note that investors value observable signals of audit quality when making investment decisions (DeAngelo 1981; DeFond and Zhang 2014). We provide evidence that audit partner names are an important, yet simple, signal of audit quality that can help alleviate information asymmetry.[5] Our results also contribute to the disclosure processing costs literature, as we find evidence that the market takes time to process the implications of a restatement from a partner's client to the partner's other clients. Further, we answer the call by Blankespoor et al. (2020, pg. 27) to understand the effect of particular dissemination channels on market efficiency. In particular, our results suggest that disclosure of audit partner names on Form AP may be an impediment to market efficiency. Our results also contrast with a literature that finds a limited role of audit engagement partner characteristics in the United States, perhaps because audit firms have robust quality control systems for partner staffing, rotation, and monitoring decisions to ensure quality audits (e.g., Aobdia 2020; Aobdia et al. 2021, 2023; Gipper et al. 2021; Laurion, Lawrence, and Ryans 2017).

Collectively, our results suggest that it may be unproductive for researchers to focus on the immediate effects surrounding the implementation of a new disclosure rule, particularly when the rule is meant to provide information that may initially be of limited value but will accumulate over time. We note that our study is related to Gul et al. (2019) who also focus on shocks to audit partners' track records in China. While similar in nature, Gul et al. (2019) focus on the Chinese setting, where the effect of individual audit partners on audit quality has been extensively documented (e.g., Gul, Wu, and Yang 2013). In contrast, we investigate the phenomenon in the United States, where evidence that individual audit partners affect audit quality is much more

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[5] We note that we only test whether audit partner disclosure is a valuable signal in this paper. While theory states information asymmetry will be reduced with signals of quality, we leave it to future research to empirically demonstrate this finding.

limited, in part because audit partners spend a limited amount of time on the audits they lead relative to the remainder of the engagement team (e.g., Aobdia et al. 2021, 2023; Gipper et al. 2021; Laurion et al. 2017).[6] Gul et al. (2019) also focus on financial reporting fraud as opposed to restatements. Financial reporting frauds are much more serious and imply intentional misrepresentation of financial statements as opposed to restatements which are often unintentional errors and more common. Therefore, while the two studies are similar, the question as to whether equity markets value partner disclosures in the United States remains unanswered.

II.   BACKGROUND, LITERATURE, AND HYPOTHESIS DEVELOPMENT

Institutional Background

In May of 2007, Treasury Secretary Henry Paulson created the U.S. Department of the Treasury's Advisory Committee on the Auditing Profession (hereafter the Committee) and tasked the Committee with providing recommendations to increase investor confidence in the profession. Secretary Paulson noted that confidence in the auditing profession is important given the role audit quality plays in overall financial reporting quality and well-functioning financial markets. Among the Committee's final recommendations was a mandate that audit partners sign the audit report to improve accountability and transparency among audit firms.

Tasked with the standard-setting process to implement this recommendation, the PCAOB issued an initial concept release in July of 2009 detailing the requirement for the partner's signature on the audit report and requested comments. The initial feedback was mixed. Proponents of the rule argued the signature would increase a sense of partner accountability for the audit and provide

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[6] While studies investigating audit quality in China are important in their own right, the vast differences in political and regulatory forces question whether findings can be generalized to the United States. For example, audit partner disclosure in China requires partners to sign their name on the audit opinion filed with the financial statements. Compromises made during the rule-making process in the United States resulted in the separate disclosure of the partner's name on Form AP and no disclosure in the actual audit opinion, making the disclosure more opaque and difficult to access by investors. See Lennox and Wu (2022) for further discussion on the differences and a detailed review of China-related accounting literature.

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useful information for investors to judge partner quality (CalPERS 2009; CII 2009). Opponents of the rule, including the largest audit firms, argued a variety of reasons for not including the signature on the audit report including increased partner liability, existing quality control procedures, and engagement team structure (Deloitte 2009; PwC 2009).

After a lengthy comment letter process, the PCAOB adjusted its proposal multiple times, resulting in a final proposal in December of 2015, known as Rule 3211, requiring disclosure of the audit partner's name on a new form, Form AP. The proposed Form AP was to be filed with the PCAOB no more than 35 days after the date on which the auditor's report is first included in an SEC filing unless the filing is related to an IPO, in which case it must be submitted within 10 days. The contents are then made public via a new searchable database on the PCAOB's website. Disclosure on a separate form was noted by the PCAOB as an "acceptable second-best approach" and that "investors and investor groups generally preferred auditor signature or disclosure on the auditor's report" (PCAOB 2015). SEC approval of the rule took place in May of 2016, thus ending a long and contentious rulemaking period. The rule mandates the disclosure of the lead audit partner on Form AP for all audit reports signed on or after January 31, 2017.

Prior Research and Hypothesis Development

Disclosure regulation in the United States is largely driven by claims that reducing information asymmetry frictions between capital providers and companies results in well-functioning markets. The theoretical underpinning of this argument is a large body of academic literature documenting that additional disclosure alleviates market frictions and reduces information asymmetry.[7] While additional disclosure reduces information asymmetry, there is a premium for credible information (Kausar, Shroff, and White 2016; Lennox and Pittman 2011;

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[7] See Beyer, Cohen, Lys, and Walther (2010) for a review of the literature.

Electronic copy available at: https://ssrn.com/abstract=4702538

Minnis 2011). James Doty, former Chairman of the PCAOB, echoes these claims by stating "independent audits provide … trust, and thus bridge the gap between entrepreneurs who need capital and investors who can provide capital. There have been many changes over the years in the nature of the information that capital users provide to markets, changes designed to maintain the relevance of the information to capital providers" (James R. Doty 2014). As Doty notes, the dynamic nature of the capital markets requires changes in disclosure over time and the audit profession is not immune to this change. Thus, while audit opinions provide assurance on whether a client's financial statements are fairly presented in accordance with GAAP, additional information is likely relevant and important to investors.

A signal is relevant to investors if the provided information helps more precisely determine a client's quality and allows for more efficient capital allocation. Specifically, information that helps determine the quality of a client's financial reporting will increase the precision of a client's financial disclosure, where high-quality financial reporting is defined as a function of a company's innate characteristics, financial reporting system, and audit quality (DeFond and Zhang 2014). Audit quality, unlike an audit opinion, provides investors "greater assurance that the financial statements faithfully reflect the client's underlying economics, conditioned on its financial reporting system and innate characteristics" (DeFond and Zhang 2014).[8] Therefore, disclosures that allow investors to better understand audit quality will assist in determining the overall quality of the client's financial reporting and further reduce information asymmetries.

Prior literature documents significant variation in audit quality at the audit firm-, office-, and partner-level. For example, characteristics such as audit firm size, branch office

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[8] The primary distinction between audit quality and an audit opinion is assurance over how well the financial statements reflect underlying economics instead of technical compliance with GAAP. Given this difference, two clients with clean audit opinions can vary drastically in terms of their actual audit quality.

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characteristics, and industry expertise have been shown to be associated with differences in audit quality (e.g., DeAngelo 1981; DeFond, Erkens, and Zhang 2017; Francis and Michas 2013; Francis and Yu 2009; Reichelt and Wang 2010). Recently, a booming literature has pushed the level of analysis further down to the audit partner-level, and finds, outside of the United States, that individual partner characteristics influence audit quality (e.g., Gul et al. 2013; Lennox and Wu 2018). Thus, to the extent that investors are aware of variation in the level of audit quality provided, they should value the disclosure of the names of audit partners. Prior literature finds results consistent with this argument in several settings including Taiwan, the United Kingdom, and China (Aobdia et al. 2015; Carcello and Li 2013; Gul et al. 2019). In particular, the literature identifies reputation effects of audit failures at the audit firm- and partner-level outside of the United States in the form of a decline in stock price for other clients audited by the same auditor or audit partner at the time the audit failure is announced (e.g., Gul et al. 2019; Li, Qi, Tian, and Zhang 2017; Weber et al. 2008). We use this powerful setting to investigate whether capital market participants value audit partner disclosures on Form AP. Specifically, the announcement of a restatement suggests that a particular audit partner signed off on financial statements that were misstated signaling the partner either provides low quality audits or tends to match with riskier clients.

However, there are several important reasons why equity investors may not react to a shock to a partner's track record in the United States. First, recent archival literature using U.S. data surprisingly finds limited evidence that partner rotations or characteristics influence audit quality (e.g., Aobdia et al. 2021, 2023; Laurion et al. 2017). One explanation is that the bulk of the work for audits of public companies in the U.S. is delegated to middle managers (Aobdia et al. 2023), thus the role of audit partners might be more limited relative to other countries. Another explanation is that audit oversight in the U.S. has been extensive since the Sarbanes Oxley Act of

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2002 and the inception of the PCAOB, leading audit firms to implement robust quality control system mechanisms for partner staffing, rotation, and monitoring decisions to ensure quality audits (e.g., Aobdia 2020; Gipper et al. 2021). Consistent with this idea, Deloitte was censured by the PCAOB in 2007 for failing to remove an underperforming audit partner from the audit of Ligand Pharmaceutical in 2003 and 2004. Eventually, Deloitte resigned from the engagement in August of 2004 and Ligand restated its financial statements for 2003 - 2004 (Boone, Khurana, and Raman 2015; PCAOB 2007). Similar PCAOB enforcement actions over the years and quality control criticisms identified as part of PCAOB inspections likely deterred audit firms, particularly the largest ones, from staffing underperforming audit partners on their public audits.[9] Thus, to the extent that audit partner characteristics exhibit less variation or matter less from an audit quality standpoint in the U.S., capital markets would not value the disclosure of their names as much as in other jurisdictions. Consistent with this idea, prior literature on the initial disclosure of partner names in the U.S. fails to find evidence of either an increase in audit quality or a market reaction to the disclosure (Cunningham et al. 2019; Doxey et al. 2021).

