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Mandatory Audit Partner Rotations and Audit Quality in the United States

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Mandatory Audit Partner Rotations and Audit Quality in the United States

Abstract: This study uses a sample of mandatory partner rotation events hand-collected from SEC filings to investigate the relation between mandatory audit partner rotation and audit quality in the United States. Across a variety of control groups and audit quality proxies, we do not find evidence consistent with rotation materially improving audit quality (i.e., "fresh look"). Though somewhat limited, the only statistically significant evidence we document suggests that audited financial statements may be more likely to contain a material misstatement (i.e., subsequently be restated) following a mandatory audit partner rotation, particularly when the audit firm tenure is short. We also provide evidence from client disclosures that mandatory rotation rules trigger auditor-client realignment. Together, our results provide important evidence on the merits of mandatory partner rotation rules in the United States.

Keywords: U.S. mandatory audit partner rotations; material misstatements; audit quality.

JEL Classifications: M40, M41, M42

Data Availability: Data are publicly available from sources identified in the article.

I. INTRODUCTION

Section 203 of the Sarbanes-Oxley Act (SOX) requires audit engagements of publicly traded companies to change the lead (and concurring) engagement partner(s) at least every five years. Thus, if a lead engagement partner has served for five consecutive years in that capacity, the audit firm must assign a new lead engagement partner to that client. Proponents of mandatory partner rotation contend that the new lead audit partner provides a fresh perspective to the audit engagement while retaining client-specific knowledge from previous audits that would be lost under a mandatory *audit firm* rotation regime. For this reason, many consider mandatory audit partner rotation a key element for maintaining the independence that is necessary for high-quality audits. However, with the "fresh look" also comes a "learning curve" for the new engagement partner, which may cause a temporary decline in audit quality or a deterioration in the auditor-client relationship as partner-specific knowledge about the client is lost on the transition.

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Because partner identities for U.S. audit engagements have historically not been disclosed publicly, research on the effects of *mandatory* partner rotation and partner tenure on audit quality in the United States is scant.[1] PCAOB Auditing Standard AS 3101.10 requires that public accounting firms, not the individual audit partners, sign audit reports in the United States; thus, without access to proprietary data, it has been extremely difficult to identify when audit partner rotation occurs. Extant research on the effects of partner rotation either uses proprietary data (Gipper, Hail, and Leuz 2019, hereafter GHL), uses publicly available data from comment letters to infer rotation (Laurion, Lawrence, and Ryans 2017, hereafter LLR), which cannot distinguish between mandatory and voluntary rotation, or examines audits of non-US firms (e.g., Stewart, Kent, and Routledge 2016; Lennox, Wu, and Zhang 2014; Chi, Huang, Liao, and Xie 2009). In this study, we implement a novel approach that uses publicly available data to identify audit partner rotations that occur due to mandated audit partner term limits. To do so, we analyze over 1.3 million SEC filings and identify instances when companies disclose in their proxy statement or 8-K filings that the lead external audit partner will rotate to comply with the five-year audit partner term limit as specified by Sox Section 203. We also use an expanded mandatory rotation sample identified using a modification of the comment letter approach from LLR to identify partner changes that occur after five years of partner tenure. Using this data, we investigate the relation between *mandatory* partner rotations and audit quality using a variety of audit quality proxies. We find no evidence suggesting that the new audit partner provides a "fresh look" that improves audit quality. In fact, we find limited evidence that audited financial statements are more likely to be materially misstated (i.e., subsequently restated) in the initial

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[1] We note that the recent requirement to file a Form AP with the PCAOB results in the disclosure of lead engagement partner identities. However, Form AP data does not indicate when audit partner rotations are mandatory. Thus, until Form AP filings have been around for at least five years, it will not be clear when rotations are mandatory.

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year(s) following mandatory audit partner rotation than in the terminal year(s) of partner tenure, particularly when audit firm tenure is short. However, this evidence should be interpreted in light of the limited sample size and infrequency of material restatements. Nonetheless, the weight of our evidence is not consistent with partner rotation yielding material improvements to the audit process due to the "fresh look" of the new audit partner.

Our study makes several important contributions to the literature. We are the first study to our knowledge to use publicly available data to directly explore the effects of mandatory lead audit partner rotation in the United States as required by SOX Section 203. Second, we present a new way to identify mandatory partner rotations for future research interested in the effects of mandatory audit partner rotation. The audit firms and PCAOB do not provide engagement level data to the broad research community regarding mandatory audit partner rotations, so our study identifies an innovative way in which researchers may identify mandatory partner rotations even without access to proprietary data. We find that partner rotation disclosures are increasing in recent years, providing a resource for researchers interested in the effects of mandatory audit partner rotation before the Form AP filings have been disclosed for five years when researchers can infer mandatory rotations. Finally, we find that some clients change auditors at the end of a partner's tenure due to the audit firm's inability to comply with partner rotation rules, suggesting an additional cost of mandatory rotation rules.

While our results provide important insights, they are subject to several caveats. First, the companies in our sample are larger and have longer audit firm tenure than the average U.S. public company. As such, the conclusions from this study may not generalize to all companies. Second, while our sample of rotation events is similar to prior research (LLR), statistical insignificance in some settings may be the result of a lack of statistical power. As such, we

follow the advice in Cready, He, Lin, Shao, Wang, and Zhang (2019) and evaluate the evidence

in light of the 95 percent confidence intervals.

II. BACKGROUND AND RESEARCH QUESTIONS

Background

Regulators and standard setters have long expressed concerns that economic and social

bonds between the audit partner and the client may increase over the length of a relationship,

possibly diminishing the audit partner's professional skepticism and independence. For example,

an audit partner might fear that disagreeing with an audit client's financial reporting decisions

will result in losing the audit engagement and its fees, adversely affecting the audit partner's

portfolio of clients and possibly the partner's compensation and standing in the firm (Nelson

2009). As the partner's tenure with a client increases, their economic dependence may likewise

increase, particularly if the client represents a large portion of the partner's "book of business" or

a significant contribution to the audit firm or audit office's bottom line.

In addition to career or fiscal motives, an audit partner might feel pressure not to

disappoint a client when strong social ties have developed between the partner and the client.

Strong social ties could make the audit partner hesitant to push back on the client's accounting

decisions (Bazerman, Moore, Tetlock, and Tanlu 2006), particularly when the treatment involves

a significant amount of judgment. Regulators and standard setters have implemented lead partner

rotation rules to mitigate such concerns about independence impairments. The first Chief

Accountant of the SEC, Carman Blough, suggested that requiring audit supervisors and lead

partners to change on a regular basis might limit bonding between auditors and clients (Blough

1951). In response to the Metcalf Report's recommendation of mandatory audit firm rotation, the

American Institute of Certified Public Accountants (AICPA) introduced a requirement limiting

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the tenure of lead audit partners on SEC client engagements to seven consecutive years (AICPA 1978). SOX Section 203 reduced the number of consecutive years an audit partner may serve as the lead or concurring audit partner to five years for public companies and extended the cooling off period to five years. While the term limit has not changed since the inception of SOX, the PCAOB has suggested that partner rotation may not sufficiently address independence problems and has considered instituting mandatory audit *firm* rotation (Doty 2011).

Audit Quality

Proponents of mandatory rotation rules often contend that the new audit partner provides a "fresh look" on the engagement that can identify overlooked aspects of the audit, thereby improving audit quality. In theory, the new lead audit partner provides a new perspective on the existing audit program and might be able to identify insufficient or non-existent procedures necessary for a high-quality audit (Lennox and Wu 2018). Partner rotation (as opposed to audit firm rotation) does not sacrifice all of the cumulative audit knowledge and experience gained by the external audit firm when the new partner rotates on the engagement.

For the most part, empirical studies on the effects of audit partner rotation examine data from countries such as Australia (Stewart, Kent, and Routledge 2016), China (Lennox, Wu, and Zhang 2014), and Taiwan (Chi, Huang, Liao, and Xie 2009), as these countries require the disclosure of audit partner identities (see Lennox and Wu 2018 for a more comprehensive review of this literature). However, the degree to which conclusions drawn from studies of foreign audit firms generalize to U.S. audit firms is somewhat limited. First, the litigation environment, coupled with the strong regulatory oversight in the United States, incentivize audit partners to maintain independence, even absent impending rotation. Thus, the benefits of a mandatory rotation regime are likely lower in the United States. Second, the large U.S. public accounting

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firms (with the majority of public clients) are substantially larger than the largest public

accounting firms in other countries; thus, the firms have significantly more potential partners

with public company auditing experience in the relevant industry that can rotate onto a vacant

engagement. Thus, the implications of partner rotation likely differ between U.S. based audit

engagements and those conducted internationally.

Further highlighting the need for additional study into the ramifications of audit partner

rotation is the fact that the findings of prior research on the subject are inconsistent. For example,

Lennox et al. (2014) examine the "fresh look" argument using proprietary data on audit

adjustments in the setting of mandatory rotation of Chinese audit partners. Their findings suggest

improvements in the quality of the audits performed in the year preceding *and* the initial year

following a required rotation. They conclude that mandatory rotation in China improves audit

quality through both a fresh perspective of the new partner and by disciplining the outgoing audit

partner to provide high audit quality in the year preceding a rotation (i.e., review effect).

