Do Peer Revenue Restatements Still Generate Contagion?

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Abstract

Kedia, Koh, and Rajgopal (2015), hereafter KKR, provide evidence that cohort firms learn from peer firms' restatements and begin manipulating revenue following a peer revenue-related restatement, particularly when the restatement is less severe. The increasing trend of less severe restatements since KKR's study along with technological advances that have made it easier for cohort firms, investors, and regulators to monitor peer firms' financial reporting warrant a fresh look at reporting contagion. We begin our analysis by replicating KKR's findings. Although we find evidence of contagion during their sample period, we find no such evidence after 2008. We then explore whether cohort firm responses to peer firm revenue restatements in more recent years vary based on the prior reporting aggressiveness of the cohort firm. Rather than finding evidence of reporting contagion, we find that aggressively reporting cohort firms are more likely to report conservatively in response to peer restatements. Finally, we explore whether cohort firm responses vary based on the degree to which peer restatements are likely to be scrutinized. Again, we find no evidence of contagion. Instead, we find that high scrutiny peer restatements lead to caution for cohort firms that previously were reporting aggressively. Overall, our results suggest that regulatory changes and technological advancements during the past 15 years have substantially altered how cohort firms respond to peer firm restatements.

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1 Introduction

Managers use financial reports to communicate firm performance and reduce information asymmetry. Although a manager's target audience primarily involves providers of capital, a substantial body of research (e.g., Roychowdhury et al., 2019; Bernard et al., 2020) suggests that financial data are also informative to other firms in the same industry cohort. Kedia, Koh, and Rajgopal (2015), hereafter KKR, find that industry cohort firms are more likely to initiate an earnings manipulation strategy after a peer firm restatement is announced, particularly when the restatements are of a less egregious nature. KKR suggest that the peer restatement provides information on "both the costs of earnings management and specific accounts used" (p. 2342), essentially providing a road map for earnings manipulation. Our purpose in this paper is to determine whether KKR's findings persist in more recent time periods – i.e., do restatements *still* generate contagion? – and to provide new insights about the dynamics of financial reporting among peer and cohort firms.

Our study is important for several reasons. To begin, restatements are one of the primary observable indicators of poor reporting quality and have been shown to induce stock price declines among both restating firms and non-restating industry peers (Gleason et al. 2008). By exploring whether cohort firms have continued to report more aggressively as they "learn" from the reporting misadventures of their peers or whether their behavior has changed over time, our study benefits investors and speaks to the Securities and Exchange Commission's (SEC's) recent initiative to enhance the use of market and industry data to help prevent, detect, and enforce against improper reporting.[1] Further, there have been substantial changes in the nature of restatements since 2008

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[1] See https://www.sec.gov/files/fy-2025-congressional-budget-justification.pdf.

(the end of KKR's sample period) as well as substantial technological advancements. Specifically, while the number of restatements has decreased, an increasing proportion of restatements are *less* egregious.[2] Given KKR's assertion that these restatements are the most likely source of reporting contagion, it is possible that contagion has increased over time. However, technological advances and greater disclosure of financial information in machine readable formats (e.g., XBRL) may make it easier for cohort firms to monitor and learn from peer firm financial disclosures (Bernard et al., 2020; Cho and Muslu, 2020), potentially reducing the "information" that is inferred from a restatement. Because these same factors likely enhance the ability of regulators and other monitors to detect cohort firms' aggressive reporting following peer restatements, the costs of initiating a manipulation strategy in response to a peer restatement likely have increased. Thus, it is also possible that contagion has decreased over time. Overall, understanding the current state of aggressive reporting across peer and cohort firms is important for financial statement users as well as stakeholders with financial reporting oversight responsibilities.

In our tests, we focus on the reporting of revenue. Top-line revenue has always been an important performance indicator, and its importance has increased dramatically in the last decade as the number and importance of knowledge firms has increased (e.g., Hopkins et al., 2023; Srivastava, 2014). Knowledge firms tend to report losses, especially in the early stages of their life cycles, thereby reducing the usefulness of earnings and elevating the importance of revenue in evaluating firm performance. Revenue issues also dominate reporting improprieties and auditing challenges in recent years, topping the list of restatement categories (Audit Analytics) and Public Company Accounting Oversight Board (PCAOB) inspection deficiencies in 2020 (PCAOB, 2022), and ranking second in SEC comment letter frequency (EY, 2020). Focusing specifically on the

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[2] The decline in the number of restatements excludes those associated with Special Purpose Acquisition Companies (SPACs). See https://go.auditanalytics.com/l/908172/2022-05-19/6tpwv for specifics.

aggressive reporting of revenue provides substantial empirical advantages as well, allowing us to use a model (i.e., Stubben, 2010) that is better specified, exhibits less bias, and has more power for detecting earnings management leading to manipulation than commonly used discretionary accrual models.