Second, in contrast with other jurisdictions where partners are required to sign the audit opinion, disclosure in the U.S. is accomplished through Form AP. As such, investors may not immediately be aware of the information or react as strongly given that prior literature has documented that investors generally react stronger to disclosure in the financial statements (e.g., Blankespoor et al. 2020; Christensen et al. 2017). The discussion above leads to our first hypothesis, stated in the null.

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[9] PCAOB inspections include a review of quality control systems of an audit firm, including partner management policies (e.g., Aobdia 2020). Quality control issues identified by the PCAOB remain nonpublic if remediated within one year of the issuance of the PCAOB inspection report (section 104(g)(2) of the Sarbanes Oxley Act of 2002). This structure provides strong incentives for audit firms to remediate quality control issues, which likely explains why public disclosures of quality control criticisms are reasonably rare despite the PCAOB frequently identifying them (Aobdia 2020).

H1: A partner's non-restating clients do not exhibit a significant market reaction to the announcement of a client restatement.

Whether equity markets will react similarly to restatements of different severity levels is unclear. Restatements are classified by severity into two different types, reissuance and revision restatements. Reissuance restatements, known as 'Big R' restatements, occur when clients identify a *material error* in a prior period's financial statement and correct the error by amending the financial statements and issuing an 8-K.[10] Big R restatements provide a strong signal that the audit partner failed to gather sufficient evidence that the financial statements were free of material misstatements (e.g., Christensen, Glover, Omer, and Shelley 2016). Alternatively, revision restatements, known as 'little r' restatements occur when clients identify an *immaterial error* or series of errors in prior periods that are corrected by a revision in the current period's financial statements.[11] Choudhary, Merkley, and Shipper (2021) document an increase (decrease) in little r (Big R) restatements over time along with evidence of differential market reactions to the types of errors. Given Big R restatements are, by definition, material and larger restatements, we predict a stronger market reaction among Big R restatements as opposed to little r restatements. However, as the difference between the two types of restatements is mainly driven by the error's materiality, auditors and management may exercise their discretion in applying materiality thresholds for restatement classification (Acito, Burks, and Johnson 2019; Black, Choudhary, and Goodman

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[10] Choudhary et al. (2021) define a reissuance restatement as "under current guidance, an error judged to be material to the financial reports for the period(s) in which it occurred requires a statement of non-reliance (Item 4.02 in an 8-K filing) for the report(s) containing the error, a full recast of financial statements in either an amended filing or a future filing, and a revised audit opinion with discussion in the auditor's report."

[11] Choudhary et al. (2021) define a revision as "a correction of an error or series of errors whose nature is such that (i) the error is immaterial to each individual financial reporting period in which the error occurred using both period-by-period and cumulative analyses of the effects of the error and (ii) collectively, the cumulative magnitude of the errors is sufficient to distort current period financials if all the errors are corrected in the period in which the errors are discovered … These errors do not require an Item 4.02 statement of non-reliance because the financial statements containing the errors are not materially misstated and thus can be relied upon. These errors are corrected by revising comparative financial statements included in the current period financial report and adjusting the opening balance of retained earnings to correct the past-period (s) error(s)."

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2022; Choudhary, Merkley, and Schipper 2019; Thompson 2022). Therefore, investors may be suspicious of opportunistic behavior and not react differentially to the type of restatement. Overall, it remains an empirical question if the equity market reaction is dependent on the severity of a restatement. This discussion leads to our second hypothesis, stated in the null.

H2: A partner's non-restating clients do not exhibit a differential market reaction to the announcement of a client restatement, regardless of restatement severity.

We also test whether equity markets react differently for large auditors. DeAngelo (1981) argues that audit quality depends on auditor size, allowing investors to use auditor size as a signal of audit quality. Follow up archival studies generally find that Big 4 auditors are associated with higher audit quality (e.g., DeFond et al. 2017; DeFond and Zhang 2014; Teoh and Wong 1993). Larger auditors have more extensive quality control systems, such as partner assignment and rotation policies, which can help them minimize quality differences in their audits (Aobdia 2020). To the extent that there is less variation in partner quality within the Big 4 firms relative to non-Big 4 firms, we would expect the equity market reaction to be attenuated for Big 4 partners. Nevertheless, PCAOB enforcement actions against the Big 4 firms suggest that some of their partners underperform.[12] Further, Big 4 audit firms typically audit larger clients, and their audits and audit partners might be under greater investor scrutiny than those of smaller companies. Overall, it is not clear ex-ante whether there is a differential equity market reaction if the partner is employed by the Big 4 or a non-Big 4 audit firm. This leads to our third and final hypothesis, stated in the null.

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[12] For example, the PCAOB suspended a Deloitte partner in 2008 because, following discovery of a material overstatement of assets, revenues, and earnings after the earnings announcement but before release of the 10-K, this partner agreed to a change of a materiality threshold prior to the issuance of the audit opinion and to questionable offsets of the overstatement (PCAOB 2008). Deloitte was censured in 2013 for keeping this individual in a consultation role, in violation of the initial 2008 PCAOB order (PCAOB 2013).

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H3: A partner's non-restating clients do not exhibit a differential market reaction to the announcement of a client restatement, regardless of whether the partner is a Big 4 or non-Big 4 partner.

III. RESEARCH DESIGN AND SAMPLE SELECTION

Research Design

Following prior literature (e.g., Chaney and Philipich 2002; Gul et al. 2019; Weber et al. 2008), we examine whether an auditor's non-restating clients experience a negative market reaction to the announcement of another client's restatement. Specifically, we examine the market reaction of *tainted* auditors, where a tainted auditor is defined as the audit (i) partner, (ii) office, or (iii) firm associated with a client's misstated financial statements. We also include a sample of peers operating within the same industry as the restating client but employing an alternative audit firm (hereby, industry peers) to ensure that any documented market reaction is greater than the previously documented industry contagion effect (Gleason et al. 2008). We estimate the following regression model:

$$
\begin{aligned}
CAR\,[-2\,,+2]_{itr} ={} & \beta_0 + \beta_1 Tainted\ Auditor_{itr} + \beta_2 Size_{itr} + \beta_3 Market\ to\ Book_{itr} + \beta_4 ROA_{itr} \\
& + \beta_5 Loss_{itr} + \beta_6 Leverage_{itr} + \beta_7 Volatility_{itr} \\
& + \beta_8 Institutional\ Ownership_{itr} + \beta_9 Big\ 4_{itr} + \beta_{10} Auditor\ Tenure_{itr} \\
& + \beta_{11} Analyst\ Following_{itr} + \beta_{12} Bog\ Index_{itr} \\
& + Year, Industry, \& Restatement\ FE + \varepsilon_{itr}
\end{aligned}
$$

$$(1)$$

where *i* indexes a client, *t* indexes the year of the restatement announcement, and *r* indexes the specific restatement. *CAR* [-2,+2] is the five-day cumulative abnormal return centered on the restatement announcement. Daily abnormal returns are obtained from CRSP and are calculated as a client's raw return minus the CRSP value-weighted index. The variable of interest, *Tainted Auditor*, takes on one of the following three variables: *Tainted Partner*, *Tainted Office*, or *Tainted Firm*. *Tainted Partner*, our main variable of interest, is an indicator variable that equals one if a

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client is currently or was previously audited by a tainted partner (see Figure 1 for more information on this measure). *Tainted Office* and *Tainted Firm* are indicators at the office- and firm-level, defined similarly. We also modify this model by including all three *Tainted Auditor* variables after constructing them to be mutually exclusive (i.e., we set *Tainted Office* and *Tainted Firm* to zero when *Tainted Partner* is equal to one, and *Tainted Firm* to zero when *Tainted Office* is equal to one). This allows us to identify whether the reactions at the various levels within the audit firm are statistically different from one another.

The model includes several control variables motivated from prior literature to account for various client characteristics (e.g., Gleason et al. 2008; Gul et al. 2019). Specifically, we control for client size (*Size*) as larger clients are subject to a greater amount of external scrutiny. We control for future growth expectations (*Market to Book*) to account for heighted pressures to meet future expectations. We further control for performance (*ROA*), whether the client reports a loss (*Loss*), capital structure (*Leverage*), and operational volatility (*Volatility*). We also control for the percentage of shares held by institutional investors (*Institutional Ownership*), whether a client's auditor is a member of the Big 4 (*Big 4*), and the audit firm's tenure (*Tenure*). To further control for a client's information environment, we include a variable for analyst following (*Analyst Following*). Finally, we control for a client's financial reporting readability (*Bog Index*). All variables are defined in Appendix A.

The model includes both year and industry fixed effects, based on 2-digit SIC codes, to account for macroeconomic conditions and time-invariant industry characteristics. Restatement fixed effects (i.e., indicator variables equal to one for a given restatement event for both treated and control groups) are included to control for restatement-specific characteristics. Continuous controls are winsorized at the top and bottom percentile and standardized to have a mean of zero

and a standard deviation of one for ease of interpretation. Finally, standard errors are clustered by 2-digit SIC code and restatement announcement date.

Data and Sample Description

Table 1 describes the sample selection process. We begin by identifying all annual restatements announced between 2017 and 2022 related to the misapplication of accounting principles and fraud from Audit Analytics.[13] As Form AP went into effect on January 31st of 2017, we begin our sample in 2017 and end in 2022 due to data availability, leaving a total of 2,945 annual restatements.

Using these restatements, we can identify the audit partner associated with the audit opinion during the misstated period for 1,439 partners via Audit Analytics' Audit Opinions Form AP database. We require the partner to be publicly visible to market participants before the restatement announcement (i.e., Form AP has been filed). We then identify a partner's non-restating clients for the period beginning when the misstatement begins and ending with its ultimate disclosure. Again, we require the market to be able to link the partner to the client. Ultimately, we identify a total of 2,362 clients audited by a tainted partner. We then remove 621 due to missing controls and another 776 due to overlapping events.[14] Our final sample includes 965 clients audited by a tainted partner with sufficient data and a clean window around the restatement announcement date.