However, using data from Taiwan, Chen, Lin, and Lin (2008) find that firm-level discretionary

accrual levels decrease as audit partner tenure increases, suggesting that longer-tenured audit

partners provide higher quality audits. Likewise, Chi et al. (2009) find that Taiwanese audit

quality is significantly lower in the year of mandatory partner rotation. The combination of

location-specific differences in findings (i.e., differences between studies using data from

Taiwan and China) as well as the noted differences between the U.S. regulatory and legal

environment and that of foreign jurisdictions highlights the need for research in the area of

mandatory audit partner rotations in the United States.

Research on the relation between audit partner rotation (and by extension audit partner

tenure) and audit quality within the U.S. setting is rather limited. Using a hand-collected sample

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of public companies, Manry, Mock, and Turner (2008) find an inverse relationship between discretionary accrual levels and audit partner tenure among small companies but find no relationship among large companies. Under the assumption that audit firms only change audit partners when required by the Section 203 mandate, Litt, Sharma, Simpson, and Tanyi (2014) identify the fifth consecutive fiscal year following an auditor switch by a public company and assume that audit partner rotations occur following this year. Their findings suggest that a reduction in financial reporting quality accompanies the assumed change in the audit partner. Likewise, Sharma, Tanyi, and Litt (2017) adopt a similar approach and find that the initial year following assumed rotation events is associated with higher fees and a longer audit report lag.

A recent study by LLR infers engagement partner identity by using the names of audit firm personnel copied on comment letter correspondences between the SEC and U.S. public companies. The authors find mixed evidence on the benefits of changing audit partners. They find no relation between partner rotation and the likelihood that the financial statements contain a misstatement. However, they do find an increase in the frequency of restatement announcements and an increase in deferred tax allowances following a rotation event. They conclude that their results provide "some evidence suggesting that U.S. partner rotations support a fresh look at the audit engagement" (pg. 209).

We view our study as an extension of LLR due to our ability to address certain limitations the authors note in their study. First, the authors must assume that the accounting firm personnel copied on the SEC comment letter responses reflect the *current* lead audit partner on the engagement, as opposed to other audit firm employees (e.g., concurring partner, client relationship partner, office managing partner, prior period audit partner). Our primary sample does not use comment letters and, therefore, does not rely on such an assumption. Second, the

authors note that "endogenous partner rotations are a concern in our setting because we cannot observe whether a partner rotation is mandatory or voluntary" (pg. 211). If low financial reporting/audit quality, which might be more likely for audit engagements receiving an SEC comment letter, triggers voluntary audit partner rotation events (i.e., rotations before reaching the five-year engagement partner term limit), this would bias evidence towards supporting the "fresh look" hypothesis. This issue is important in light of the findings of the current working paper by GHL, which suggests that 38 percent of all audit partner rotations are non-mandatory *and* that poor audit quality tends to precede voluntary partner rotations. Finally, because client firms often do not receive SEC comment letters in consecutive years, there are instances where LLR cannot determine whether the rotation event occurred in year *t* or *t-1* and must make assumptions about the partner transition year. Our primary approach for identifying mandatory rotation events addresses some of these limitations by using audit committee disclosures to identify known mandatory audit partner rotations as well as the exact year of the rotation.[2] However, it is important to acknowledge that this approach is not without its limitations. Unlike comment letters, the rotations identified in this study are a result of a firm disclosure choice. The endogenous nature of the disclosure may affect the inferences of our findings, as well as the generalizability of our results. We discuss these issues in depth in Section 3. For this reason, we do not suggest that our approach is "better" than that of LLR. Rather, we adopt an alternative approach that identifies a new set of more than 100 *mandatory* rotation events in the U.S. that has its own strengths and weaknesses. We suggest that our paper represents a valuable complement to LLR, whereby our results can be evaluated in conjunction with theirs.

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[2] Our expanded sample uses comment letters to identify additional rotation events. While we too must make assumptions about audit firm personnel copied on comment letter correspondences, we ensure that the timeline of correspondences indicates mandatory rotation, reducing the likelihood that we identify voluntary rotations.

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A current working paper by GHL explores the effects of mandatory partner rotation in the United States using proprietary engagement data provided by the PCAOB. While GHL primarily focus on the economic effects of audit partner tenure on firms in terms of audit fees and audit hours, GHL also perform limited tests on the effects of partner tenure on audit quality (Table 3). GHL do not directly compare year before to year after partner rotation when assessing rotation effects on *audit quality*. Rather, they include a partner tenure count variable and do not find evidence of an average linear relation between partner tenure and audit quality (they do find some evidence of a negative relation between restatement announcements and partner tenure). GHL do document that audit hours decrease, and audit fees increase over the tenure cycle, suggesting economic benefits to *audit firms* of prolonged partner tenure. The proprietary data affords GHL a large sample in which they can identify mandatory rotation events with a high degree of confidence. Our study differs in two important respects. First, our paper focuses primarily on audit quality effects and considers the effects in the initial year following rotation while GHL investigate the linear relation between partner tenure and quality. Second, we use publicly available data, which aids in the replication and extension of our work by future studies. This aspect is of particular importance as it will be several years before the partner identity disclosures required in Form AP will be in place long enough for researchers to identify mandatory rotations using Form AP data. We find that the disclosure of mandatory audit partner rotations is increasing in recent years, so researchers adopting our approach should have increasingly greater data on mandatory rotation in the future.

As our literature review suggests, prior literature lacks evidence of the relation between *mandatory* audit partner rotation and audit quality in the U.S. setting. Our study intends to address this gap in the literature, leading to our first research question.

RQ1: What are the effects of mandatory audit partner rotation on audit quality?

We do not make a directional prediction as to the effects of partner rotations on audit quality. On the one hand, the audit firm loses important engagement-specific experience generated by the outgoing audit partner due to the rotation. At the same time, the incoming audit partner faces a steep learning curve when becoming acquainted with client management, accounting idiosyncrasies, and possibly the audit team personnel. Thus, audit quality may decline in the initial year(s) following an audit partner rotation. On the other hand, the new audit partner might provide a fresh perspective on the engagement, which could identify limitations or shortcomings in the current audit program. Furthermore, if familiarity with the client breeds complacency or an unwillingness to stand up to management, then partners at the end of their rotation periods may provide lower quality audits. Alternatively, a "lame-duck" partner may find it easier than a new partner to stand up to the client due to heightened independence (similar to the finding relating to "lame-duck" audit firms in Cassell, Myers, Seidel, and Zhou 2016). Finally, PCAOB and firm oversight promote high audit quality across the board, which may result in more consistent audit quality that is relatively unaffected by rotation.

We also stress that a lack of difference in audit quality between mandatory rotation years and non-rotations years does not necessarily indicate that the length of partner tenure does not affect audit quality. First, five years is a relatively short tenure, so the benefits (or costs) of extended partner tenure may not fully accrue in the current allowable auditor tenure. Second, the "review effect" documented by Lennox et al. (2014), suggests that partner rotation may influence audit quality through a deterrent effect. If so, the reporting quality observed in the terminal years of the partner tenure in a mandatory rotation regime may not reflect the quality provided in a regime that does not limit the tenure of the lead engagement partner.

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Audit Fees

An audit firm may set the initial year's audit fee below the expected fee of all other accounting firms to attract new audit clients (i.e., low-balling). In the extreme, this might mean the audit firm performs the initial year's audit at an economic loss. A firm may incur this loss with the expectation that they can recoup the loss through increased prices and more efficient audits in subsequent years. A variety of low-balling related topics have been examined, including whether low-balling exists, the determinants of the degree to which accounting firms are willing to low-ball the audit price, how long the lower-than-expected audit pricing persists, and the association between low-balling and independence (Deis and Giroux 1992; Ettredge and Greenberg 1990; Simon and Francis 1988; Turpen 1990).

While the previously noted literature documents the practice of low-balling to attract new clients to an accounting firm, it is unclear whether accounting firms employ the tactic for client retention. Because partner rotation carries some of the switching costs associated with switching audit firms, the marginal cost of switching auditors is lowered for clients experiencing a partner rotation. The reduction in switching costs might increase the likelihood that clients about to experience a partner rotation will solicit bids from alternative audit service providers. In fact, the 2016 DEF 14A filing for Red Robin Gourmet Burgers, Inc. provides anecdotal evidence of the perceived reduction in costs of switching audit firms when faced with an impending mandatory partner rotation event (see Appendix D, Panel B). Per the filing dated April 5, 2016, the audit committee cites an impending mandatory rotation of the current audit partner as justification for soliciting bids from other external auditors (we note that they ended up changing auditors in 2016). If mandatory rotations cause an audit client to solicit bids from other auditors, audit firms might feel compelled to "low-ball" the audit fees in the initial year of a partner's tenure.

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Conversely, a "fresh look" at the audit engagement may lead to an increase in audit hours, causing fees to increase in the initial year of a partner's tenure - *if* the auditor can pass additional fees on to the client. Likewise, as the partner's tenure on an engagement increases, the auditor may be able to charge higher audit fees. This leads us to our second research question.

RQ2: What are the effects of a mandatory audit partner rotation on the audit fees?