To perform our analyses, we collect a sample of firm-years from 2003 through 2020 and identify revenue restatements announced within industry groupings. We use these restatement announcements to define peer revenue restatement firm-years as well as industry-matched non-restating cohort firm-years. Our tests focus on non-restating cohort firm-years and explore how cohort firm financial reporting evolves leading up to and following a peer restatement. Given the importance of a reliable identification strategy, our tests of cohort firm revenue reporting behavior include comparisons to both (1) other firms with no peer restatements in the same year and (2) cohort firms' own reporting behavior in non-peer-restatement years. By using revenue-based restatements as the public signal of a peer's financial reporting improprieties and employing a within-cohort-firm design to account for innate, time-invariant firm characteristics, we reduce the concern that the changes we document stem from factors other than the peer revenue restatement. We also include features to control for mean reversion in accruals and concurrent industry trends.

We begin our analysis by replicating the results from KKR using their post-SOX sample period (i.e., 2003 – 2008). Consistent with KKR, we find that peer firm restatements of revenue are associated with the initiation of a cohort firm revenue manipulation strategy through 2008. In other words, we find evidence of contagion in revenue manipulation in this period. After 2008, however, we do not find evidence that peer firm revenue restatements motivate cohort firms to begin a revenue manipulation strategy.

Motivated by KKR's assertion that peer restatements *teach* cohort firms how to manipulate revenue, we explore whether cohort firms' responses to peer restatements in the post-KKR period differ based on the revenue management practices undertaken by the cohort firm *prior* to the peer restatement. Specifically, we differentiate between cohort firm-years with low versus high levels of abnormal revenue prior to a peer restatement. If peer restatements cause cohort firms to *initiate* an earnings manipulation strategy (consistent with KKR), then the greatest change in behavior should be observed among firms that have the "most to learn" from a peer restatement – i.e., cohort firms with low prior levels of abnormal revenue. Empirically, however, we find no evidence of contagion regardless of whether prior levels of abnormal revenue were high or low.

It is also possible that cohort firms respond to peer restatements but that their responses are more measured – becoming more aggressive, but not rising to the level of misstatements. To explore this possibility, we next investigate whether peer restatements lead to increases in abnormal revenue and, if so, whether this effect is stronger among cohort firms with the most to learn from peers' restatements. Again, we find no evidence of contagion. Instead, we find that cohort firms with high prior levels of abnormal revenue actually *reduce* their aggressive revenue reporting in the year of a peer restatement. Taken together, our findings suggest that in contrast to the contagion documented by KKR, the cohort firm response in more recent time periods (1) occurs when revenue is reported more aggressively prior to the peer restatement and (2) results in a short-term decrease as opposed to an increase in aggressive reporting behavior.

We also conduct tests related to the potential scrutiny that peer restatements are likely to receive. The inference from KKR is that cohort firms will increase their aggressive reporting in response to peer restatements receiving less scrutiny because the expected cost of misreporting would be lower. During our sample period, peer restatements subject to less scrutiny are associated

with no change in cohort firm reporting behavior. However, when peer restatements involve fraud or SEC investigation or occur across multiple consecutive years, we find that cohort firms that were reporting aggressively *reduce* their aggressive reporting. In other words, the "slowdown effect" that we observe on average is concentrated in instances where peer firm misreporting would be more obvious and scrutiny would be greater. This finding provides additional evidence that the reporting behavior of cohort firms vis-à-vis peer restatements has changed since 2008.

Finally, we conduct several robustness tests. Our results are robust to a falsification test wherein the timing of the peer revenue restatement is altered and to an alternative one-stage specification employed to reduce concerns about potential bias associated with the two-stage discretionary revenue model (Chen et al., 2018). We also conduct several tests to alleviate the concern that the observed results are an artifact of accrual reversals.

Our study makes several important contributions. To begin, we build from KKR and other research examining cohort firms' financial reporting responses to peer restatements (e.g., Beatty et al., 2013; Gleason et al., 2008; Li, 2016). Our results suggest that in the current environment, contagion no longer describes the average cohort firm's response to peer restatements of revenue. Instead, our findings suggest that most of the "learning" that is occurring on the part of cohort firms results in a *reduction* in reporting aggressiveness. A number of factors could be responsible for this post-KKR change in behavior, including (1) the advent of XBRL and other technological advances which have greatly simplified the monitoring of peer firm financial reports and SEC filings, and (2) increasing civil penalties for managers under the Dodd-Frank Act and the Sarbanes-Oxley Act (SOX). The combination of these factors may have resulted in a shift in the cost-benefit tradeoff such that cohort firms, which are now much better equipped to analyze peer firm reporting behavior, are less inclined to imitate the reporting behavior of peers, particularly when such

behavior is likely to draw scrutiny.