We repeat this procedure to identify clients audited by tainted offices (audit firms). We classify audit offices as those operating within the same 2017 Census Bureau Core-Base Statistical

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[13] We focus on annual restatements (i.e., those restatements for which the restating period overlaps with the client's fiscal year-end) as 10-K filings are audited, while 10-Q filings are only reviewed.
[14] We remove observations that either announced earnings or filed an annual or quarterly report within the seven-day window surrounding the restatement announcement. We also remove observations that filed their own restatement in the prior two months.

Area (CBSA).[15] We identify a total of 1,269 (1,247) tainted offices (audit firms) with 35,172 (541,499) non-restating clients. Observations with missing controls (11,538 and 158,537) and those with overlapping events (11,033 and 172,672) are then removed. Finally, to keep the number of clients audited by the same firm reasonable, we restrict each restatement event to the 30 tainted firm clients closest in size to the restating client. The final sample includes 12,601 (40,213) clients audited by a tainted office (audit firm).

Finally, we identify industry peers not audited by a tainted auditor but operating in the same industry as the restating client. Following Gleason et al. (2008), we identify 74,075 peers operating within the same 8-digit Global Industry Classifications Standards (GICS) code as the restating clients. Observations with missing controls (18,947) and those with overlapping events (23,828) are then removed. Finally, we again restrict each restatement event to the 30 peers closest in size to the restating client. This leaves a final sample of 15,780 industry peers.

Table 2 Panel A presents a summary of the number of restating clients, clients audited by a tainted auditor, and industry peers based on the year of the restatement announcement. The number of restatement announcements appears to be fairly distributed throughout the sample except for 2021, where we see a fairly large increase.[16,17] Table 2 Panel B presents descriptive statistics. *Tainted Partner* has a mean value of 0.0172, indicating that 1.72% of our sample consists of client observations that employed a tainted partner. This proportion increases to 2.63% (decreases to 1.34%) when the sample is split into Big R (little r) restatements. In terms of control

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[15] 2017 CBSA areas and zip codes are gathered from the U.S. Census Bureau website. We use 2017 as this is the first year of Form AP disclosure. However, CBSAs infrequently change, so we do not expect the date of the CBSA classification to impact our results in a significant manner.

[16] This table reports the number of clients that *announced* a restatement in each year and does not provide information on the misstatement period.

[17] In April of 2021, the SEC issued guidance requiring SPACs account for their warrants as liabilities and classify their equity as temporary, which resulted in a significant number of restatements (Coates and Munter 2021). Therefore, in untabulated tests, we remove SPAC restatement announcements in 2021 related to this guidance and find that our main results are robust to the exclusion of these restatement announcements.

variables, an average client commands assets of $5.0 billion, has 60% of its shares held by institutional investors, and about 64% of observations are audited by the Big 4.

IV.   MAIN RESULTS

Univariate Results

While we mainly focus on the five-day return window, we also investigate alternative windows to examine whether disclosure processing costs exist in this setting, given that Form AP is a relatively obscure filing. Panel A of Table 3 reports mean and median cumulative abnormal returns using various restatement announcement windows.[18] For this table, we remove *Tainted Partner* observations from the *Tainted Office* sample and then *Tainted Office* observations from the *Tainted Firm* sample to ease comparisons.[19] We find a few noteworthy insights. First, the returns are significantly negative across nearly all windows for all groups, suggesting a negative market reaction to the announcement of a restatement exists at the partner-, office-, and firm-level. Further, an industry contagion effect also exists, consistent with Gleason et al. (2008). Second, the information does not appear to be immediately impounded into price with returns drifting more negative for all groups. This result is particularly pronounced for the *Tainted Partner* sample as the average *CAR* [-1,+1] of -0.46 percent is more than three times smaller than the average *CAR* [-2,+5] of -1.46 percent. This suggests disclosure processing costs exist, perhaps because Form AP is an obscure filing released on an infrequently visited website. However, we also observe a drift for the other groups as well, where the information is more readily available from

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[18] Our research design (i.e., a within-restatement analysis) allows us to abstract away from how the market responds to the restatement for the restating firm. Nevertheless, we find mean (median) CARs for 1,089 restating firms using [-1, +1], [-2, +2], [-2, +5], and [-2, +10] windows of -1.38 (-0.81), -1.55 (-0.86), -1.57 (-1.02), and -1.56 (-1.11) percent, respectively. If we restrict our analysis to restatements that elicited a negative market reaction, the magnitude of the CARs increase to -6.78 (-3.94), -6.97 (-4.13), -6.89 (-4.40), and -7.43 (-4.67) percent. These return magnitudes are consistent with prior literature (e.g., Ji et al. 2019).

[19] For example, we have 12,601 *Tainted Office* and 965 *Tainted Partner* observations. When we remove partner observations from office observations, we are left with 11,636 *Tainted Office* observations (12,601 – 965).

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traditional filings. Thus, our results are consistent with high disclosure processing costs either because of difficulty accessing the information or because understanding the implications of such information might take some time (e.g., Ali and Hirshleifer 2020).

Panel B of Table 3 provides the mean and median differences between the *Tainted Partner* sample and the other groups. Across all windows, we find evidence suggesting clients employing tainted partners are impacted to a greater extent than the comparison group with the differences statistically significant in all but the [-1,+1] window when examining the mean difference between the *Tainted Firm* and *Tainted Partner* samples.

Multivariate Results

Table 4 reports the results of the estimation of Equation (1). The dependent variable in columns 1 through 4 is *CAR* [-2,+2]. We initially examine the non-restating clients' equity market reaction to a restatement announcement using *Tainted Firm* as the variable of interest in column 1. We find that the coefficient on *Tainted Firm* is insignificant, providing no evidence to suggest an adverse market effect at the firm-level beyond an industry effect. Next, we investigate the office-level effect in column 2 using *Tainted Office* as the variable of interest, and find that the coefficient is insignificant. Column 3 presents the results for our main specification. Consistent with equity markets finding the disclosure of audit partner names informative, we find a negative and statistically significant coefficient on *Tainted Partner* at the 1% level. This suggests that the partner-level effect is significantly more negative than the pooled office-, firm-, and industry-effect. In terms of economic magnitude, clients whose audit partner is associated with a restatement experience cumulative abnormal returns around the restatement announcement that are 0.86 percentage points lower, on average, than other impacted clients and industry peers.

Our main design examines the differential impact at the partner-level against a pooled sample of clients at the office-, firm-, and industry-level. Therefore, in column 4, we test whether the effect differentially impacts each group by including three mutually exclusive indicators (i.e., *Tainted Partner*, *Tainted Office*, and *Tainted Firm*). This allows for direct comparisons of the partner effect to that of the office and firm. The results are consistent as the coefficient on *Tainted Partner* remains statistically negative. Tests of coefficient equality are included at the bottom of the table and are consistent with equity markets reacting more strongly at the partner-level. Specifically, we find a significant difference between *Tainted Partner* and *Tainted Firm* as well as between *Tainted Partner* and *Tainted Office*.

Alternative windows in columns 5 to 7 (i.e., [-1, +1], [-2, +5], and [-2, +10]) allow us to evaluate the sensitivity of our results to the five-day return window choice.[20] Column 5 finds the reaction is negative and statistically significant ($p = 0.085$), albeit with a weaker magnitude, when using *CAR* [-1, +1] as the dependent variable. Columns 6 and 7 find the reaction is significant and economically larger using longer return windows. Consistent with our univariate evidence, we find weaker market reactions in shorter windows with the effect growing stronger in longer windows, implying the market impounds this information into price slowly. Taken together, Table 4 provides evidence of a partner's non-restating clients exhibiting a significant market reaction to the announcement of a client restatement, allowing us to reject our null hypothesis (H1) and conclude that equity markets appear to value audit partner disclosure.

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[20] In untabulated tests, we also examine whether the documented returns reverse in the period following the restatement announcement by examining cumulative abnormal returns for the [+11, +60], [+11, +90], or [+11,+180] windows. We fail to find significant evidence of a reversal for the *Tainted Partner* sample. This result contrasts with Abbott and Buslepp (2022), who find that negative equity returns following the PwC Oscar's blunder reversed within a month.

Restatement Severity

To investigate if a partner's non-restating clients exhibit a differential market reaction to the announcement of a more severe client restatement, we examine Big R (reissuance) and little r (revision) restatements separately. Following prior literature, we expect equity markets to react more strongly to the Big R restatements (Choudhary et al. 2021).

Table 5 reports the results from the estimation of Equation (1) for Big R (little r) restatements in columns 1 through 4 (5 through 8) where the dependent variable is *CAR* [-1, +1], *CAR* [-2, +2], *CAR* [2, +5], and *CAR* [-2, +10] in columns 1 through 4 (5 through 8).[21] Focusing on Big R restatements, we find the coefficient on *Tainted Partner* is negative and significant in all columns. Consistent with our previous findings, the coefficient estimate grows in magnitude as the window increases. The effect is economically meaningful as non-restating clients audited by a tainted partner experience cumulative abnormal returns around a Big R restatement that are lower by 0.46 to 2.72 percentage points, on average.[22] Columns 5 through 8 investigate little r restatements. We find that the coefficient on *Tainted Partner* is negative and statistically significant in all columns except column 5. However, the economic magnitudes appear to be smaller than that of Big R restatements and suggest that clients audited by a tainted partner experience cumulative abnormal returns around a little r restatement that are, on average, lower by 0.23 to 0.97 percentage points.