III. DATA AND RESEARCH DESIGN

Data

We identify lead engagement partner rotation events using the SEC's EDGAR repository. Because SOX limits the allowable partner tenure to five years, our sample begins following the introduction of SOX, in 2003. We begin by searching EDGAR for Form DEF 14A, Form DEFA 14A, and Form 8-K filings between 2003 and 2019, and identify over one million filings. For each filing, we search for the keywords "rotation", "rotate", or "rotated" and identify filings with these keywords. Within these filings, we search for the keywords "partner", "lead", or "engagement" and manually check these filings to assess the nature of the event. We assess whether 1) the filing references an actual lead audit partner rotation, 2) whether the filing indicates that the partner rotation was mandatory, and 3) if the filing discloses the rotation year.

While this approach allows us to know the nature (i.e., mandatory versus voluntary) and timing of partner rotation with nearly 100 percent confidence, this does result in a relatively small sample size. For example, LLR identify approximately 200 total rotation events with sufficient data to run their analysis using the comment letter approach, while our approach yields a sample of 136 mandatory rotation events after we apply data restrictions. As a result, we augment our sample with a variation of the comment letter approach designed specifically to isolate mandatory rotation events. We examine the timeline of each comment letter

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correspondence to identify instances when the timeline suggests that the departing partner served

a five-year term prior to rotation.[3] This approach identifies 35 additional rotation events with

sufficient data to suggest that the rotation is mandatory, resulting in 171 mandatory rotation

events. Because this approach relies on assumptions about the position of individuals copied on

comment letters and introduces concerns associated with comment letters potentially identifying

low audit quality years, we perform tests on both the primary sample, which excludes these

observations, as well as an augmented sample that includes these observations. We also employ a

third sample that includes year *t-2* and year *t+1* relative to rotation (similar to LLR). The

appendices provide examples of mandatory rotation disclosures (Appendix C, Panel A), partner

rotations where the nature was unclear, and therefore excluded from our analysis (Appendix C,

Panel B), as well as a discussion of the modified comment letter approach (Appendix E).

Research Design

      To estimate the relation between mandatory audit partner rotation and audit quality, we

estimate the following regression (Equation 1):

$$Audit\ Outcome_{it} = \beta_0 + \beta_1 Rotate_{it} + \beta X_{it} + \varepsilon_{it}$$

(1)

      Rotate equals one in the year (two years) following a mandatory rotation and zero in the

one (two years) before the rotation. Thus, our tests compare the year(s) of the longest partner

tenure to the year(s) of the shortest partner tenure. We use *Material Restate, Restate, Absolute*

Abnormal Accruals, and Audit Fees as outcomes in (1). We estimate the regression with controls

for client size, sales growth, inventory, foreign income, return on assets, as well as year fixed

effects (all continuous variables are winsorized at the 1 percent and 99 percent levels). β_1

estimates the difference in audit outcomes between rotation years and non-rotation years to test

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[3] See Appendix E for a detailed discussion on our strategy for implementing this approach.

RQ1 (audit quality) and RQ2 (audit fees). See Appendix A for variable definitions. We include

year fixed effects due to the time trend in these disclosures (see Table 1) as well as a time trend

in some audit quality proxies, in particular, restatements.

We do not include firm fixed effects in our primary analysis for several reasons. First, we

require each sample firm to have both pre- and post-rotation data, largely eliminating concerns

about omitted variable bias due to fixed unobservable traits since fixed traits would necessarily

be uncorrelated with *Rotate*.[4] Second, the inclusion of fixed effects greatly reduces the degrees of

freedom by adding approximately 100 variables to regressions, which inflates the standard errors

(Brooks 2008). Given that we have minimal concerns about bias, we present results without firm

fixed effects in the main analysis. Nonetheless, we present results of tests with firm fixed effects

in Appendix B. As expected, the coefficient point estimates are very similar to those without

firm fixed effects; however, the precision of those estimates is greatly reduced (standard errors

and consequently 95 percent confidence intervals are substantially larger). We discuss this

further in the additional analysis section of the paper.

IV. RESULTS

Sample and Descriptive Statistics

We present a year-by-year breakdown of the mandatory rotation years as identified in

SEC filings in Table 1. While we observe at least one mandatory rotation in each year, and six

rotations as early as 2003, we note a substantial increase in recent years, with 106 mandatory

rotation disclosures occurring between 2013 and 2018. In our reading of proxy statements, the

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[4] This does not mean that the decision to disclose a mandatory partner rotation is unrelated to fixed characteristics.
Rather, it means that fixed traits are unrelated to *Rotate*, since each firm is present when *Rotate = 1* and *Rotate = 0*.
If we compared rotation firms to non-rotation firms, the voluntary nature of this disclosure could introduce concerns
about omitted variable bias. As we discuss, the fact that our sample is determined based on a voluntary disclosure
could impact the generalizability of the sample, but it should not introduce bias into our estimates since every firm
in our sample makes such a disclosure.

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reason for the increase is not clear. However, the disclosures *do not* suggest that financial

reporting outcomes are driving this disclosure. We also present a year-by-year breakdown of the

expanded sample, which includes inferred mandatory rotation events based on a modified

comment letter approach. This approach identifies mandatory rotations as instances where the

client firm copies the same outgoing accounting firm professional for five consecutive years and

identifies an additional 35 rotation events (see Appendix E for explanation of approach).

[Insert Table 1 about here.]

We also present descriptive statistics of each of our samples in Table 2. As expected,

given that the same firms are present in the *Rotate = 0* and the *Rotate = 1* samples in each panel,

we find no statistically significant differences between groups based on *Rotate* in any sample.[5]

[Insert Table 2 about here.]

The Endogenous Decision to Disclose Mandatory Rotation

Two concerns arise due to the voluntary nature of the disclosure of lead partner rotation

in our main sample. First, the decision to disclose an impending partner rotation could be driven

by either poor quality in a prior period and the decision to signal a change *or* concerns that the

rotation will have an adverse effect on audit quality. In either case, prior or expected quality

drives the decision to disclose rotation, compromising the validity of tests. While we cannot rule

this out entirely, our reading of these disclosures (see examples in Appendix C) suggests that it is

unlikely to be the case for several reasons. First, none of the disclosures of partner rotation

reference either previous or expected audit quality, when discussing the rotation. In fact, the

disclosures nearly always briefly mention rotation without any additional discussion, suggesting

this is a statement of fact rather than a justification of prior audit quality or expected audit

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[5] Note that the sample size in these comparisons do not tie to one particular specification from the related multiple regression tests because the data requirements differ based on the tests (e.g., year restrictions for restatement tests, data restrictions to calculate accruals).

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quality. Second, 12 of the 122 unique firms in our primary sample, disclose rotations multiple times, suggesting the disclosure is sticky, and not driven by quality concerns.[6] Relatedly, if audit committees make these disclosures to justify a change from a high-quality audit partner, this could bias towards observing a decline in quality following rotation. We do not expect this factor to have a significant effect on our results for two reasons. First, the identity of the audit partner is not publicly known during our sample period, so we do not expect audit committees to justify partner changes to investors, as investors are unaware of the partner's identity. Second, we suggest that if the audit committee felt as though they were receiving a lower quality audit partner, they would be more likely to change auditors than accept a lower quality audit partner and disclose the partner rotation to justify the decline in audit quality.

It is possible that our sample firms do not generalize due to the voluntary nature of the disclosure. In an untabulated analysis, we note the firms that voluntarily disclose mandatory audit partner rotation tend to be larger, more profitable, more likely to enlist Big 4 auditors than non-disclosing firms, and have a longer auditor-client tenure on average – consistent with larger firms disclosing more information voluntarily. While the firm-years disclosing a mandatory rotation in the sample differ from the average in terms of size and profitability, they do not fall on the far extreme of the range. Nonetheless, we caveat that our results only generalize to the extent that rotation affects sample firms similarly to non-sample firms.[7]

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[6] It is possible that the outgoing partners for these repeat disclosers anticipate the disclosure and adjust behavior in a manner they would not if rotation were not disclosed. In untabulated analysis, we include only the first instance of a disclosure by each firm and inferences are unchanged.

[7] It is possible that companies disclosing this information have stronger corporate governance than non-disclosing firms. In untabulated analysis, we assess the generalizability of the corporate governance of disclosers to similar firms by matching sample firms to non-sample firms using propensity score matching (matching within year, without replacement on the full set of control variables). We find that disclosers do not significantly differ ($p>0.10$) in terms of total board size, CEO/Chairman duality, or audit committee tenure. However, the disclosers do have slightly more independent directors (8.0 vs. 7.4) and audit committee report length is significantly longer for the disclosers at 640 words compared to 441 words. While the magnitudes are not particularly large, we acknowledge that this may suggest that firms with stronger corporate governance are more likely to make these disclosures.

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Results

Pre/Post Rotation Comparison

We start with univariate comparisons of the audit outcomes for the year of partner rotation (year *t*) to the year before partner rotation (year *t-1*) for mandatory rotation disclosure sample and the expanded comment letter sample in Column 1 and Column 2 of Table 3 respectively. In Column 3, we expand the sample from Column 2 by including year *t-2* in the *Rotate=0* and year *t+1* in the *Rotate=1* sample, similar to LLR who include two years pre-rotation and two years post-rotation. This expansion yields a sample of over 500 observations.[8] Sample sizes within a given sample vary across the test variables for two reasons. First, a firm is excluded from a given analysis if it lacks the necessary data for the audit outcome in either the pre- or post-period (i.e., must have data in both periods). Second, for *Material Restate* and *Restate* tests, we exclude all observations after 2016 to allow time to detect the misstatement.[9]

[Insert Table 3 about here.]