Our results also contribute to the profession's understanding of current revenue reporting practices. Revenue is important in evaluating and predicting firm performance (e.g., Srivastava, 2014), and its importance has increased as the proportion of intangible-intensive firms and firms that consistently report losses continues to increase (Bushman et al., 2016; Hopkins et al., 2023). Perhaps because revenue is so important in evaluating firm performance, standard setters and regulators consistently express concern over the quality of reported revenue (McKenna, 2019).[3] Given the importance of revenue in measuring and predicting firm performance (e.g., Schipper et al., 2009) and because revenue can be used to mislead stakeholders about firm performance (e.g., Altamuro et al., 2005; Campbell et al., 2022; Stubben, 2010), it is important to understand if peer revenue restatements still motivate revenue manipulation contagion, or, if not, how cohort firms respond to peer restatements. Our analyses add insight to this important discussion.

2 Prior literature and predictions

2.1 Peer Restatements and Cohort Financial Reporting Responses

Prior research provides evidence that firms and stakeholders learn from peer restatement disclosures (e.g., Gleason et al., 2008; Gao and Zhang, 2019; Roychowdhury et al., 2019; KKR). In the study most comparable to ours, KKR find that cohort firms are more likely to begin managing earnings after a peer firm restatement. They suggest that a restatement announcement provides a road map that peer firms can use to guide their earnings manipulation strategies. KKR reason that "[i]f the [peer] restating firm, upon discovery of misrepresentation, faces little or no regulatory enforcement, then a [cohort] firm is likely to conclude that the costs of managing earnings are low." They further suggest that restatement announcements enable cohort firms to

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[3] See, for example, https://assets.ey.com/content/dam/ey-sites/ey-com/en_us/topics/assurance/accountinglink/ey-sru17009-221us-09-08-2022.pdf?download.

learn about "the details of the misconduct" (KKR, p. 2338) thereby providing cohort firms with information on how to implement a low-cost manipulation strategy.

Whether the findings of KKR persist in more recent years, however, is an open question. On one hand, there has been a significant decline in egregious restatements. Figure 1 provides trends in restatements by type and shows that roughly 80% of restatements in recent years are revision (or little r) restatements, compared to 48% at the end of KKR's sample period.[4] Little r restatements occur when prior periods are assessed as materially correct on a standalone basis and do not require the reissuance of financial statements. Instead, prior period misstatements are corrected and revised in current periodic filings. Because KKR suggest that less egregious restatements are the source of reporting contagion, contagion may have increased in more recent years.

On the other hand, KKR's most consistent findings are during years in which sources of peer firm information were markedly different from those of today. The advent of XBRL and other technological advances including large language models and artificial intelligence have greatly simplified the monitoring of peer firm financial reports and disclosures. Bernard et al. (2020) provide evidence that cohort firms actively search for information in peer firms' SEC fillings and adjust their investment activities in response to this information. Similarly, Cho and Muslu (2020) find that disclosure tone in peer firms' MD&A disclosures – which can be monitored much more easily now than in the early post-SOX period – influences cohort firms' investment decisions. Cohort firms can also use social networks to learn about peer firms' financial decisions (e.g.,

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[4] The designation of Big R (or non-reliance) restatements did not exist until August 2004 when the SEC adopted a final rule to expand the number of events that are reportable on Form 8-K under the Securities Exchange Act of 1934. One of the additions was Item 4.02 *Non-Reliance on Previously Issued Financial Statements or a Related Audit Report or Completed Interim Review*. This item requires an 8-K filing associated with the reissuance of previously issued financial statements that should no longer be relied upon because of an error in such financial statements.

Fracassi, 2016), and the advent of artificial intelligence and its ability to process large volumes of data with speed and precision has enhanced accounting task efficiency and accuracy (Tandiono, 2023) and enabled real-time financial analysis (Smith, 2018). Given these technological advances, it is possible that peer restatements no longer provide cohort firms with important insights about peer firm financial reporting strategies or the costs of misreporting. It is also important to note that these same advances enable regulators and other stakeholders to monitor companies more effectively, increasing the likelihood that misreporting will be discovered (and penalized) and potentially shifting the cohort firm cost-benefit calculus (Becker 1968) away from aggressive reporting.[5] All of these factors suggest that restatements may no longer be a catalyst for cohort learning and contagion in misreporting. These observations lead to our first research question:

Research Question 1 (RQ1): Are cohort firms still more likely to begin manipulating revenue following a peer revenue restatement?