Further, we stack the models and perform tests of coefficient equality on *Tainted Partner* to examine if a differential effect exists across Big R and little r restatements (displayed at the

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[21] As in Table 4, we fail to find evidence of an audit firm- or office-level effect based on restatement severity.
[22] In untabulated tests, we estimate the results including three mutually exclusive indicators (i.e., *Tainted Partner*, *Tainted Office*, and *Tainted Firm*), and continue to find that the coefficient on *Tainted Partner* is statistically different from *Tainted Firm* and *Tainted Office*.

bottom of Table 5). While we fail to find a significant difference when analyzing shorter return windows, we find evidence that the partner-level effect is stronger for Big R restatements in longer return windows (i.e., *CAR* [-2, +5], *CAR* [-2, +10]). This provides some evidence to reject the null hypothesis that equity markets do not react differentially based on restatement severity (H2).

Auditor Size

We next examine whether audit firm size differentially influences our results. Table 6 reports the results of Equation (1) using *CAR* [-2, +2] as the dependent variable. Columns 1 and 2 report the results when the audit firm of the restating client is a Big 4 (non-Big 4) auditor.[23] Focusing on Big 4 auditors in column 1, we find that the coefficient on *Tainted Partner* is negative and significant. Likewise, we find similar results when examining non-Big 4 auditors in column 2. To examine if there is a difference for Big 4 and non-Big 4 auditors, we stack the regressions and perform a test of coefficient equality on *Tainted Partner*. We fail to find a significant difference, even though the reaction appears to be larger for non-Big 4 auditors on average. In untabulated tests, we repeat this separately for Big R or little r restatements and reach similar conclusions (i.e., a test of coefficient equality on *Tainted Partner* fails to find a difference between Big 4 and non-Big 4 auditors). Therefore, we fail to reject the null hypothesis that no difference exists in the partner-level reaction for Big 4 and non-Big 4 auditors (H3).

Taken together, the results from Tables 4, 5, and 6 are consistent with an audit partner-level effect that varies with restatement severity, but not with audit firm size. Overall, our evidence suggests that equity markets find audit partner disclosures informative for all audit firms.

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[23] When a restating client's misstated period was audited by both a Big 4 and non-Big 4 auditor, we classify the restating client as a Big 4 restating client. Our inferences are not sensitive to this choice.

V.   ADDITIONAL ANALYSES

Market Learning Analysis

As initial studies failed to document evidence of Form AP's informativeness (e.g., Doxey et al. 2021), we investigate whether the market immediately reacted to the information content in our setting by estimating Equation (1) using subsamples that grow over time. Specifically, we examine the partner-level effect for (i) 2017, (ii) 2017 – 2018, and (iii) 2017 – 2019. If investors did not immediately recognize the information content provided by Form AP and instead took time to react, we expect to find significant coefficients on *Tainted Partner* in the later subsamples.

Consistent with our expectation, the coefficient on *Tainted Partner* is insignificant in columns 1 and 2 of Panel A of Table 7 where we examine 2017 and 2017 – 2018. When the sample period is extended to 2019, we find that the coefficient on *Tainted Partner* becomes negative and significant ($p = 0.065$). Column 4 then presents the result from the full sample. Overall, the results suggest that the market did not immediately recognize the informativeness of Form AP, consistent with prior studies (e.g., Doxey et al. 2021). [24]

Non-Restating Client Characteristics

The value of an audit lies in its ability to reduce information risk by providing assurance that the financial statements faithfully represent a client's underlying economics (DeFond and Zhang 2014). Thus, we investigate whether the documented effect varies by non-restating client characteristics that increase information risk, which make the audit a more valuable signal to investors. Specifically, we examine whether the negative response is more pronounced for clients that are traditionally associated with elevated levels of information asymmetry: smaller clients

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[24] We caveat that the failure to find an effect in the earlier years of the sample period is subject to numerous interpretations such as insufficient investors trading on the information or the inability to empirically document the effect (e.g., small sample size or wide variability in returns).

(*Size*), less profitable clients (*ROA*), and riskier clients (*Volatility* and *Accruals*).[25] For each measure, we split *Tainted Partner* into either *Above Median* or *Below Median* based on whether the client is above or below the *Tainted Partner* sample's median value. The results are displayed in columns 1 through 4 of Panel B of Table 7 where we split on *Size*, *ROA*, *Volatility*, and *Accruals*, respectively. Consistent with our main results, we generally document that all clients are impacted regardless of the split.[26] Further, consistent with the partner-level effect being more pronounced for clients with elevated information risk, we document that the magnitude varies in the expected directions (i.e., it is more pronounced for smaller, less profitable, and riskier clients); however, the coefficients are only statistically different in columns 2 and 3 when splitting on *ROA* and *Volatility*. We note that the more muted equity returns for larger non-restating clients is inconsistent with an alternative explanation whereby equity markets react to an expected increase in future regulatory scrutiny for clients of tainted audit partners.[27] Under this alternative, we would expect equity returns to be more negative for larger clients, rather than muted.

Multiple Associated Restatements

We further examine whether the partner-level reaction is concentrated in the initial restatement a partner is associated with or if the effect is similar for all restatements. We do so by decomposing *Tainted Partner* into *Initial Restatement*, an indicator variable equal to one if the corresponding restatement is the partner's initial associated restatement, and *Subsequent Restatement*, an indicator variable equal to one if the corresponding restatement is not the partner's

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[25] *Accruals* measures discretionary accruals using the cross-sectional modified Jones model (e.g., Aobdia 2019).

[26] Non-restating clients possessing below median *Accruals* fall just outside of generally accepted statistical significance in column 4 ($p = 0.104$).

[27] Prior research documents that regulators tend to focus more on larger companies, in order to prevent accounting scandals that would reach the magnitude of an Enron or a WorldCom (e.g., Aobdia 2019; Aobdia et al. 2023; Holtzman et al. 2023). More generally, the Sarbanes Oxley Act and recent SEC and PCAOB rules have focused more on the audits of larger companies relative to smaller ones.

initial associated restatement.[28] In column 5 of Panel B of Table 7, we find a significantly negative partner-level reaction for both a partner's initial restatement (*Initial Restatement*) as well as subsequent restatements (*Subsequent Restatement*). A test of coefficient equality fails to find evidence of a significant difference. Therefore, we cannot conclude that the market responds differently to initial or subsequent restatements.

New Partner *Fresh Look* Effect

In creating our sample, we identify a partner's non-restating clients in the period between the beginning of the misstatement and its announcement, which lasts, on average, 725 days in our sample. As a result, it is possible that some non-restating clients have since switched to a new partner by the time of the restatement announcement. It is further possible that the market may perceive that employing a new partner provides a *fresh look* and alleviates the concern that the tainted partner performed a poor-quality audit. To empirically investigate, we decompose *Tainted Partner* into *Associated Partner*, an indicator equal to one if the current partner is the tainted partner, and *New Partner*, an indicator equal to one if a new partner takes on the engagement after the misstatement period but before the restatement announcement. We expect the coefficient on *New Partner* to be significantly lower than the coefficient on *Associated Partner*.

We present the results in column 6 of Panel B of Table 7. We find a significantly negative partner-level reaction for clients currently employing the tainted partner (*Associated Partner*) as well as for those employing a new partner (*New Partner*). While the magnitude on *Associated Partner* appears to be larger, a test of coefficient equality again fails to find a significant difference between the two coefficients.

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[28] As partners are not observable prior to 2017, a partner may have been associated with a restatement prior to our sample period. Therefore, we are examining the initial restatement via Form AP identification and not necessarily the first restatement. However, the interpretation of our results is not altered as we are interested in the market perception of restatements.

VI. SENSITIVITY TESTS

Information Transfers Between Economically Linked Clients

It is possible that audit partners systematically match with clients that are economically linked. This would imply that our findings may manifest even in the absence of the shared partner given information transfers take place between economically linked firms (e.g., Ali and Hirshleifer 2020). We explore this concern in Appendix B by examining whether other value-relevant events (i.e., earnings announcements) also induce significant information transfers between the restating client and those clients classified as *Tainted Partner* observations. We fail to find evidence of larger information transfers around the restating clients' earnings announcements for the *Tainted Partner* observations when compared to the restating clients' industry peers. While we cannot make statistical inferences, these findings are inconsistent with *Tainted Partner* observations being any more economically linked to the restating clients than the restating clients' own industry peers.

Within-Firm, -Office, -CBSA Analysis

Throughout the paper, our sample includes the restating clients' industry peers which biases our results towards the null (i.e., our tests require the negative market reaction at the partner-level is greater than that at the industry-level). Therefore, we formally investigate the within-audit firm effect in Panel A of Table 8. Column 1 presents the baseline result where we include three mutually exclusive indicators (i.e., *Tainted Partner*, *Tainted Office*, and *Tainted Firm*). Column 2 presents results for the within-audit firm analysis by removing industry peers from the sample. Consistent with prior results, we find a significantly negative coefficient on *Tainted Partner*. In column 3, we perform a within-office analysis and continue to find a significantly negative coefficient on *Tainted Partner*. Taken together, these results strengthen our conclusion that the effects of restatements are concentrated at the partner-level.

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In column 4, we investigate the within-CBSA effect by benchmarking the effect against clients operating in the same CBSA as the restating client but employing alternative auditors as the control group. Using this control group, we continue to find consistent results and alleviate concerns that our results may be attributable to geographic shocks.

Alternative Control Restrictions

In an attempt to avoid saturating our analyses with control observations, we impose a restriction that each restatement possess no more than 30 industry peers and *Tainted Firm* observations. As this cutoff is due to researcher discretion, we examine the sensitivity of our results to this choice. Columns 1 through 5 of Panel B of Table 8 Panel B display the results with a cutoff of (i) 10, (ii) 25, (iii) 50, (iv) 100, and, finally, (v) with no restriction on the number of control observations per restatement event. The coefficient on *Tainted Partner* is significantly negative across all specifications, alleviating concerns that our results are impacted by this design choice.