Our first proxy for audit quality, *Material Restate*, represents *audited annual* financial statements that contain a material misstatement that management ultimately corrects through the issuance of restated financial statements and filing of an item 4.02 non-reliance 8-K.[10] Restatements are relatively unambiguous reflections of poor audit quality as they have low measurement error and indicate that the auditor issued an unqualified audit opinion on materially misstated financial statements (DeFond and Zhang 2014). However, *Material Restate* is

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[8] We note one downside of including year t+1, similar to LLR, is that we cannot know for certain that the partner did not switch in this year. However, changing partners the first year after a mandatory rotation is unlikely, so we expect this would have minimal effect on our inferences.

[9] In untabulated analysis, the average length between a year-end that is materially misstated and the restatement announcement is 697 days and the 75th percentile is 947 days for Audit Analytics restatements. As such, this approach will still fail to capture some misstatements that have not yet been detected and restated since the download occurred September 16, 2019 from Audit Analytics, which is 989 days after December 31, 2016. Inferences are similar if we impose a December 31, 2015 cutoff for restatements tests.

[10] Following Scholz (2014) we classify a misstatement as material when the restatements of *audited annual* financial statements result in the disclosure of a non-reliance 8-K item 4.02 filing (i.e., Big R misstatements).

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infrequent, so we also expand this definition to include all restatements, regardless of whether the firm filed an item 4.02 (*Restate*). Restatement rates with both proxies are higher in the initial year following rotation, but the difference is only marginally significant in one comparison (*Material Restate* in column 3) and insignificant in the others. While these tests are inconclusive with respect to the effect of rotation on audit outcomes, they are generally inconsistent with the "fresh look" hypothesis, which suggests that audit outcomes should improve following rotation (i.e., lower restatement rates).

One complication with restatements is that while a restatement reflects a low-quality audit, many low-quality audits may not result in a restatement. As such, the absence of a restatement does not necessarily indicate high audit quality. For this reason, we also use the absolute value of abnormal accruals (*Abnormal Accruals*) to proxy for audit quality. One benefit of accruals is that accruals are continuous and may capture within-GAAP manipulations that restatements do not capture (DeFond and Zhang 2014); this is particularly valuable in our small sample tests. Using the absolute value of abnormal accruals allows us to capture both income increasing and income decreasing earnings management. Furthermore, Aobdia (2019) finds that audits with PCAOB identified deficiencies have greater absolute abnormal accruals.[11] However, the construct validity of accruals is weaker than restatements, and they possess a great deal of measurement error. Furthermore, accruals made under the previous partner will reverse, limiting the extent to which differences will manifest themselves in accruals-based measures in a time-series setting such as this. We find no evidence that accruals differ between pre- and post-mandatory rotation (t-stat<0.70 in all specifications).

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[11] Aobdia (2019) does not find statistically significant evidence that signed abnormal accruals relate to PCAOB identified audit deficiencies, supporting the use of the unsigned measure (i.e., absolute value).

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Finally, we find no evidence of a difference between pre- and post-rotation audit fees. This could be because changing partners does not influence the number of audit hours *or* that the new partner *does increase* audit hours but does not pass these costs on to the client. In any case, we do not find evidence that the audit firm materially increases or decreases fees in the year(s) following rotation. If there is a learning curve for the new partner who performs additional audit procedures when partner rotation occurs, then the audit firm may suffer financially in the year of partner rotation if profit margins are squeezed. Because each *Rotate=0* event necessarily occurs in a year prior to a *Rotate=1* observation, year effects likely impact univariate tests, particularly for variables such as restatements, which vary significantly over time. As such, we next evaluate these findings using multiple regression in Table 4 that includes year fixed effects.

[Insert Table 4 about here.]

Panels A, B, and C present multiple regression analysis for the mandatory rotation disclosure sample, the expanded comment letter sample, and the expanded comment letter sample with two years pre- and post-rotation, respectively. Similar to univariate tests, these results continue to be inconsistent with auditor rotation providing a "fresh look" that materially improves audit quality. Specifically, all nine coefficients on *Rotate* in each audit quality regression (*Material Restate, Restate,* and *Abnormal Accruals*) are positive. Second, the coefficients are positive and statistically significant (p<0.10 two-tailed) in all three *Material Restate* specifications and in Panel A with *Restate* as the dependent variable. We display the 95 percent confidence interval for each estimate below the coefficient estimate and consider the interval consistent with the suggestions in Cready et al. (2019). Given the relatively small sample sizes (and infrequent occurrence of some dependent variables – e.g., *Material Restate*), the confidence intervals are somewhat large. However, with *Material Restate*, the lower bound of

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the confidence intervals are -0.006, -0.001, and +0.003 in Panels A, B, and C, respectively. This corresponds to an economic magnitude of under one percent in each sample. Thus, a reduction in the material restatement rate of one percent falls *outside* the 95 percent confidence level, inconsistent with the notion that rotation significantly improves audit quality.

For *Restate*, we document statistically significant evidence in Panel A, suggesting a possible *decline* in audit quality on the year of rotation, though the difference is not statistically significant in Panel B or C. The confidence interval is larger for *Restate* than that for *Material Restate*, so while the coefficient estimates are similar, the 95 percent confidence interval includes a wider range of estimates (lower bounds between 0.7 percent *increase* and 3.9 percent *reduction* in restatement rate and upper bounds between 10.6 and 13.4 percent *increase* in restatement rates). Because sample averages for *Restate* are around 10 percent, the lower bounds of the 95 percent confidence interval in Panel B and Panel C could be considered an economically meaningful reduction in the rate of restatements (audit quality improvements). Thus, while the confidence intervals all include zero, leaving open the possibility that effect size is zero (Cready et al. 2019), we do not definitively rule out audit quality improvements based on this analysis. Nonetheless, we do not find results supporting the conclusions of LLR, that the new auditor provides a fresh perspective on the audit.

For *Abnormal Accruals*, we find no compelling evidence of higher or lower accruals following rotation events with point estimates very near zero and zero again contained within all 95 percent confidence intervals. However, the width of the interval is quite a bit smaller than for restatements (relative to comparable benchmarks). For both restatements variables, the width of the confidence interval is substantially greater than the mean. For *Abnormal Accruals*, both the sample mean (about 0.038) and the standard deviation (0.038) are more than double the width of

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the confidence intervals (0.015, 0.013, and 0.011 for Panels A, B, and C respectively). Thus, these estimates appear relatively more precise. When evaluating the plausibility of audit quality improvements following rotation, we consider the lower bound of the confidence interval, which falls between -0.002 and -0.006. The lower bound of the confidence interval is less than 15 percent of the standard deviation and falls well below effect sizes documented in recent literature. For example, Bhaskar, Schroeder, and Shepardson (2019) document an effect size of an increase in discretionary accruals of 3.5 percent of assets for integrated audits relative to financial statement only audits in Table 6 and Carcello and Li (2013) document a decrease in abnormal accruals of 3.3 percent of assets associated with required partner audit report signatures in the UK in Table 3. Thus, while we again cannot "accept the null" of no effect, even the lower bounds of the 95 percent confidence intervals are far smaller than the effects of other audit attributes on *Abnormal Accruals* documented in the literature. This again, is inconsistent with a material "fresh look" by a new partner.

Finally, we consider whether audit fees differ following rotation. While the t-stats (<=0.30) and coefficient estimate magnitudes (<=0.012) are very small, the confidence intervals are fairly large (from - 0.07 to +0.07), so we draw no strong conclusions about the effects of rotation on audit fees.

Audit Firm Tenure and Partner Rotation

Audit firms with a longer tenure may be better positioned to cope with the disruption imposed on an engagement caused by partner rotation. For example, when a firm has audited a client for many years, the cumulative knowledge of the firm and the rest of the audit team may ease the transition to a new partner. Furthermore, firms with tenure greater than ten years could potentially have a prior partner rotate *back* to the engagement. Thus, it is possible that the effects

of tenure are pronounced in early years of firm tenure, but diminish as firm tenure increases. To test this possibility, we re-perform tests on our expanded sample (Table 4, Panel C), and interact *Rotate* with *Firm Tenure at Rotation*, which equals the audit firm tenure at the year of rotation – we demean *Firm Tenure at Rotation* to facilitate coefficient interpretation.[12] The results are presented in Table 5.

[Insert Table 5 about here.]

We detect a positive and significant coefficient on *Rotate* when *Material Restate* is the dependent variable, consistent with prior tests. Because we demean *Audit Firm Tenure at Rotation*, column 1 indicates that firms with mean *Audit Firm Tenure at Rotation* (22.4 years in our sample) are 5.0 percent more likely to have a material restatement of the current year's annual financial statements in the first two years following rotation than the two years preceding rotation. The coefficient on the interaction is negative, though it would only be statistically significant using one-tailed tests. The coefficient suggests that for each additional year of firm tenure at rotation, the increased likelihood of material restatement after rotation decreases by 0.2 percent. The standard deviation of *Audit Firm Tenure at Rotation* is 12.6 years, so the effects of rotation on restatements would be approximately cut in half at one standard deviation above the mean. Conclusions related to other variables are similar to Table 4. In particular, we find marginally significant evidence that *Restate* is more likely in the two years following rotation for a firm with mean *Audit Firm Tenure and Rotation,* though the interaction is insignificant (t-stat < 1.00). Coefficients and t-stats are very close to zero for *Abnormal Accruals* and *Audit Fee* tests on both the constituent effect and the interaction.