KKR assert that peer restatements teach cohort firms how to implement a manipulation strategy. If so, cohort firms that previously *had not been reporting aggressively* – i.e., those that have the most to learn – should begin reporting *more aggressively* following a peer firm restatement. It is also possible, however, that cohort firms that previously *had been reporting aggressively* will report *more conservatively* following a peer restatement to reduce the likelihood that their own reporting comes under scrutiny. In other words, cohort firm responses to peer restatements announcements may be conditioned by prior reporting aggressiveness. We consider these possibilities with our second research question:

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[5] In recent years, standard setters also have focused a great deal of attention on simplifying revenue recognition standards in part by moving towards more principles-based measurement (ASU 2009-13/14 and ASU 2014-09). Although prior research conducted on revenue recognition standard changes prior to SOX (e.g., Altamuro et al., 2005; Zhang, 2005; Srivistava, 2014) suggests that managers use the discretion afforded in principle-bases regimes opportunistically (e.g., pre-SOP 91-1, pre-SOP 97-2, pre-SAB 101), recent evidence of changes to more principles-based revenue recognition standards in the post-SOX era fails to find such evidence except in nuanced settings (e.g., Myers et al., 2022). In fact, some evidence points toward improved revenue-related disclosure quality under ASC 606 (Hinson et al., 2022). The differences may disincentivize manipulation in response to peer restatements.

Research Question 2 (RQ2): Do cohort firms' reporting decisions following a peer restatement vary based on prior revenue reporting aggressiveness?

KKR find that there is cross-sectional variation in cohort firm responses to peer restatements based on the expected cost of detection (Becker 1968). For example, they find that contagion does not exist for restatements that are more likely to be heavily scrutinized, such as those subject to SEC investigation or class-action lawsuits. Given the evolving regulatory landscape since KKR's study, with increased focus on governance mechanisms such as clawback provisions and a rise in civil and criminal penalties for misreporting, cohort firms that were previously reporting aggressively may be more sensitive to downside risk.[6] As a result, these firms may report *more conservatively* following a peer restatement that is likely to draw more scrutiny to reduce the likelihood that their own reporting comes under question. In contrast, cohort firms that previously had not been reporting aggressively may only be more likely to begin reporting *more aggressively* following a peer firm restatement if the potential level of scrutiny is sufficiently low. Finally, it is also possible that technological advances have improved the ability of investors and regulators to monitor and analyze peer and cohort firm reporting decisions in recent years such that the scrutiny *that is attributable specifically to a peer restatement* may no longer be important, on-average. Based on these possibilities, our final research question is as follows:

Research Question 3 (RQ3): Do cohort firms' reporting decisions following a peer restatement vary based on the degree to which peer restatements are likely to be scrutinized?

3 Sample, descriptive statistics, and models

3.1 Sample

To construct our sample, we obtain all available data for companies in Compustat and Audit

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[6] See for example the Report to Congress: Increased Penalties Under the Sarbanes-Oxley Act of 2002 (https://www.ussc.gov/research/congressional-reports/2003-report-congress-increased-penalties-under-sarbanes-oxley-act-2002) and provisions of the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010.

Analytics with fiscal years between 2003 (the initiation of SOX) and 2020, inclusive. We first identify firm-years when a peer firm announces a revenue restatement. We consider all such restatements because, regardless of the direction of the income change caused by the restatement, revisions to income generally draw regulatory and investor scrutiny because they signal poor internal controls, prior-period "cookie-jar" accounting, and/or a culture of low-quality financial reporting.[7] Because our tests focus on non-restating firms, we remove all firm-years with a revenue restatement announcement from the sample used for our analyses. Following KKR, we remove all except the first misstated firm-year revealed through a revenue restatement, resulting in a sample of 66,767 firm-years that we use to replicate the KKR analysis. For our primary tests, which utilize a measure of abnormal revenue reporting during the post-KKR period, we follow prior research (Stubben, 2010) and exclude 17,307 firm-years in regulated industries (i.e., utilities, insurance, and financial institutions) because the factors causing their revenue accruals likely differ from those of firms in other industries. We further remove 12,935 firm-years with insufficient data to estimate abnormal revenue and 15,298 firm-years prior to 2009 (in order to focus on a post-KKR sample). Application of these criteria results in a sample of 21,227 firm-years available for our analysis. For tests that incorporate year $t+1$ data, the sample size is reduced to 18,152 firm-years. Table 1 presents the sample selection details.

Although firm-years with a restatement announcement are not included in our sample, they are used to identify industry-years with a peer firm restatement. Treatment firm-years are non-restating firm-years within the 2-digit SIC industry with a peer firm revenue restatement announced during the year.[8] Control firm-years include firm-years where industry peers do not

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[7] As discussed in our robustness tests, we find similar results when we limit restatements to those that decrease net income.
[8] Specifically, the restatement must be announced after the filing of the company's prior year annual report and prior to the fiscal year-end date.

announce any revenue restatements during the year. Because we explore whether cohort firms'

responses to a peer restatement vary based on the extent of aggressive revenue reporting prior to

the peer restatement, we separately identify firms reporting revenue aggressively (and not

aggressively) in the annual period prior to the peer revenue restatement (year *t-1*). Figure 2

illustrates the timing of variable measurement.