Untabulated Sensitivity Analyses

In untabulated tests, we ensure our main results are robust to (i) the exclusion of all fixed effects, (ii) various time fixed effects such as year-quarter, year-month, or industry-year, (iii) various industry fixed effects such as 3-digit SICs, 2-digit SICs, or the Fama-French 48 industry classification scheme, (iv) alternative standard error clustering approaches at the client- or filing date-level as well as two-way clustering at both the client- and month-level or the client- and quarter-level, (v) constructing our sample of industry peers using 2-digit SICs, 3-digit SICs, or the Fama-French 48 industry classification scheme as opposed to using GICS codes as in Gleason et al. (2008), and (vi) the removal of SPAC-related restatements.[29]

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[29] Specifically, in April of 2021, the SEC issued guidance requiring SPACs account for their warrants as liabilities and classify their equity as temporary (Coates and Munter 2021). As a result, a significant number of SPACs issued restatements. To ensure that our results are robust to the exclusion of these restatements, we obtain SPAC information

VII. CONCLUSION

In 2015, the PCAOB enacted Rule 3211 which required auditors to begin disclosing the name of the audit partner for all of their public clients on Form AP. The PCAOB argued that by providing investors with knowledge of who performed an audit, investors can perceive important signals about the client's audit quality (PCAOB 2015). The PCAOB believed this more precise signal of audit quality would culminate in capital market benefits, such as a lower cost of capital (PCAOB 2015). However, opponents argued that partners are part of the larger audit firm, where quality control systems already ensure high audit quality across all engagements, and partner disclosure only puts undue liability on partners (Deloitte 2009; PwC 2009).

This paper examines the PCAOB's claims that investors would find partner disclosure informative in the United States using accounting restatements as a setting. We find an audit partner's non-restating clients experience a significant negative market reaction to the announcement of a restatement for one of their partner's other clients. We then examine whether this effect varies based on restatement severity and auditor size. We find the effect is concentrated in more severe restatements and is present even for Big 4 audit partners. Overall, consistent with the PCAOB's predictions, we find that investors view the disclosure as informative.

In addition to documenting that investors find partner disclosure via Form AP informative, this paper contributes to the disclosure literature by documenting shocks to an audit partner's track record as another signal of audit quality (DeFond and Zhang 2014). Using the unique features of restatement contagion, we disentangle the impact of shocks to an audit partner's track record from other confounding factors, such as the immediate ramifications of a restatement to the restating

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from Audit Analytics and remove restatements from our sample if the restating firm was a SPAC and the restatement was classified as either due to 'Debt, quasi-debt, warrants & equity (BCF) security issues (Key #4)' or 'Debt and/or equity classification issues (Key #26)' by Audit Analytics.

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client. We also contribute to the disclosure processing literature as our results become stronger in longer return windows, providing evidence suggesting that the capital market reactions may be partially delayed because of the difficulty investors face accessing the information.

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Variable	Definition	Source
CAR [-1, +1]	The three-day cumulative abnormal return centered on the restatement announcement. The daily abnormal returns are measured as the client's raw returns minus the CRSP value-weighted market index.	CRSP
CAR [-2, +2]	The five-day cumulative abnormal return centered on the restatement announcement. The daily abnormal returns are measured as the client's raw returns minus the CRSP value-weighted market index.	CRSP
CAR [-2, +5]	The eight-day cumulative abnormal return centered on the restatement announcement. The daily abnormal returns are measured as the client's raw returns minus the CRSP value-weighted market index.	CRSP
CAR [-2, +10]	The thirteen-day cumulative abnormal return centered on the restatement announcement. The daily abnormal returns are measured as the client's raw returns minus the CRSP value-weighted market index.	CRSP
Tainted Partner	An indicator variable that equals one if the client was audited by a partner with a fiscal-year-end restatement on another audit, and zero otherwise (see Figure 1).	Audit Analytics
Tainted Office	An indicator variable that equals one if the client was audited by an office with a fiscal-year-end restatement on another audit, and zero otherwise.	Audit Analytics
Tainted Firm	An indicator variable that equals one if the client was audited by an audit firm with a fiscal-year-end restatement on another audit, and zero otherwise.	Audit Analytics
Initial Restatement	An indicator variable that equals one if *Tainted Partner* equals one and the restatement is the initial restatement the partner is associated with since the initial disclosure of PCAOB Form AP.	Audit Analytics
Subsequent Restatement	An indicator variable that equals one if *Tainted Partner* equals one and the restatement is not the initial restatement the partner is associated with since the initial disclosure of PCAOB Form AP.	Audit Analytics
Associated Partner	An indicator variable that equals one if *Tainted Partner* equals one and the tainted partner is the client company's current auditor.	Audit Analytics
New Partner	An indicator variable that equals one if *Tainted Partner* equals one and the tainted partner is not the client company's client company's current auditor.	Audit Analytics
Accruals	The residual of the cross-sectional (i.e., 2 digit SIC and year) regression of accruals (deflated by beginning period assets) on gross property, plant and equipment (PP&E) deflated by beginning period assets, change in revenues deflated by beginning period assets, one over beginning period assets, and prior year return on asset (ROA) defined as income before extraordinary items deflated by average assets. Accruals are defined as income before extraordinary items minus cash flow from operations excluding extraordinary items and discontinued operations.	Compustat
Controls		
Size	The natural logarithm of total assets measured as of the fiscal year-end prior to the restatement announcement.	Compustat
Market to Book	Market value of equity divided by book value of equity measured as of the fiscal year-end prior to the restatement announcement.	Compustat
ROA	Net income divided by total assets measured as of the fiscal year-end prior to the restatement announcement.	Compustat
Loss	An indicator variable that equals one if the client is operating at a loss as of the fiscal year-end prior to the restatement announcement, and zero otherwise.	Compustat
Leverage	Total liabilities divided by total assets measured as of the fiscal year-end prior to the restatement announcement.	Compustat
Volatility	The standard deviation of daily stock returns over a 12-month period	CRSP
Institutional Ownership	The percentage of shares owned by 13-F filers measured as of the closest 13-F report date prior to the restatement announcement.	Thomson Reuters
Big 4	An indicator variable that equals one if the client is audited by a Big 4 audit firm, and zero otherwise.	Audit Analytics
Tenure	The number of years that the client has engaged their current audit firm.	Audit Analytics
Analyst Following	The number of unique analysts providing earnings per share forecasts for a client each year.	IBES
Bog Index	A client's 10-K readability measure (Bonsall, Leone, Miller, Rennekamp 2017).	Brian Miller's Webpage

Appendix B
Potential Information Transfer Between Economically Linked Clients

To investigate whether our results are driven by economic linkages between a partner's clients, we exploit Foster (1981)'s information transfer framework (i.e., information disclosed by one firm may be used to update future expectations of other economically linked firms). If our results are driven by the *Tainted Partner* clients' significant economic linkage to the restating clients, we expect other value-relevant events (i.e., earnings announcements) to also induce significant information transfers. Prior literature finds this is a powerful setting and documents various intra-industry information transfers (e.g., Brochet, Kolev, and Lerman 2008; Christensen et al. 2023; Foster 1981; Hann, Kim, and Zheng 2019). The absence of a significant information transfer between the restating client and *Tainted Partner* clients may suggest our results are driven by the hypothesized audit-related concerns and not the degree of economic linkage between the two clients.

We identify all earnings announcements for our restating clients in the two-years preceding and following their restatement announcement. We then examine if information transfers take place from the restating clients' earnings announcements to peers operating in the same GICS-based industry. Importantly, we include the *Tainted Partner* observations in the sample. Following Christensen et al. (2023), we define the following equation:

$$|CAR|_{itr} = \beta_0 + \beta_1 |CAR_{Restate}|_{ftr} + \beta_2 Tainted\ Partner_{itr} + \beta_3 |CAR_{Restate}|_{ftr}$$
$$* Tainted\ Partner_{itr} + \text{Controls} + \text{Fixed Effects} + \varepsilon_{itr}$$

$$(2)$$

where *i* indexes a non-restating observation (i.e., a *Tainted Partner* client or industry peer), *f* indexes a restating client, *t* indexes the quarter of the earnings announcement, and *r* indexes the specific restatement. The dependent variable, *|CAR|*, is the absolute value of the three-day cumulative abnormal return centered on the earnings announcement of the restating clients for the non-restating observations. $|CAR_{Restate}|$ is the absolute value of the three-day cumulative abnormal return centered on the restating clients' own earnings announcements. *Tainted Partner* is an indicator variable that equals one if a non-restating client was classified as a *Tainted Partner* in our main analyses. If the *Tainted Partner* clients are economically linked to a greater degree to the restating clients than the restating client's industry peers, we expect to find a positive coefficient on β_3.

We include the following control variables from prior literature: the market value of equity of the non-restating observation (*MVE*), the book-to-market ratio of the non-restating observation (*BTM*), the market value of equity of the restating client (*Res_MVE*), the book-to-market ratio of the restating client (*Res_BTM*), the analyst following of the restating client (*Following*), and the percentage of shares of the restating client held by institutional investors (*Institutional Ownership*) (Brochet et al. 2018; Christensen et al. 2023). The model also includes restatement and year-quarter fixed effects along with standard errors clustered at the earnings announcement.