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[12] We interact *Firm Tenure at Rotation* with each of the control variables as well, following the suggestions in Swanquist and Whited (2018). Inferences are similar if we use either of the other two samples.

This provides preliminary evidence that audit firm tenure may mute the impact of partner rotation. We hesitate to draw strong conclusions on cross-sectional tests on an already limited sample. Furthermore, as noted previously, our sample has a relatively long auditor tenure on average. If auditor tenure does mute the effects of mandatory partner rotation, it could be the case that the effects of rotation are smaller in our sample than the broader population. As such, we suggest the interplay between audit partner rotation and audit firm tenure as a fruitful avenue for future research. In particular, does mandatory rotation early in an audit firm's tenure cause particularly strong disruptions to the auditor?

V. ADDITIONAL ANALYSIS

Alternative tests for "fresh look"

We use *Restate Material, Restate,* and *Abnormal Accruals* as our primary proxies for audit quality because they are relatively common proxies for audit quality in the literature. Nonetheless, in additional analysis, we add additional measures from LLR that could provide evidence of the "fresh look" effect of mandatory partner rotation: *Material Restate Announce, Restate Announce, Positive Special Items,* and *Negative Special Items*. First, we follow LLR and consider the announcements of restatements. Restatements and restatement announcements are inherently intertwined as a restatement announcement reveals the initial misstatement, which will be reflected in a restatement. We suggest that restatements provide stronger tests because, while an auditor is always responsible for preventing material misstatements, the detection of a misstatement may or may not involve the auditor. Thus, the theoretical link is stronger with restatements. Nonetheless, restatement announcements may reflect the "fresh look" if the "incoming audit partner identifies errors or inconsistencies" (LLR pg. 210). Second, we follow LLR and use special items as an alternative proxy. This measure is uncommon in auditing

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literature for several reasons. First, special items are primarily driven by the nature of the underlying event. While clients have some discretion as to which items to classify as a special item, most variation in special items will be unrelated to audit quality and likely difficult to capture with control variables. Thus, the infrequent use of special items as an audit quality measure is unsurprising. However, a client may be incentivized to classify income reducing (negative) items as special and to *not* classify income increasing (positive) items as special to give the appearance that negative events are transient and positive events are persistent. Thus, all else equal, a high quality and independent auditor may preclude a client from classifying negative events as special items and may be more likely to require a client to classify positive events as special items.

We re-perform prior tests using these alternative proxies on our largest sample. LLR document a significant result with *Restate Announce* and estimate that rotation increases *Restate Announce* by 5.1 percent. The *high end* of our 95 percent confidence interval (0.033) in Column 2 falls below this estimate, suggesting that power alone does not appear to explain the difference in results between our sample and LLR.[13] Coefficient estimates on *Rotate* in the remaining specifications are statistically insignificant, though the confidence intervals encompass values that would likely be considered economically significant. For example, the high end of the confidence interval for *Material Restate Announce* is 1.5 percent (0.015). Given the low mean for this variable (approximately 1 percent of observations), this effect size may be considered significant. The tests on these additional proxies fail to yield evidence of a significant "fresh look" from the incoming audit partner; however, we again urge the reader to interpret the results in light of the confidence intervals.

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[13] It is important to note that we do not conclude that LLR's results are invalid, rather our evaluation of our results suggests that power alone does not appear to drive the differences in our findings and LLR.

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[Insert Table 6 about here.]

Partner Tenure and Audit Quality

In prior tests, we consider the effects of partner rotation on audit quality. However, these effects are inextricably tied to the effects of *partner tenure* on audit quality since rotation inherently reduces partner tenure. By identifying known mandatory rotation events, we know the exact partner tenure for each of the five years preceding the rotation event (and the first year following the rotation).[14] To explore audit quality over partner tenure, we re-estimate equation (1) after replacing *Rotate* with a *Tenure* count variable (Panel A) as well as indicator variables for *Tenure One* through *Tenure Four* in Panel B (*Tenure Five* is always at zero because we relegate *Tenure Five* to the intercept). The tests here are generally consistent with prior tests, though far from conclusive. Specifically, we note a *negative* and marginally significant coefficient on *Tenure* in column 1, consistent with prior *Material Restate* tests (i.e., *decreasing* later in a partner's tenure). Similarly, with the year-by-year regressions, we note positive coefficients across all specifications for *Tenure* = 1 (marginally significant for *Material Restate* and significant for *Abnormal Accruals*). Together, while we find no conclusive evidence, the evidence documented is not consistent with either an impairment of audit quality as tenure increases, or a "fresh look" for new auditors improves audit quality.

[Insert Table 7 about here.]

The Effects of Mandatory Partner Rotation on Auditor Realignment

Although mandatory partner rotation is intended to preserve partner independence, a cost exists when the client and auditor prefer to retain the current client-partner relationship. In the

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[14] While we know that year *t-5* relative to the mandatory rotation event was the first year of a partner's tenure, we do not know whether it occurred due to mandatory or voluntary rotation; therefore, we exclude the observations five years before mandatory rotation from this sample so as not to commingle potential voluntary and mandatory rotation events.

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most extreme situations, a forced partner rotation can cause the client to change audit firms. It is not possible to know how many audit firm realignments occur due to mandatory partner rotation rules. However, anecdotally, we note that audit committees sometimes disclose a change in audit firm due to the auditor's inability to comply with mandatory rotation requirements. In our search for disclosures about mandatory rotation, we note 75 instances where the client discloses that a firm change is made because the auditor cannot comply with rotation regulations. We present two such examples in Appendix D, Panel A. As discussed above, we also note one instance where the audit committee discloses that it is soliciting bids from competing audit firms due to an impending mandatory partner rotation – we note that the client *does change* firms following this disclosure (see example in Appendix D, Panel B).

While these anecdotes do not directly inform the effects of mandatory rotation on audit quality, they do provide insights into the costs of current U.S. audit partner rotation rules. When mandatory rotation disrupts the desired partner-client relationship, a cost is imposed on both parties as they try to deal with these costs. In some cases, the costs exceed the benefits of retaining the audit firm, and the client switches auditors or possibly negotiates lower fees with the incumbent auditor as a condition of audit firm retention.

Client Fixed Effects

As noted above, we do not include client fixed effects in our main analysis because the requirement that a client be present in both the pre- and post-rotation period in our sample makes the effects of any client fixed traits essentially orthogonal to the treatment variable, *Rotate*, largely eliminating concerns about omitted variable bias. Nonetheless, we include fixed effects as an additional robustness test. As expected, we find results consistent with those in our main analysis; though also as expected, the standard errors are higher due to the significant loss of

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degrees of freedom given the small panel and large number of fixed effects. For reference, we tabulate the results with fixed effects in Appendix B.

VI. CONCLUSION

We use publicly available data to assess the effects of *mandatory audit partner rotation* on financial reporting outcomes in the United States. Utilizing firm disclosures from proxy statements and information on audit partner identities inferred from SEC comment letter correspondence, we identify a sample of 171 mandatory audit partner rotation events. We compare audit quality proxies in the one (two) year(s) following rotation to the one (two) year(s) preceding rotation for our known mandatory rotation events. Throughout a variety of samples and audit quality proxies, our evidence is inconsistent with a material improvement in audit quality following rotation (i.e., "fresh look"). On the contrary, we find some evidence that material misstatements are more likely to occur in the first year(s) following a mandatory audit partner rotation, though we urge caution given the limited sample and the low frequency of restatements. Nonetheless, our results provide important insights as the first study to our knowledge to use public data to isolate mandatory rotation events in the United States.

Our results are subject to several caveats. First, because partner identities were not publicly disclosed until recently, our sample size is limited, which limits the power of our analysis. Nonetheless, we find results that are generally consistent with a current working paper by GHL that utilizes proprietary data to assess the relation between partner tenure and audit quality. Consistent with our findings, they find no relation between partner tenure and accruals or financial restatements. One notable difference is that they do find a positive and significant relation between partner tenure and audit fees. A second caveat is that we identify our sample based on voluntary firm disclosures. If firms with strong corporate governance make these

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disclosures, the results may only generalize to firms with stronger governance, and the effects of

rotation on audit quality may differ for firms with weaker governance. Finally, we caveat that

even if our results can be generalized, they do not necessarily shed light on the full effects of

partner rotation. That is, if impending rotation encourages the outgoing partner to increase audit

effort as shown in Lennox et al. (2014), then we may not observe improved quality in the year of

partner rotation even if the regulation results in improved audit quality in general.