Table 2, Panel A provides descriptive statistics by year for firms that announce revenue

restatements. Revenue restatements are identified from the Audit Analytics' Non-Reliance

Restatement database, which includes restatements announced through the filing of an 8-K with

the SEC (Big R restatements) as well as those disclosed in periodic annual filings (little r

restatements). The sample includes 790 revenue restatements, ranging between 24 and 85 per year.

As shown in Panel B, the average number of firms within an industry cohort is 55 and there are

1,247 industry-years in our sample. Just over 30 percent of industry years include a revenue

restatement.

Table 2, Panel C provides descriptive statistics for the variables that are included in our

models. We winsorize extreme values of the distribution of each continuous variable to the 1st and

99th percentiles to minimize the influence of outliers. *AbnRev* has a mean near zero, which is

expected since it is a residual from an OLS model. The number of firms that begin misstating

revenue following an industry peer revenue restatement (*Begin_Man_Rev$_t$*) is only 0.5 percent of

the sample. The average percentage of peer restatements within an industry defined by 2-digit SIC

(*Public_IND%$_{t-1}$*) is 2.5 percent, and 80 percent of firm-years are characterized by at least one peer

restatement of revenue in the prior year (*IND Rev Restate*).[9] Thus, only a very small fraction of

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[9] Although 31.5 percent of industry-year groupings have at least one revenue restatement, 80 percent of the sample firm-years are characterized by peer restatement because revenue restatements often occur in the larger industry groupings.

cohort firms exposed to an industry peer revenue restatement *begin* to misstate revenue following

a peer restatement (i.e., only 317 of the approximately 53,600 firm-years exposed to a prior year

peer revenue restatement).[10] An important takeaway from these descriptive statistics is that the

initiation of a revenue manipulation strategy by a cohort firm that is subsequently revealed through

a restatement following a peer revenue restatement is an infrequent occurrence.

3.2 Abnormal revenue

To measure revenue management, we use a model developed in Stubben (2010). This approach

produces a measure of firms' abnormal revenues and reflects significant deviations from GAAP

as well as revenue management that is more subtle or less material.[11] Prior research finds that

Stubben's model produces discretionary reporting estimates that are better specified, exhibit less

bias, and have more power than commonly used discretionary accrual models (Stubben, 2010;

Christensen et al., 2022).

Following prior research (Stubben, 2010; Krishnan and Yu, 2012; Brown et al., 2019), we

estimate discretionary revenue using the following model in the cross-section by industry (two-

digit SIC) and year:

$$\Delta AR_{it} = \alpha_0 + \alpha_1(\Delta Sales_{it} \times Size_{it}) + \alpha_2(\Delta Sales_{it} \times LnAge_{it}) + \alpha_3(\Delta Sales_{it} \times Age^2_{it}) + \alpha_4(\Delta Sales_{it} \times GRR\_P_{it}) + \alpha_5(\Delta Sales_{it} \times GRR\_N_{it}) + \alpha_6(\Delta Sales_{it} \times GRM_{it}) + \alpha_7(\Delta Sales_{it} \times GRM^2_{it}) + \varepsilon_{it} \quad (1),$$

where all variables are defined in Appendix A.[12] Revenue and accrual variables (ΔAR and $\Delta Sales$)

are scaled by average total assets. *GRR_P* and *GRR_N* are the industry-median adjusted revenue

growth rate when growth is positive and negative, respectively. *GRM* is the industry-median

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[10] Approximately 80 percent of the 66,767 firm-years (roughly 53,600) reside within an industry with at least one peer restatement of revenue in the prior year.

[11] More subtle earnings management could include the manipulation of real activities (e.g., sales discounts, relaxed credit requirements, channel stuffing, and bill and hold sales), which would also be reflected in the discretionary revenue estimate (Stubben, 2010, 699).

[12] We require a minimum of 10 observations in each industry-year regression.

adjusted gross margin and *Age* is the number of years the firm has been a public entity captured

by the length of time Compustat reports total assets for the firm (Cassell et al., 2016). The residual

from these industry-year regressions (*AbnRev*) represents the estimate of the abnormal portion of

a firm's revenues. To identify firms that report revenues aggressively prior to the announcement

of a peer revenue restatement, we create an indicator variable, *HighAbnRev*, set equal to one if

AbnRev is in the upper tercile of the sample in the year prior to the peer revenue restatement, and

to zero otherwise. We also create an indicator variable, *LowAbnRev*, set equal to one if *AbnRev* is

in the lower tercile of the sample in the year prior to the peer revenue restatement, and to zero

otherwise.