Table B1, displayed below, presents the results of the estimation of Equation (2). Consistent with prior work, the coefficient on *|CAR Restate|* is significantly positive indicating the

presence of information transfers from the restating client's earnings announcement.[30] On the other hand, we fail to find a significant coefficient on our variable of interest (i.e., the interaction term between $|CAR_{Restate}|$ and *Tainted Partner*). Further, an F-test on the main effect (i.e., |CAR Restate|) and the interaction fails to find that the main effect for Tainted Partner observations is statistically different from zero, which is inconsistent with different clients of the same audit partner even being economically connected. Overall, these results are inconsistent with *Tainted Partner* clients experiencing a greater degree of information transfer around the restating clients' earnings announcements.
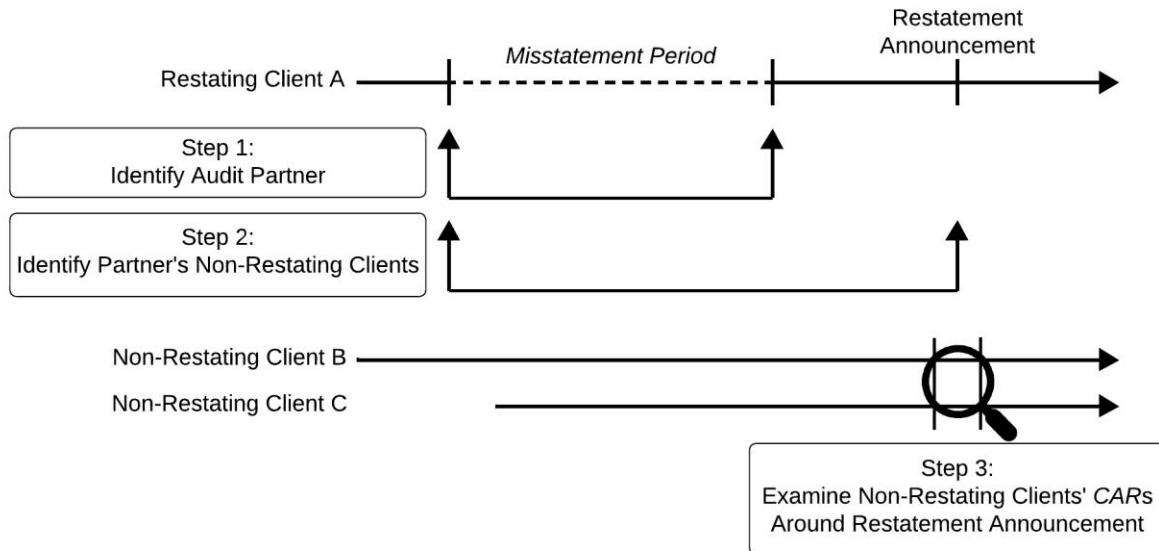
Table B1
Information Transfers Around Restating Firms' Earnings Announcements

Dependent Variable:	|*CAR*|
	(1)
$|CAR_{Restate}|$	0.0086***
	(3.24)
Tainted Partner	0.0010
	(0.94)
$|CAR_{Restate}|$ * Tainted Partner	**-0.0140**
	(-1.20)
MVE	-0.0033***
	(-44.63)
BTM	-0.0024***
	(-13.21)
Res_MVE	0.0013***
	(2.62)
Res_BTM	0.0061***
	(7.64)
Following	-0.0000
	(-1.15)
Institutional Ownership	0.0006
	(0.59)
Restatement Fixed Effects:	Yes
Year – Quarter Fixed Effects:	Yes
Adjusted R^2	0.1440
Observations	364,540

This table examines information transfers from a restating client's earnings announcements to their industry peers and clients audited by a tainted audit partner. The dependent variable is |CAR| which is the absolute value of the three-day cumulative abnormal return centered on the earnings announcement of the restating client for the non-restating observations. The variable of interest is the interaction term between $|CAR_{Restate}|$ and *Tainted Partner*. $|CAR_{Restate}|$ is the absolute value of the three-day cumulative abnormal return centered on the restating clients' own earnings announcement. *Tainted Partner* is an indicator variable that equals one if a non-restating client was classified as a *Tainted Partner* in the main analyses. *t*-statistics are shown in parentheses below the estimated coefficients and use standard errors clustered by the earnings announcement. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

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[30] The magnitude of our documented transfer (i.e., 0.86 percent) is smaller than other studies examining earnings announcement-related information transfers; however, it is important to note that we are only examining the earnings announcements of restating clients which may systematically induce weaker transfers than the general population of earnings announcements.

Figure 1
Research Design Illustration

This figure provides a visual representation of our research design. To examine if an audit partner's non-restating clients experience an adverse market reaction to the announcement of another client's restatement, we gather annual accounting-related restatement data from Audit Analytics. Included in the data are the start and end dates of the misstated period as well as the restatement announcement date. We then identify the audit partner that signed off on the misstated financial statements using Audit Analytics' Form AP database, requiring that the partner's name be publicly visible at the time of the restatement announcement. Next, we identify the partner's non-restating clients between the beginning of the misstatement period and its eventual public disclosure. Finally, we examine the non-restating clients' cumulative abnormal returns around the restating client's restatement announcement as the restatement announcement provides the market with a powerful signal the partner may have provided poor audit quality. We require non-restating firms have clean windows for the examination (i.e., they do not announce earnings, file financials, or announce their own restatement in the period surrounding the restatement announcement). Further, in order to benchmark the partner's non-restating clients' returns, we create control groups using the same preceding procedure at the audit office- and firm-level. We also benchmark the partner's non-restating client returns against the returns obtained by the restating client's industry peers to reduce a concern that economic links across companies might explain our results.

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Table 1
Sample Selection

Number of Annual Restatements Announced Between 2017 and 2022:			**2,945**
Auditor Identification	**Partners**	**Offices**	**Firms**
Tainted Auditors	1,439	1,269	1,247
Non-Restating Clients	2,362	35,172	541,499
Less: Missing Controls	(621)	(11,538)	(158,537)
Less: Overlapping Events	(776)	(11,003)	(172,672)
Less: Observation Restriction	-	-	(170,077)
Number of Tainted Clients	**965**	**12,601**	**40,213**
Industry Peers			
Industry Peers	74,075		
Less: Missing Controls	(18,947)		
Less: Overlapping Events	(23,828)		
Less: Observation Restriction	(15,520)		
Number of Industry Peers	**15,780**		

This table provides details about the sample selection process. We start by identifying all announced annual restatements related to the misapplication of accounting principles and fraud between 2017 and 2022 from Audit Analytics. Next, we identify the audit partner, office, and firm that signed off on the audit opinion for the restating client at any point during the misstatement period to identify tainted partners, offices, and firms. We then identify non-restating clients that currently or previously employed a tainted partner, office, or firm. We remove observations missing control variables as well as those possessing an event that overlaps with the restatement announcement. Further, we limit each restatement event to no more than 30 tainted firm observations. We retain the closest 30 clients in size to the restating client if more than 30 are identified. Next, we identify industry peers as those operating within the same 8-digit GICS code as the restating client. Again, we require that these observations have non-missing control variables and do not possess events that overlap with the restatement announcement. Similarly, we limit each restatement event to no more than 30 industry peer observations. We retain the 30 industry peers closest in size to the restating client if more than 30 are identified.

Table 2
Sample Description & Descriptive Statistics

Panel A: Yearly Information

Year	Restatement Announcements	Non-Restating Audit Clients			Industry Peers
		Tainted Partners	Tainted Offices	Tainted Firms	
2017	140	48	801	4,461	1,603
2018	184	144	1,980	6,860	2,714
2019	183	159	2,191	7,096	3,001
2020	131	131	1,534	4,978	2,207
2021	357	323	3,999	11,402	4,105
2022	137	160	2,096	5,416	2,150
Total	1,132	965	12,601	40,213	15,780

Panel B: Descriptive Statistics

Variable	Observations	Mean	Standard Deviation	25th Percentile	Median	75th Percentile
CAR [-1, +1]	55,993	-0.0012	0.0665	-0.0239	-0.0017	0.0198
CAR [-2, +2]	55,993	-0.0030	0.0857	-0.0326	-0.0032	0.0241
CAR [-2, +5]	55,983	-0.0048	0.1094	-0.0440	-0.0056	0.0290
CAR [-2, +10]	55,949	-0.0080	0.1459	-0.0605	-0.0094	0.0367
$Tainted\ Partner$	55,993	0.0172	0.1301	0.0000	0.0000	0.0000
$Tainted\ Partner_{Big\ R}$	16,628	0.0263	0.1600	0.0000	0.0000	0.0000
$Tainted\ Partner_{little\ r}$	39,365	0.0134	0.1150	0.0000	0.0000	0.0000
$Tainted\ Office$	55,993	0.2250	0.4176	0.0000	0.0000	0.0000
$Tainted\ Firm$	55,993	0.7182	0.4499	0.0000	1.0000	1.0000
$Size$	55,993	5017.6	13,247	206.87	889.46	3087.8
$Ln(Size)$	55,993	6.7267	2.0005	5.3369	6.7917	8.0355
$Market\ to\ Book$	55,993	3.9040	8.7156	1.1705	2.2505	4.6737
ROA	55,993	-0.0765	0.2803	-0.1002	0.0109	0.0595
$Loss$	55,993	0.4219	0.4939	0.0000	0.0000	1.0000
$Leverage$	55,993	0.5514	0.2951	0.3459	0.5403	0.7335
$Volatility$	55,993	0.0352	0.0225	0.0192	0.0295	0.0442
$Institutional\ Ownership$	55,993	0.5977	0.3400	0.3047	0.7016	0.8924
$Big\ 4$	55,993	0.6375	0.4807	0.0000	1.0000	1.0000
$Tenure$	55,993	9.0822	6.4926	3.0000	7.0000	15.000
$Analyst\ Following$	55,993	7.8680	8.3258	1.0000	5.0000	11.000
$Bog\ Index$	55,993	89.465	19.438	86.000	92.000	98.000
$Initial\ Restatement$	55,993	0.0043	0.0657	0.0000	0.0000	0.0000
$Subsequent\ Restatement$	55,993	0.0129	0.1128	0.0000	0.0000	0.0000
$Associated\ Partner$	55,993	0.0151	0.1221	0.0000	0.0000	0.0000
$New\ Partner$	55,993	0.0021	0.0459	0.0000	0.0000	0.0000

This table presents yearly information on the number of restatements in Panel A from which we construct our sample of clients with tainted partners, offices, and firms as well as the industry peer sample. The sample selection process is presented in Table 1. Panel B presents summary statistics. All variables are defined in Appendix A. In this table alone, we also display the raw unlogged value of *Size* in millions to ease interpretations.