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Table 1: Rotations Disclosed by Year

Year	Mandatory Rotation Disclosure Sample	Firm Disclosure or Comment Letter (Expanded Comment Letter Sample)
2003	6	6
2004	4	4
2005	3	3
2006	2	2
2007	2	2
2008	2	2
2009	1	1
2010	1	1
2011	4	12
2012	1	4
2013	9	17
2014	10	18
2015	18	22
2016	24	24
2017	22	24
2018	23	25
2019	4	4
Total	**136**	**171**

Table 2: Descriptive Statistics

Panel A: Mandatory Rotation Disclosure Sample (t-1 and t)	*Rotate = 0*		*Rotate = 1*		
Variable	n	Mean	n	Mean	t-test
Size	136	8.968	136	9.013	0.189
Growth	136	0.072	136	0.060	-0.531
Inventory	136	0.081	136	0.081	0.074
Foreign Income	136	0.485	136	0.485	0.000
ROA	136	0.048	136	0.046	-0.169
Tenure at Rotation	136	22.074	136	22.074	0.000

Panel B: Expanded Comment Letter Sample (t-1 and t)	*Rotate = 0*		*Rotate = 1*		
Variable	N	Mean	n	Mean	t-test
Size	171	8.821	171	8.873	0.239
Growth	171	0.078	171	0.072	-0.295
Inventory	171	0.082	171	0.084	0.107
Foreign Income	171	0.509	171	0.491	-0.324
ROA	171	0.048	171	0.046	-0.219
Tenure at Rotation	171	22.363	171	22.363	0.000

Panel C: Expanded Comment Letter Sample (t-2, t-1, t, and t+1)	*Rotate = 0*		*Rotate = 1*		
Variable	n	Mean	n	Mean	t-test
Size	342	8.788	313	8.926	0.881
Growth	342	0.077	313	0.073	-0.317
Inventory	342	0.083	313	0.084	0.038
Foreign Income	342	0.491	313	0.508	0.428
ROA	342	0.046	313	0.042	-0.494
Tenure at Rotation	342	22.392	313	23.406	-1.028

Table 3: Rotation Firms Pre- and Post-Rotation: Univariate Analysis

This table presents the univariate comparison of audit quality proxies and audit fees around the rotation events for each of the three samples. [A]

Mandatory Rotation Disclosure Sample (t-1 and t)			**Expanded Comment Letter Sample (t-1 and t)**			**Expanded Comment Letter Sample (t-2, t-1, t, and t+1)**		
	n	**t-stat**		**n**	**t-stat**		**n**	**t-stat**
Material Restate [B]			**Material Restate [B]**			**Material Restate [B]**		
Rotate = 0	87	0.023	Rotate = 0	118	0.017	Rotate = 0	236	0.021
Rotate = 1	87	0.046	Rotate = 1	118	0.034	Rotate = 1	209	0.043
Difference		*0.023*	*(0.83)*	*Difference*	*0.017*	*(0.82)*	*Difference*	*0.022*	*(1.32)*
Restate [B]			**Restate [B]**			**Restate [B]**		
Rotate = 0	87	0.080	Rotate = 0	118	0.093	Rotate = 0	236	0.114
Rotate = 1	87	0.103	Rotate = 1	118	0.110	Rotate = 1	209	0.124
Difference		*0.023*	*(0.52)*	*Difference*	*0.017*	*(0.43)*	*Difference*	*0.010*	*(0.32)*
Abnormal Accruals			**Abnormal Accruals**			**Abnormal Accruals**		
Rotate = 0	109	0.036	Rotate = 0	137	0.037	Rotate = 0	274	0.040
Rotate = 1	109	0.037	Rotate = 1	137	0.039	Rotate = 1	246	0.037
Difference		*0.001*	*(0.23)*	*Difference*	*0.002*	*(0.43)*	*Difference*	*-0.003*	*(-0.64)*
Log Audit Fees			**Log Audit Fees**			**Log Audit Fees**		
Rotate = 0	132	15.078	Rotate = 0	167	14.988	Rotate = 0	329	14.962
Rotate = 1	132	15.138	Rotate = 1	167	15.049	Rotate = 1	305	15.094
Difference		*0.060*	*(0.43)*	*Difference*	*0.061*	*(0.48)*	*Difference*	*0.132*	*(1.40)*

A *To maintain a balanced sample, we require each firm to have at least one observation pre- and post-rotation for each sample.*
B *For restatement tests, we only exclude observations after 2016 to allow for time to detect and correct the misstated financial statements.*

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Table 4: Rotation Firms Pre- and Post-Rotation: Multiple Regression Analysis

The sample includes firm-years around mandatory rotation events: years *t-1* and *t* in Panels A and B and years *t-2, t-1, t and t+1* in Panel C. Panel A includes the Mandatory Rotation Disclosure sample and Panels B and C include the expanded sample of rotations. The dependent variables are *Material Restate, Restate, Abnormal Accruals, Audit Fees* in columns 1, 2, 3, and 4, respectively. All specifications include year fixed effects. Models are estimated using OLS with standard errors that are robust to heteroskedasticity and clustered by client (Petersen 2009). T-statistics are presented in parentheses below the coefficients. *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively (based on two-tailed tests). All variables are defined in Appendix A.

Panel A: Multiple Regression Analysis on Mandatory Rotation Disclosure Sample years t-1 and t

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Rotate	*0.050**	*0.071***	*0.003*	*0.003*
	(1.79)	*(2.20)*	*(0.88)*	*(0.08)*
Rotate 95% Confidence	*[-0.006, 0.106]*	*[0.007, 0.134]*	*[-0.004, 0.011]*	*[-0.067, 0.073]*
Size	0.004	-0.008	-0.003*	0.460***
	(0.46)	(-0.50)	(-1.66)	(15.55)
Growth	0.076	0.074	0.041**	-0.201
	(1.27)	(0.74)	(2.48)	(-0.76)
Inventory	0.073	-0.036	0.014	0.716**
	(0.77)	(-0.12)	(0.66)	(2.01)
Foreign Income	0.027	0.110*	0.014***	0.734***
	(0.88)	(1.75)	(2.69)	(8.62)
ROA	0.108	-0.188	-0.113***	-0.489
	(0.72)	(-0.77)	(-2.99)	(-0.98)
Fixed Effects	Year	Year	Year	Year
Observations	174	174	218	264
R-squared	0.222	0.127	0.381	0.824

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Table 4 Continued: Rotation Firms Pre- and Post-Rotation: Multiple Regression Analysis

Panel B: Multiple Regression Analysis on Expanded Comment Letter Sample years t-1 and t

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Rotate	*0.036**	*0.038*	*0.004*	*0.007*
	(1.93)	*(1.09)*	*(1.31)*	*(0.25)*
Rotate 95% Confidence	*[-0.001, 0.072]*	*[-0.031, 0.106]*	*[-0.002, 0.011]*	*[-0.051, 0.066]*
Size	0.005	-0.000	-0.002	0.490***
	(0.92)	(-0.04)	(-1.46)	(18.13)
Growth	0.053	0.035	0.053***	-0.237
	(1.08)	(0.34)	(3.28)	(-1.00)
Inventory	0.063	-0.087	0.028	0.889**
	(0.74)	(-0.38)	(0.92)	(2.35)
Foreign Income	0.017	0.079	0.004	0.712***
	(0.85)	(1.57)	(0.69)	(8.63)
ROA	0.081	-0.464*	-0.066	-0.408
	(0.65)	(-1.68)	(-1.51)	(-0.88)
Fixed Effects	Year	Year	Year	Year
Observations	236	236	272	334
R-squared	0.195	0.088	0.322	0.825

Table 4 Continued: Rotation Firms Pre- and Post-Rotation: Multiple Regression Analysis

Panel C: Multiple Regression Analysis on Expanded Sample years t-2, t-1, t, and t+1

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Rotate	*0.049***	*0.041*	*0.001*	*0.012*
	(2.10)	*(1.01)*	*(0.38)*	*(0.38)*
Rotate 95% Confidence	*[0.003, 0.094]*	*[-0.039, 0.121]*	*[-0.004, 0.007]*	*[-0.052, 0.076]*
Size	0.003	-0.008	-0.002	0.490***
	(0.53)	(-0.72)	(-1.29)	(18.13)
Growth	0.026	-0.125	0.052***	-0.039
	(0.61)	(-1.64)	(3.51)	(-0.24)
Inventory	-0.016	-0.151	0.030	0.709*
	(-0.18)	(-0.77)	(1.16)	(1.94)
Foreign Income	0.026	0.070	0.002	0.705***
	(1.14)	(1.49)	(0.47)	(8.42)
ROA	0.069	-0.302	-0.049	-1.035**
	(0.53)	(-1.19)	(-1.19)	(-2.60)
Fixed Effects	Year	Year	Year	Year
Observations	467	467	519	634
R-squared	0.145	0.066	0.208	0.813

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Table 5: Audit Firm Tenure and Audit Partner Rotations

The sample includes firm-years around mandatory rotation events in years *t-2, t-1, t, and t+1* on the expanded sample of rotations. All specifications include controls and year fixed effects. Models are estimated using OLS with standard errors that are robust to heteroskedasticity and clustered by client (Petersen 2009). T-statistics are presented in parentheses below the coefficients. *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively (based on two-tailed tests). All variables are defined in Appendix A.