3.3 Test of RQ 1

To test RQ1, we replicate model (1) from KKR. Specifically, we estimate the following:

$$Begin\_Mis\_Rev_{i,t} = \beta_1 \textbf{\textit{Peer\_Restate}}_{i,t-1} + \beta_2 CONTEMP\_IND\%_{i,t-1} + \beta_3 BMR_{i,t} + \beta_4 Leverage_{i,t} + \beta_5 LnAssets_{i,t} + \beta_6 ROA_{i,t} + \beta_7 STDEarn_{i,t} + \beta_8 Return_{i,t} + \beta_9 Herfindex_{i,t} + \beta_{10} IndBMR_{i,t} + \varepsilon_{i,t} \qquad (2).$$

The dependent variable, *Begin_Mis_Rev*, is an indicator variable equal to one for the first year a

firm misstates revenue (subsequently revealed through a restatement), and zero otherwise. We

measure *Peer_Restate* using either *PUBLIC_IND%* or *IND Rev Restate*. *PUBLIC_IND%* is the

percentage of industry peers that announced a revenue restatement in the previous year (*t-1*). *IND*

Rev Restate is an indicator set to one when at least one industry peer announced a revenue

restatement in the previous year (*t-1*). While KKR use the continuous variable, the use of an

indicator variable facilitates interpretation, particularly in subsequent models that include

interactions between *IND Rev Restate* and different cross-sectional variables. Further, the low

frequency of revenue misstatements relative to the large number of firms within several of the 2-

digit SIC industry classifications provides little meaningful variation in the continuous measure.

If the inferences from KKR persist, the coefficient β_1 will be positive. *CONTEMP_IND%* captures the percentage of the industry that has begun misstating revenue before year t but has not yet publicly announced a restatement. Other control variables follow KKR and are defined in the appendix. In our replication, we partition the sample based on time periods similar to those examined in KKR (2003 – 2005 and 2006 – 2008). After replicating KKR, we focus on the period of 2009 – 2020 for RQ2 and RQ3.

3.4 Test of RQ2

To test RQ2, we initially modify equation (2) as follows:

$$Begin\_Mis\_Rev_{i,t} = \beta_1 Peer\_Restate_{i,,t-1} + \beta_2 LowAbnRev_{t-2} + \boldsymbol{\beta_3 Peer\_Restate_{I,t-1}} \times \boldsymbol{LowAbnRev_{t-2}} + \beta_4 HighAbnRev_{t-2} + \boldsymbol{\beta_5 Peer\_Restate_{i,i,t-1}} \times \boldsymbol{HighAbnRev_{t-2}} + \beta_6 CONTEMP\_IND\%_{i,t-1} + \beta_7 BMR_{i,t} + \beta_8 Leverage_{i,t} + \beta_9 LnAssets_{i,t} + \beta_{10} ROA_{i,t} + \beta_{11} STDEarn_{i,t} + \beta_{12} Return_{i,t} + \beta_{13} Herfindex_{i,t} + \beta_{14} IndBMR_{i,t} + \varepsilon_{i,t} \qquad (3)$$

Equation (3) includes *LowAbnRev* and *HighAbnRev*, capturing either low or high levels of discretionary revenue in the year prior to the peer revenue restatement, and their interactions with *Peer_Restate*. If peer restatements cause cohort firms with the most to learn from a peer restatement to initiate a revenue manipulation strategy, then β_3 should be positive. With respect to aggressively reporting cohort firms, a positive value for β_5 would indicate an increased likelihood of future cohort firm restatements among firms that were already reporting revenue aggressively prior to the peer restatement, which would be more consistent with the persistence of (rather than contagion in) aggressive reporting behavior. In contrast, a negative value for β_5 would suggest that peer restatements cause aggressively reporting cohort firms to begin reporting more conservatively, presumably to minimize the likelihood that their own financial reports will come under scrutiny.

It is also possible that cohort firms respond to peer restatements with aggressive reporting but that their responses do not rise to the level of a material misstatement. To explore this

possibility, we replace our original dependent variable ($Begin\_Mis\_Rev_{i,t}$) with $AbnRev_{t-1}$ and examine whether peer restatements are associated with higher levels of contemporaneous abnormal revenue. As in equation (3), we use interactions to determine whether any effect is *amplified* among cohort firms with the most to learn from a peer restatement or *reduced* among cohort firms already reporting high levels of abnormal revenue who might be subject to increased scrutiny. To explore changes in abnormal revenue reporting following a peer restatement, we use the following model:

$$AbnRev_{t-1} = \beta_1 Peer\_Restate_{i,,t-1} + \beta_2 LowAbnRev_{t-2} + \boldsymbol{\beta_3 Peer\_Restate_{I,t-1} \times LowAbnRev_{t-2}} + \beta_4 HighAbnRev_{t-2} + \boldsymbol{\beta_5 Peer\_Restate_{i,i,t-1} \times HighAbnRev_{t-2}} + \beta_6 CONTEMP\_IND\%_{i,t-1} + \beta_7 BMR_{i,t} + \beta_8 Leverage_{i,t} + \beta_9 LnAssets_{i,t} + \beta_{10} ROA_{i,t} + \beta_{11} STDEarn_{i,t} + \beta_{12} Return_{i,t} + \beta_{13} Herfindex_{i,t} + \beta_{14} IndBMR_{i,t} + \sum_{i=1 to n} \xi_i Firm_i + \varepsilon_{i,t} \quad (4).$$

The coefficients of interest in equation (4) are β_3 and β_5, and the predictions and inferences for these two coefficients are as previously discussed. Our control variables in these models follow KKR, and we also incorporate firm fixed effects to account for the innate, time-invariant firm characteristics that influence abnormal revenues. It is also important to note that by including lagged *LowAbnRev* and *HighAbnRev* as control variables, we accommodate reversals in revenue accruals that would occur absent intentional manipulation.

3.5 Tests of RQ3

For RQ3, we use two adjusted specifications of equation (4) to explore whether the scrutiny associated with a peer restatement influences cohort firm reporting. First, we replace *Peer_Restate* with two mutually exclusive indicator variables, *MultipleYearsIndRestatements* and *SingleYearIndRestatement*, capturing whether there are consecutive years of peer restatements through year *t-1* (high scrutiny) or whether a peer restatement only occurs in year *t-1* and not in the immediately preceding year (low scrutiny). For the second test, we replace *Peer_Restate* with two different mutually exclusive indicator variables, *FraudIndRestatements* and

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OtherIndRestatement, identifying situations where there is at least one peer restatement involving fraud or occurring as the result of an SEC investigation in year *t-1* (high scrutiny) and situations where these conditions do not hold (low scrutiny). If, as found by KKR, low peer restatement scrutiny still increases the likelihood that cohort firms will report aggressively, the baseline coefficients for *SingleYearIndRestatements* and *OtherIndRestatements* should be positive. If cohort firms are only more likely to begin reporting *more aggressively* following a peer firm restatement if they were previously reporting revenues conservatively and peer restatement scrutiny is sufficiently low, then the interaction of *SingleYearIndRestatements* and *OtherIndRestatements* with *LowAbnRev* should be positive. If cohort firms previously reporting revenue aggressively are more likely to report *more conservatively* following a peer firm restatement that is likely to draw more scrutiny, then the interaction of *MultipleYearsIndRestatements* and *FraudIndRestatements* with *HighAbnRev* should be negative.

4 Empirical results

4.1 RQ1

Table 3 presents the results of our replication of KKR using revenue restatements as well as our tests of RQ1. Columns 1 through 3 provide the results measuring peer restatements with the continuous restatement variable *PUBLIC_Ind%* and columns 4-6 provide the results measuring peer restatements with the indicator variable, *IND_Rev Restate*. Consistent with KKR, in columns 1 and 2, we find a positive and significant association between an increasing proportion of industry peer restatements in year *t-1* and the likelihood of initiating revenue misreporting in year *t* that is later revealed through a restatement. KKR's sample ends in 2008; thus, our pre-2009 results replicate their findings. We also examine the influence of peer restatements using an indicator for peer restatements (*IND Rev Restate)* rather than the continuous variable. Consistent with the

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conclusions from KKR, we observe a significant and positive coefficient on *Ind Rev Restate* in column 4, which examines 2003-2005. However, the use of the indicator variable provides an insignificant association in the years 2006-2008.

Table 3 also reveals that during the 2009-2020 time period (columns 3 and 6), the coefficients on both the continuous measure and the indicator variable capturing peer revenue restatements are insignificant. As such, we do not find evidence that peer firm revenue restatements motivate cohort firms to begin a revenue manipulation strategy in more recent years. In other words, our findings do not support the notion that peer restatements still generate contagion, on average. Based on KKR's inference that peer restatements provide cohort firms a roadmap to learn how to implement a manipulation strategy, we next explore whether contagion may exist among cohort firms with the most to learn from the peer restatement announcement. In this and all subsequent analyses, we focus on the 2009 – 2020 time period.

4.2 RQ2

Table 4 presents the results of our tests of RQ2. Given evidence in Table 3 that the conclusions are the same in 2009-2020 using the continuous measure of peer restatements (*PUBLIC_IND%*) and the indicator variable for peer restatements (*Ind Rev Restate*), we use the indicator variable for the remainder of our tests to simplify the interpretation of interactions. Further, the indicator form of the variable provides greater power to observe differences across groups. Table 4 does not provide any evidence to suggest that peer restatements generate contagion for cohort firms with the most to learn (or any other subset of cohort firms). That is, the coefficients for *IND Rev Restate* and the two interactions are not significantly different from zero. These results provide additional evidence that peer restatements no longer spur cohort firms to manipulate revenue.