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Table 3
Univariate Results

Panel A: Market Reactions

| | Audit Clients | | | | | | Industry Peers (n = 15,780) | |
| | Tainted Partner (n = 965) | | Tainted Office (n = 11,636) | | Tainted Firm (n = 27,612) | | | |
Return Window	Mean	Median	Mean	Median	Mean	Median	Mean	Median
[-1, +1]	-0.46**	-0.40***	0.01	-0.13***	-0.21***	-0.19***	-0.03	-0.13***
[-2, +2]	-1.05***	-0.84***	-0.10	-0.26***	-0.33***	-0.27***	-0.34***	-0.41***
[-2, +5]	-1.46***	-1.19***	-0.28**	-0.54***	-0.50***	-0.51***	-0.51***	-0.61***
[-2, +10]	-2.09***	-1.76***	-0.72***	-1.06***	-0.81***	-0.84***	-0.76***	-1.05***

Panel B: Univariate Differences

| | Office – Partner | | Audit Firm – Partner | | Industry – Partner | |
Return Window	Mean Difference	Median Difference	Mean Difference	Median Difference	Mean Difference	Median Difference
[-1, +1]	0.48*	0.27***	0.25	0.22***	0.43**	0.27***
[-2, +2]	0.96***	0.57***	0.72***	0.57***	0.71***	0.43***
[-2, +5]	1.18***	0.65**	0.96***	0.68**	0.95***	0.58*
[-2, +10]	1.37**	0.70*	1.28***	0.92**	1.33**	0.71*

This table reports the mean and median cumulative abnormal returns for various windows centered around the restatement announcement date for the various samples in Panel A. The abnormal returns are measured as the client's raw return minus the CRSP value-weighted index. For both panels, we remove *Tainted Partner* observations from the *Tainted Office* sample as well as *Tainted Partner* and *Tainted Office* observations from the *Tainted Firm* sample in order to allow for comparisons. Panel B displays the mean and median differences between the various subsamples. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

Dependent Variable:	CAR [-2, +2]				CAR [-1, +1]	CAR [-2, +5]	CAR [-2, +10]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tainted Firm	0.0008			0.0005			
	(0.50)			(0.34)			
Tainted Office		0.0012		0.0024			
		(1.05)		(1.19)			
Tainted Partner			-0.0086***	-0.0077***	-0.0026*	-0.0124***	-0.0164***
			(-4.02)	(-3.33)	(-1.75)	(-5.15)	(-3.52)
Size	-0.0006	-0.0006	-0.0006	-0.0007	-0.0006	-0.0029**	-0.0042**
	(-0.56)	(-0.56)	(-0.56)	(-0.61)	(-1.03)	(-2.39)	(-2.06)
Market to Book	-0.0003	-0.0003	-0.0003	-0.0003	-0.0003	-0.0005	-0.0002
	(-0.85)	(-0.85)	(-0.87)	(-0.88)	(-1.56)	(-0.83)	(-0.22)
ROA	-0.0000	0.0000	-0.0000	0.0000	-0.0001	0.0006	0.0006
	(-0.03)	(0.02)	(-0.03)	(0.02)	(-0.21)	(0.47)	(0.29)
Loss	-0.0027**	-0.0027**	-0.0027**	-0.0027**	-0.0011	-0.0059***	-0.0081***
	(-2.33)	(-2.33)	(-2.32)	(-2.32)	(-1.45)	(-3.06)	(-3.37)
Leverage	0.0002	0.0002	0.0002	0.0002	-0.0001	0.0007	-0.0001
	(0.21)	(0.21)	(0.22)	(0.21)	(-0.22)	(0.75)	(-0.11)
Volatility	0.0009	0.0009	0.0009	0.0009	-0.0014*	0.0008	0.0010
	(0.73)	(0.73)	(0.72)	(0.72)	(-1.92)	(0.31)	(0.38)
Institutional Ownership	0.0007	0.0007	0.0007	0.0007	0.0003	0.0009	-0.0004
	(1.05)	(1.06)	(1.05)	(1.07)	(0.62)	(0.86)	(-0.40)
Big 4	-0.0046***	-0.0046***	-0.0047***	-0.0046***	-0.0013	-0.0032	-0.0045
	(-2.79)	(-2.74)	(-2.80)	(-2.81)	(-1.21)	(-1.44)	(-1.44)
Auditor Tenure	0.0015***	0.0015***	0.0015***	0.0015***	0.0004	0.0020***	0.0028***
	(3.21)	(3.19)	(3.19)	(3.20)	(1.44)	(3.36)	(3.20)
Analyst Following	0.0010	0.0010	0.0010	0.0010	0.0006	0.0016	0.0024**
	(1.33)	(1.32)	(1.34)	(1.32)	(1.37)	(1.51)	(2.02)
Bog Index	0.0005	0.0005	0.0005	0.0004	0.0005	0.0002	-0.0004
	(0.65)	(0.65)	(0.63)	(0.63)	(1.31)	(0.18)	(-0.33)
Year Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Restatement Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	55,993	55,993	55,993	55,993	55,993	55,983	55,949
Adjusted R^2	0.0646	0.0646	0.0647	0.0648	0.0894	0.0636	0.0773

Wald Test: *Tainted Firm = Tainted Office*	3.50*	[0.07]
Wald Test: *Tainted Firm = Tainted Partner*	13.47***	[0.00]
Wald Test: *Tainted Office = Tainted Partner*	16.96***	[0.00]

This table examines the equity market reaction of accounting restatements to non-restating clients. The dependent variable for columns 1 through 4 is *CAR* [-2,+2], the five-day cumulative abnormal return centered on the restatement announcement date. The dependent variable for columns 5 through 7 is *CAR* [-1, +1], *CAR* [-2, +5], and *CAR* [-2, +10], respectively. The variable of interest is either *Tainted Firm*, *Tainted Office*, or

Tainted Partner. *Tainted Partner* is an indicator variable that equals one if the client was audited by a tainted partner, and zero otherwise. *Tainted Office* and *Tainted Firm* are measured in an identical way at the office- and audit firm-level. All other variables are defined in Appendix A. Industry fixed effects are based on 2-digit SIC codes. *t*-statistics are shown in parentheses below the estimated coefficients and use standard errors that are clustered two-way by industry and restatement announcement date. The *F*-statistics and *p*-values from Wald tests of coefficient equality for column 4 are presented at the bottom of the table. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

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Table 5
Reissuance Versus Revision Restatement Effects

Restatement Type:	Reissuance (Big R) Restatements				Revision (little r) Restatements			
Dependent Variable:	*CAR* (-1, +1)	*CAR* (-2, +2)	*CAR* (-2, +5)	*CAR* (-2, +10)	*CAR* (-1, +1)	*CAR* (-2, +2)	*CAR* (-2, +5)	*CAR* (-2, +10)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tainted Partner	-0.0046*	-0.0111***	-0.0191***	-0.0272***	-0.0023	-0.0072**	-0.0085**	-0.0097*
	(-1.75)	(-3.68)	(-5.13)	(-4.12)	(-1.15)	(-2.33)	(-2.56)	(-1.87)
Size	0.0006	0.0032	0.0018	0.0007	-0.0015	-0.0021	-0.0048***	-0.0062**
	(0.42)	(1.59)	(0.81)	(0.24)	(-1.47)	(-1.35)	(-2.73)	(-2.15)
Market to Book	-0.0000	-0.0002	-0.0007	-0.0004	-0.0007**	-0.0004	-0.0005	-0.0001
	(-0.06)	(-0.29)	(-0.95)	(-0.38)	(-2.40)	(-0.88)	(-0.62)	(-0.10)
ROA	-0.0031	-0.0025	-0.0009	-0.0026	0.0006	0.0013	0.0011	0.0024
	(-1.63)	(-1.41)	(-0.59)	(-0.95)	(0.88)	(1.49)	(0.82)	(1.15)
Loss	-0.0051**	-0.0032	-0.0069**	-0.0096**	-0.0002	-0.0026**	-0.0056**	-0.0077***
	(-2.20)	(-1.32)	(-2.22)	(-2.11)	(-0.24)	(-2.21)	(-2.63)	(-3.01)
Leverage	-0.0002	-0.0005	-0.0007	-0.0010	0.0001	0.0004	0.0013	0.0002
	(-0.25)	(-0.70)	(-0.51)	(-0.44)	(0.14)	(0.37)	(0.97)	(0.15)
Volatility	-0.0017	-0.0010	-0.0002	-0.0021	0.0000	0.0022	0.0015	0.0032
	(-1.37)	(-0.58)	(-0.06)	(-0.53)	(0.01)	(1.35)	(0.64)	(1.12)
Institutional Ownership	0.0002	-0.0002	-0.0002	-0.0011	0.0005	0.0010	0.0013	-0.0003
	(0.22)	(-0.22)	(-0.22)	(-0.58)	(0.97)	(1.12)	(1.01)	(-0.22)
Big 4	-0.0056	-0.0076*	-0.0089	-0.0094	-0.0015	-0.0036*	-0.0009	-0.0028
	(-1.65)	(-1.70)	(-1.61)	(-1.06)	(-0.97)	(-1.93)	(-0.51)	(-0.89)
Auditor Tenure	0.0015*	0.0022*	0.0031**	0.0035**	0.0003	0.0013**	0.0016**	0.0025***
	(1.92)	(1.98)	(2.58)	(2.15)	(0.76)	(2.21)	(2.34)	(3.06)
Analyst Following	0.0008	0.0012	0.0015	0.0033	0.0008*	0.0011	0.0018	0.0024*
	(0.86)	(1.04)	(0.94)	(1.53)	(1.67)	(1.25)	(1.57)	(1.76)
Bog Index	0.0002	0.0003	0.0010	0.0017	0.0008	0.0005	-0.0002	-0.0012
	(1.18)	(0.31)	(0.71)	(1.01)	(1.50)	(0.71)	(-0.20)	(-1.01)
Year Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Restatement Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,628	16,628	16,627	16,619	39,365	39,365	39,356	39,330
Adjusted R^2	0.0739	0.0741	0.0873	0.1009	0.0599	0.0597	0.0534	0.0684
Wald Test: *Tainted Partner* $_{Big R}$ = *Tainted Partner* $_{little r}$					0.78	1.26	4.57**	4.43**
					(0.380)	(0.264)	(0.034)	(0.037)