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Rotate	*0.050***	*0.058*	*-0.000*	*-0.001*
	(2.15)	*(1.45)*	*(-0.00)*	*(-0.04)*
Rotate 95% Confidence	*[0.004, 0.096]*	*[-0.021, 0.137]*	*[-0.006, 0.006]*	*[-0.071, 0.068]*
Rotate x Firm Tenure at Rotation	*-0.002*	*-0.003*	*0.000*	*-0.000*
	(-1.57)	*(-0.94)*	*(0.17)*	*(-0.16)*
Rotate x Firm Tenure at Rotation 95% Confidence	*[-0.004, 0.000]*	*[-0.008, 0.003]*	*[-0.000, 0.001]*	*[-0.004, 0.004]*
Firm Tenure at Rotation	*0.003*	*0.001*	*-0.001*	*0.001*
	(0.70)	*(0.10)*	*(-0.66)*	*(0.07)*
Controls	Included	Included	Included	Included
Controls Interacted with Firm Tenure at Rotation	Included	Included	Included	Included
Fixed Effects	Year	Year	Year	Year
Observations	471	471	523	637
R-squared	0.172	0.099	0.213	0.818

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Table 6: Multiple Regression Analysis with Alternative Audit Quality Proxies

The sample includes firm-years around mandatory rotation events in years *t-2, t-1, t, and t+1* on the expanded sample of rotations. The dependent variables are *Material Restate Announce, Restate Announce, Positive Special Items, Negative Special Items* in columns 1, 2, 3, and 4 respectively. All specifications include year fixed effects. Models are estimated using OLS with standard errors that are robust to heteroskedasticity and clustered by client (Petersen 2009). T-statistics are presented in parentheses below the coefficients. *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively (based on two-tailed tests). All variables are defined in Appendix A.

	Material Restate Announce	*Restate Announce*	*Positive Special Items*	*Negative Special Items*
Rotate	*0.004*	*-0.006*	*0.022*	*-0.024*
	(0.66)	*(-0.32)*	*(0.67)*	*(-0.65)*
Rotate 95% Confidence	*[-0.007, 0.015]*	*[-0.045, 0.033]*	*[-0.043, 0.086]*	*[-0.096, 0.048]*
Size	-0.002	-0.007	0.006	0.006
	(-0.88)	(-1.55)	(0.54)	(0.40)
Growth	0.002	-0.001	0.211**	-0.226*
	(0.25)	(-0.01)	(2.23)	(-1.82)
Inventory	-0.007	-0.062	-0.057	0.277
	(-0.34)	(-0.93)	(-0.34)	(1.15)
Foreign Income	0.004	0.014	0.022	0.195***
	(0.61)	(0.68)	(0.61)	(3.59)
ROA	0.058	-0.057	0.404**	-1.005***
	(0.92)	(-0.45)	(2.00)	(-3.92)
Fixed Effects	Year	Year	Year	Year
Observations	655	655	519	634
R-squared	0.053	0.024	0.051	0.113

Table 7: Auditor Tenure and Audit Outcomes

This table presents the results from estimations of Equation 1 with *Rotate* replaced by *Tenure* (tenure indicators) in Panel A (B) on a sample with known partner tenure. All specifications include controls and year fixed effects. Models are estimated using OLS with standard errors that are robust to heteroskedasticity and clustered by client (Petersen 2009). T-statistics are presented in parentheses below the coefficients. *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively (based on two-tailed tests). All variables are defined in Appendix A.

Panel A: Auditor Tenure Count Variable and Audit Outcomes

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Tenure	*-0.005*	*-0.005*	*-0.001*	*-0.003*
	(-1.33)	*(-0.60)*	*(-0.88)*	*(-0.51)*
Controls	Included	Included	Included	Included
Fixed Effects	Year	Year	Year	Year
Observations	584	584	695	838
R-squared	0.108	0.026	0.200	0.820

Panel B: Auditor Tenure Indicator Variables and Audit Outcomes

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Tenure = 1	*0.026*	*0.030*	*0.006**	*0.040*
	(1.60)	*(0.94)*	*(1.71)*	*(1.44)*
Tenure = 2	*0.006*	*0.037*	*-0.001*	*-0.006*
	(0.48)	*(0.88)*	*(-0.27)*	*(-0.12)*
Tenure = 3	*-0.007*	*0.009*	*-0.002*	*0.041*
	(-0.41)	*(0.21)*	*(-0.41)*	*(0.97)*
Tenure = 4	*0.004*	*0.049*	*0.002*	*0.044*
	(0.35)	*(1.47)*	*(0.61)*	*(1.64)*
Controls	Included	Included	Included	Included
Fixed Effects	Year	Year	Year	Year
Observations	584	584	695	838
R-squared	0.111	0.029	0.203	0.821

Appendix A: Variable Definitions

Variable	Definition
Rotate	Indicator variable equal to 1 if observation occurs in year *t* or *t+1* following mandatory audit partner rotation
Material Restate	Indicator variable equal to 1 if the client subsequently restates the year's annual financial statements and the restatement is disclosed in an item 4.02 8-K filing, 0 otherwise
Material Restate Announce	Indicator variable equal to 1 if the client announces a restatement in an item 4.02 8-K filing in the time between the prior year's annual report filing date and the current year's annual report filing date, 0 otherwise
Restate	Indicator variable equal to 1 if the client subsequently restates the year's annual financial statements, 0 otherwise
Restate Announce	Indicator variable equal to 1 if the client announces a restatement in the time between the prior year's annual report filing date and the current year's annual report filing date, 0 otherwise
Abnormal Accruals	Absolute value of the residual from the following model following (Kothari, Leone, and Wasley 2005) estimated by industry-year: $TA/A = \alpha + \lambda_0(1/A) + \lambda_1(\Delta REV-\Delta REC)/(A) + \lambda_2 (PPE/A) + \lambda_3(NI/A)$, where A = average assets; TA = total accruals (income before extraordinary items − cash flows from operations); ΔREV = change in revenue; ΔREC = change in receivables; PPE is net property plant and equipment; and NI is income before extraordinary items
Log Audit Fees	$Ln(Audit\ Fees_t)$
Tenure	Count variable between one and five indicating the length of the current lead engagement partner's tenure
Firm Tenure at Rotation	Audit Firm Tenure in years, at the Year of the Mandatory Rotation – demeaned for tests
Size	$Ln(Assets_t)$ where Assets is in millions
Growth	$(Sales_t - Sales_{t-1})/Sales_{t-1}$
Inventory	$Inventory_t/Assets_t$
Foreign Income	Indicator variable equal to 1 if the company has foreign income greater than 0 in year *t*, 0 otherwise
ROA	$Net\ Income_t/Assets_{t-1}$
Positive Special Item	Indicator variable equal to 1 if special items are greater than 0, 0 otherwise
Negative Special Item	Indicator variable equal to 1 if special items are less than 0, 0 otherwise

Appendix B: Analyses with Firm Fixed Effects

Appendix B1: Multiple Regression Analysis with Firm Fixed Effects

The sample includes firm-years around mandatory rotation events: years *t-1* and *t* in Panels A and B and years *t-2, t-1, t and t+1* in Panel C. Panel A includes the primary sample and Panels B and C include the expanded sample. The dependent variables are *Material Restate, Restate, Abnormal Accruals, Audit Fees* in columns 1, 2, 3, and 4, respectively. All specifications include controls, firm fixed effects, and year fixed effects. Models are estimated using OLS with standard errors that are robust to heteroskedasticity and clustered by client (Petersen 2009). T-statistics are presented in parentheses below the coefficients. *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively (based on two-tailed tests). All variables are defined in Appendix A.

Panel A: Multiple Regression Analysis on years t-1 and t

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Rotate	*0.086**	*0.074*	*0.001*	*0.015*
	(1.93)	*(1.61)*	*(0.21)*	*(0.65)*
Rotate 95% Confidence	*[-0.003, 0.175]*	*[-0.018, 0.166]*	*[-0.007, 0.008]*	*[-0.031, 0.061]*
Controls	Included	Included	Included	Included
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	174	174	218	264
R-squared	0.399	0.298	0.269	0.717

Panel B: Multiple Regression Analysis on Expanded Sample years t-1 and t

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Rotate	*0.061**	*0.025*	*0.001*	*-0.014*
	(1.79)	*(0.44)*	*(0.31)*	*(-0.63)*
Rotate 95% Confidence	*[-0.007, 0.129]*	*[-0.086, 0.135]*	*[-0.006, 0.008]*	*[-0.057, 0.029]*
Controls	Included	Included	Included	Included
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	236	236	272	334
R-squared	0.318	0.179	0.245	0.666

Appendix B1 Continued: Multiple Regression Analysis with Firm Fixed Effects

Panel C: Multiple Regression Analysis on Expanded Sample years t-2, t-1, t, and t+1

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Rotate	*0.060*	*0.063*	*0.001*	*-0.018*
	(1.56)	*(1.32)*	*(0.19)*	*(-0.74)*
Rotate 95% Confidence	*[-0.016, 0.137]*	*[-0.032, 0.158]*	*[-0.006, 0.007]*	*[-0.066, 0.030]*
Controls	Included	Included	Included	Included
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	467	467	519	634
R-squared	0.176	0.095	0.185	0.613

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Appendix B2: Audit Firm Tenure and Audit Partner Rotations with Firm Fixed Effects

The sample includes firm-years around mandatory rotation events in years *t-2, t-1, t, and t+1* on the expanded sample of rotations. All specifications include controls, firm fixed effects, and year fixed effects. Models are estimated using OLS with standard errors that are robust to heteroskedasticity and clustered by client (Petersen 2009). T-statistics are presented in parentheses below the coefficients. *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively (based on two-tailed tests). All variables are defined in Appendix A.