This table examines the equity market reaction to accounting restatements separately for Big R and little r restatements. Columns 1 through 4 (5 through 8) present the results using Big R (little r) restatements. The dependent variables for columns 1 through 4 (5 through 8) are *CAR* [-1,+1], *CAR* [-2,+2], *CAR* [-2,+5], and *CAR* [-2,+10]. The variable of interest is *Tainted Partner,* an indicator variable that equals one if the client was audited by a tainted partner, and zero otherwise. Industry fixed effects are based on 2-digit SIC codes. *t*-statistics are shown in parentheses below the estimated coefficients and use standard errors that are clustered two-way by industry and restatement announcement date. The *F*-statistics and *p*-values from Wald tests of coefficient equality across the types of restatements are presented at the bottom of the table. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 6
Big 4 Versus Non-Big 4 Restatement Effects

Dependent Variable:	CAR (-2, +2)	
Auditor:	Big 4	Non-Big 4
	(1)	(2)
Tainted Partner	-0.0054**	-0.0103***
	(-2.18)	(-3.20)
Size	-0.0014	-0.0019
	(-1.05)	(-1.21)
Market to Book	-0.0002	-0.0007
	(-0.42)	(-1.03)
ROA	0.0003	-0.0000
	(0.31)	(-0.00)
Loss	-0.0028*	-0.0033
	(-1.71)	(-1.56)
Leverage	0.0010	-0.0009
	(0.73)	(-1.12)
Volatility	0.0027*	-0.0001
	(1.74)	(-0.09)
Institutional Ownership	0.0009	-0.0005
	(1.23)	(-0.50)
Auditor Tenure	0.0022***	-0.0007
	(4.07)	(-0.82)
Analyst Following	0.0006	0.0037***
	(0.75)	(3.09)
Bog Index	-0.0001	0.0012
	(-0.15)	(1.22)
Year Fixed Effects:	Yes	Yes
Industry Fixed Effects:	Yes	Yes
Restatement Fixed Effects:	Yes	Yes
Observations	34,163	21,830
Adjusted R^2	0.0488	0.0780
Wald Test: *Tainted Partner* $_{Big\ 4}$ = *Tainted Partner* $_{Non-Big\ 4}$		1.47
		(0.228)

This table examines the equity market reaction of accounting restatements separately for Big 4 and non-Big 4 audit firms. Columns 1 and 2 examine the effects based on whether the restating client was audited by a Big 4 or non-Big 4 firm. The dependent variable, *CAR* [-2,+2], is the five-day cumulative abnormal return centered on the restatement announcement date. The variable of interest is *Tainted Partner*, an indicator variable that equals one if the client was audited by a tainted partner, and zero otherwise. Industry fixed effects are based on 2-digit SIC codes. *t*-statistics are shown in parentheses below the estimated coefficients and use standard errors that are clustered two-way by industry and restatement announcement date. *F*-statistics and *p*-values from Wald tests examining coefficient equality across the types of auditors are presented at the bottom of each panel. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

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Table 7
Additional Analyses

Panel A: Market Learning Analysis

Dependent Variable:	CAR (-2, +2)			
Sample Period:	**2017**	**2017 – 2018**	**2017 – 2019**	**Full Period**
	(1)	(2)	(3)	(4)
Tainted Partner	**0.0131**	**-0.0037**	**-0.0070***	**-0.086***
	(0.91)	**(-0.75)**	**(-1.88)**	**(-4.02)**
Controls:	Yes	Yes	Yes	Yes
Fixed Effects:	Yes	Yes	Yes	Yes
Observations	6,064	15,638	25,735	55,993
Adjusted R^2	0.0576	0.0441	0.0398	0.0647

Panel B: Cross-Sectional Analyses

Dependent Variable:	CAR (-2, +2)					
Analysis:	Client Characteristics				**Multiple Events**	**Fresh Look**
Variable of Interest:	*Size*	*ROA*	*Volatility*	*Accruals*		
	(1)	(2)	(3)	(4)	(5)	(6)
Above Median	-0.0056**	-0.0055***	-0.0127***	-0.0099***		
	(-2.56)	(-2.81)	(-3.29)	(-4.59)		
Below Median	-0.0118***	-0.0117***	-0.0046***	-0.0065		
	(-3.40)	(-3.65)	(-2.90)	(-1.65)		
Initial Restatement					-0.0126**	
					(-2.60)	
Subsequent Restatement					-0.0072***	
					(-3.05)	
Associated Partner						-0.0152**
						(-2.10)
New Partner						-0.0076***
						(-3.02)
Controls:	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:	Yes	Yes	Yes	Yes	Yes	Yes
Observations:	55,993	55,993	55,993	50,341	55,993	55,993
Adjusted R^2	0.0647	0.0647	0.0647	0.0700	0.0647	0.0647
Wald Test: $\beta_1 = \beta_2$	2.73	3.57*	4.14**	0.75	1.01	0.83
	(0.103)	(0.064)	(0.046)	(0.390)	(0.318)	(0.365)

This table presents the results of the estimation of several additional analyses. Panel A examines the partner effect over time using alternative periods. Throughout the table, the dependent variable, *CAR* [-2,+2], is the five-day cumulative abnormal return centered on the restatement announcement date and the variable of interest is *Tainted Partner*, an indicator variable that equals one if the client was audited by a tainted partner, and zero otherwise. Columns 1 through 4 present the results using data from 2017, 2017 through 2018, 2017 through 2019, and finally using the full sample. Panel B presents cross-sectional analyses. In columns 1 through 4, the variables of interest are *Above Median*, an indicator variable that equals one if the client was audited by a tainted partner and the client's *Size*, *ROA*, *Volatility*, or *Accruals* value is above the *Tainted Partner* sample's median, and *Below Median*, an indicator variable that equals one if the client was audited by a tainted partner and the examined characteristic is below the sample's median. The variables of interest in column 5 are *Initial Restatement*, which is an indicator that equals one if the client was audited by a tainted partner and the associated restatement is the initial restatement the partner is associated with, and *Subsequent Restatement*, which is an indicator that equals one if the client was audited by a tainted partner and the associated restatement is not the initial restatement the partner is associated with. The variables of interest in column 6 are *Associated Partner*, which is an indicator variable that equals one if the client was audited by a tainted partner and the tainted partner is the client's current auditor, and *New Partner*, which is an indicator that equals one if the client was audited by a tainted partner and the tainted partner is not the client's current auditor. Industry fixed effects are based on 2-digit SIC codes. The *F*-statistics and *p*-values from Wald Tests of coefficient equality are presented at the bottom of the table. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 8
Sensitivity Tests

Panel A: Alternative Control Group Comparisons

Dependent Variable:	CAR (-2, + 2)			
Control Group(s):	**Baseline**	**Audit Firm & Office**	**Audit Office**	**CBSA Clients**
	(1)	(2)	(3)	(4)
Tainted Partner	**-0.0077*****	**-0.0086*****	**-0.0086*****	**-0.0060****
	(-3.33)	**(-3.26)**	**(-2.75)**	**(-2.58)**
Tainted Office	0.0024	0.0014*		0.0006
	(1.19)	(1.99)		(0.91)
Tainted Firm	0.0005			0.0002
	(0.34)			(0.19)
Controls:	Yes	Yes	Yes	Yes
Fixed Effects:	Yes	Yes	Yes	Yes
Observations	55,993	40,213	12,601	70,053
Adjusted R^2	0.0648	0.0735	0.0527	0.0871
Wald Tests:				
T. Partner = T. Office	16.96***	14.18***		6.99**
	(0.000)	(0.000)		(0.010)
T. Firm = T. Partner	13.47***			8.72***
	(0.001)			(0.004)
T. Firm = T. Office	3.50*			0.16
	(0.066)			(0.691)

Panel B: Alternative Control Group Observations

Dependent Variable:	CAR (-2, +2)				
# of Control Observations:	**10**	**25**	**50**	**100**	**None**
	(1)	(2)	(3)	(4)	(5)
Tainted Partner	**-0.0087*****	**-0.0085*****	**-0.0087*****	**-0.0084*****	**-0.0088*****
	(-3.86)	**(-4.05)**	**(-3.88)**	**(-3.88)**	**(-3.95)**
Controls:	Yes	Yes	Yes	Yes	Yes
Fixed Effects:	Yes	Yes	Yes	Yes	Yes
Observations:	30,269	50,292	76,079	117,428	241,590
Adjusted R^2	0.0647	0.0645	0.0586	0.0414	0.0375

This table presents the results of several sensitivity tests examining the robustness of our main results. Panel A examines the results using several different control groups. Column 1 presents the main results while column 2 (3) removes the industry (audit firm) control group. Column 4 replaces industry peers with a sample of companies operating in the same CBSA as the restating client. The dependent variable, *CAR* [-2,+2], is the five-day cumulative abnormal return centered on the restatement announcement date. The variable of interest is *Tainted Partner* which is an indicator variable that equals one if the client was audited by a tainted partner, and zero otherwise. The *F*-statistics and *p*-values from Wald tests of coefficient equality are presented at the bottom of the panel. Panel B examines the sensitivity of our main results to the number of control observations that we retain. Columns 1 through 5 present the results when we restrict our firm and industry peer control groups to no greater than 10, 25, 50, 100, and, finally, no restriction on the number of observations per restatement event. Throughout the table, industry fixed effects are based on 2-digit SIC codes and ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.