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Rotate	*0.018*	*0.051*	*0.001*	*-0.021*
	(1.06)	*(1.33)*	*(0.17)*	*(-1.14)*
Rotate 95% Confidence	*[-0.016, 0.053]*	*[-0.025, 0.128]*	*[-0.006, 0.007]*	*[-0.057, 0.015]*
Rotate x Firm Tenure at Rotation	*-0.002*	*-0.002*	*0.000*	*-0.001*
	(-1.37)	*(-0.81)*	*(0.39)*	*(-0.54)*
Rotate x Firm Tenure at Rotation 95% Confidence	*[-0.004, 0.001]*	*[-0.008, 0.003]*	*[-0.000, 0.001]*	*[-0.003, 0.002]*
Firm Tenure at Rotation	*-0.051*	*0.027*	*0.001*	*-0.027*
	(-1.06)	*(0.42)*	*(0.30)*	*(-1.16)*
Controls	Included	Included	Included	Included
Controls Interacted with Tenure	Included	Included	Included	Included
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	471	471	523	637
R-squared	0.193	0.107	0.190	0.650

Appendix B3: Auditor Tenure and Audit Outcomes with Firm Fixed Effects

This table presents the results from estimations of Equation 1 with *Rotate* replaced by *Tenure* (tenure indicators) in Panel A (B) on a sample with known partner tenure. All specifications include controls, firm fixed effects, and year fixed effects. Models are estimated using OLS with standard errors that are robust to heteroskedasticity and clustered by client (Petersen 2009). T-statistics are presented in parentheses below the coefficients. *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively (based on two-tailed tests). All variables are defined in Appendix A.

Panel A: Auditor Tenure Count Variable and Audit Outcomes

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Tenure	*-0.007*	*-0.006*	*-0.001*	*0.001*
	(-1.51)	*(-0.70)*	*(-0.74)*	*(0.31)*
Controls	Included	Included	Included	Included
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	584	584	695	838
R-squared	0.144	0.054	0.139	0.579

Panel B: Auditor Tenure Indicator Variables and Audit Outcomes

	Material Restate	*Restate*	*Abnormal Accruals*	*Log Audit Fees*
Tenure = 1	*0.030*	*0.031*	*0.004*	*0.000*
	(1.51)	*(0.88)*	*(0.98)*	*(0.00)*
Tenure = 2	*0.014*	*0.046*	*0.003*	*0.001*
	(0.78)	*(1.31)*	*(0.47)*	*(0.03)*
Tenure = 3	*0.003*	*0.022*	*-0.000*	*0.023*
	(0.15)	*(0.56)*	*(-0.03)*	*(0.67)*
Tenure = 4	*0.002*	*0.049*	*0.003*	*0.015*
	(0.17)	*(1.48)*	*(0.75)*	*(0.78)*
Controls	Included	Included	Included	Included
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Observations	584	584	695	838
R-squared	0.145	0.058	0.141	0.580

Appendix C: Examples of SEC filings with information on engagement partner rotation

Panel A: Lead Engagement Partner Rotation - Mandatory

Example I: Electronic Arts Inc.

Link: https://www.sec.gov/Archives/edgar/data/712515/000119312515237104/d939178ddef14a.htm

Relevant text: *Our KPMG LLP lead audit partner has been working on the Company's audit since the first quarter of fiscal 2011, and completed his rotation upon the filing of the Annual Report. The Audit Committee approved a new lead audit partner, who commenced work on the Company's audit in the first quarter of fiscal 2016.*

Example II: International Paper Company

Link: https://www.sec.gov/Archives/edgar/data/51434/000119312514138690/d640335ddef14a.htm

Relevant text: *Further, in conjunction with the mandated rotation of the audit firm's lead engagement partner for the period beginning with the 2014 reporting year, the Audit & Finance Committee and its chairperson were directly involved in the selection of Deloitte & Touche LLP's new lead engagement partner.*

Panel B: Lead Engagement Partner Rotation – Unclear whether rotation is mandatory (excluded)

Example I: Patterson Companies, Inc.

Link: https://www.sec.gov/Archives/edgar/data/891024/000119312514294403/d749476ddef14a.htm

Relevant text: *EY has been our company's independent registered public accounting firm since 1985, and the lead audit partner has served in that capacity since 2013.*

Example II: Alcoa Inc.

Link: https://www.sec.gov/Archives/edgar/data/4281/000119312509054514/ddef14a.htm

Relevant text: *The lead audit partner is rotated at least every five years in accordance with Securities and Exchange Commission requirements. In 2009, a new lead partner was designated. The committee has concluded that the independent auditor is independent from the company and its management.*

Appendix D: Examples of how mandatory partner rotation affects auditor realignment

Panel A: Auditor realignment due to incapacity to comply with mandatory rotation rules

Example I: Helios and Matheson Analytics, Inc

Link:

https://www.sec.gov/Archives/edgar/data/1040792/000143774915008069/hmny20150423_def14a.htm

Relevant text: *The audit of the Company as of and for the year ended December 31, 2014 will mark completion of the fifth consecutive audit for both the lead and concurring review partner, and Mercadien is unable to rotate a suitable lead and concurring review partner for the 2015 periods.*

Example II: Sotherly Hotels Inc.

Link: https://www.sec.gov/Archives/edgar/data/1301236/000119312513456897/d636305d8k.htm

Relevant text: *The Partner Rotation Rules limit the lead audit partner and concurring audit partner's audit services to a term of five years. Both partners will be reaching the end of this five year term, and PBMares does not have the capacity to continue providing audit services to the Company.*

Panel B: Solicitation of bids from competing audit firms due to an impending mandatory partner rotation

Red Robin Gourmet Burgers, Inc

Link: https://www.sec.gov/Archives/edgar/data/1171759/000104746915003210/a2224066zdef14a.htm

Relevant text: *With a rotation of audit engagement partner required in 2015, the audit committee decided to open the annual selection process to several other independent registered public accounting firms.*

Note: The firm subsequently changes its audit firm from Deloitte & Touche LLP to KPMG.

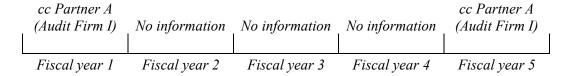**Appendix E: Identification of mandatory rotation using SEC comment letters**

To identify mandatory rotation using SEC comment letters, we go through a firm's entire history of comment letter responses. If the same partner(s) from the same audit firm is copied in five consecutive years, then we assume that in the sixth year, there is a mandatory rotation. We also check that the firm does not change its auditor in the sixth year.

It is possible that a firm only has comment letter responses in year one and year three, but not in year two. If the same partner(s) from the same audit firm is copied in the beginning year and the end year and the time gap is less than four years, then we assume that the lead partner remains unchanged during the gap. For example, if one firm only has comment letter responses and copies the same audit partner(s) from the same audit firm only in year one and year five, then we assume that the lead audit partner remains unchanged through year one to year five. Therefore, year six will be a mandatory rotation year. On the contrary, if the copied audit partner(s) or the audit firm name changes or the gap is greater than three years, then we draw no conclusion about whether there is a mandatory partner rotation.

Example I: The same partner(s) from the same audit firm is copied in five consecutive years.

cc Partner A (Audit Firm I)	cc Partner A (Audit Firm I)	cc Partner A (Audit Firm I)	cc Partner A (Audit Firm I)	cc Partner A (Audit Firm I)
Fiscal year 1	Fiscal year 2	Fiscal year 3	Fiscal year 4	Fiscal year 5

We assume that year six is a mandatory rotation year.

Example II: The same partner(s) from the same audit firm is copied, and the time gap is less than four years.

cc Partner A (Audit Firm I)	No information	No information	No information	cc Partner A (Audit Firm I)
Fiscal year 1	Fiscal year 2	Fiscal year 3	Fiscal year 4	Fiscal year 5

We assume that from fiscal year one to fiscal year five, the lead partner remains the same, and year six is a mandatory rotation year.

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Example III: The same partner(s) from the same audit firm is copied, and the time gap is less than four

years.

cc Partner A (Audit Firm I)	No information	cc Partner A (Audit Firm I)	No information	cc Partner A (Audit Firm I)
Fiscal year 1	Fiscal year 2	Fiscal year 3	Fiscal year 4	Fiscal year 5

We assume that from fiscal year one to fiscal year five, the lead partner remains the same, and year six is

a mandatory rotation year.

E*xample IV: Partner(s) or firm name changes.*

cc Partner A (Audit Firm I)	No information	cc Partner B (Audit Firm I)	No information	cc Partner B (Audit Firm I)
Fiscal year 1	Fiscal year 2	Fiscal year 3	Fiscal year 4	Fiscal year 5

Since there is an audit partner change in year three, this case is not included in our sample.

Example V: The same partner(s) from the same audit firm is copied, but the time gap is greater than three

years.

cc Partner A (Audit Firm I)	No information	No information	No information	No information	cc Partner A (Audit Firm I)
Fiscal year 1	Fiscal year 2	Fiscal year 3	Fiscal year 4	Fiscal year 5	Fiscal year 6

Since the time gap is greater than three years, we do not assume a rotation, and this case is not included in

our sample.