Although Table 4 provides no evidence of contagion in *restatements*, it is possible that cohort firms' aggressiveness has become more measured in recent years. To explore this avenue of contagion, we use equation (4) to examine abnormal revenue, a proxy for a less egregious form of earnings management than revenue manipulation leading to a restatement. We report the results of this analysis in Table 5. If contagion is most likely among cohort firms that have the most to learn, we expect to observe a positive coefficient on the interaction of *IND Rev Restate ×LowAbnRev*. In column 1 of Table 5, the coefficient on the interaction of *IND Rev Restate ×LowAbnRev* does not differ significantly from zero. Further, we observe a *negative* coefficient on the interaction of *IND Rev Restate × HighAbnRev*. This result suggests that firms reporting revenues more aggressively prior to the peer revenue restatement report more conservatively in the year that the peer restatement is revealed. In other words, peer restatements appear to motivate *caution* rather than *contagion* among aggressively reporting cohort firms.

If cohort firms do adjust their revenue reporting following a peer restatement of revenue, how long does the change last? To answer this question, we use model (4) and modify the timing of *AbnRev* to capture abnormal revenue in year *t* and separately, year *t+1*. Similarly, we re-specify the timing of the control variables to match the timing of the measurement of the dependent variable. If the cohort firms' responses are short-lived, we would expect the coefficients β_3 and β_5 to be insignificant or reverse relative to the baseline in equation (4). If the response persists over time, then we would expect similar signs and significance on β_3 and β_5 in these specifications. We present the results of this analysis in columns 2 and 3 of Table 5. The coefficient on the interaction of *IND Rev Restate × HighAbnRev* does not differ significantly from zero in either of the two years following the peer restatement, indicating that the "slowdown effect" among aggressively-reporting cohort firms is very short-lived.

4.3 RQ 3

To this point, our tests suggest that in the post-KKR period, there is no contagion in cohort firm reporting. In our final set of primary tests, we explore whether contagion may exist based on the potential scrutiny associated with peer firm restatements. As in our tests of **RQ2**, we use a modified version of equation (4) to accommodate finer gradations of aggressive reporting. We begin by partitioning our original *IND Rev Restate* variable into variables identifying industry groups experiencing multiple years of peer restatements (*MultipleYearsIndRestatements*) versus only a single, isolated year of peer restatement (*SingleYearIndRestatement*). Panel A of Table 6 shows that – counter to the finding of KKR in earlier years – there is no evidence of increasingly aggressive reporting (i.e., contagion) when scrutiny is likely to be low, regardless of how aggressively the cohort firm was reporting revenue. Specifically, the coefficients on both of the *SingleYearIndRestatement* interactions are not statistically significant. Instead, we find (column 1) that high scrutiny causes cohort firms to report more conservatively when the cohort firms themselves were reporting aggressively prior to the peer restatements (i.e., the coefficient for *MultipleYearsIndRestatements$_{t-1}$* $\times$ *HighAbnRev$_{t-2}$* is negative and significant). This finding suggests that the baseline "slowdown effect" we find for aggressive cohort firms in Table 5 occurs when they are concerned about the increased scrutiny that likely would be associated with widespread misreporting in the industry. Consistent with Table 5, this increased caution in reporting only persists for one year (i.e., the year of the peer restatement).

In Panel B of Table 6, we define scrutiny based on whether (*FraudIndRestatements*) or not (*OtherIndRestatement*) at least one peer restatement is a result of fraud or relates to an SEC investigation, both of which would be likely to draw greater attention from regulators and other monitors. Again, we find no evidence to support KKR's assertion that peer restatements that face

less scrutiny motivate contagion among cohort firms (regardless of how aggressively cohort firms reported revenue prior to the peer revenue restatement). Specifically, the coefficients on the interaction terms involving *OtherIndRestatement* are insignificantly different from zero. In contrast, the coefficient for *FraudIndRestatements$_{t-1}$ × HighAbnRev$_{t-2}$* is negative and significant, indicating that when peers report restatements that are likely to generate more scrutiny, aggressively reporting cohort firms report more conservatively. These findings are consistent with the results presented in Panel A and provide additional evidence in support of the baseline finding in Table 5. As with previous tests, we continue to find that the "slowdown effect" does not continue beyond the year of the peer restatement.

5 Additional analyses and robustness tests

5.1 Falsification test

To further calibrate our conclusions, we perform a falsification test wherein we examine if the reduction in aggressive revenue reporting that we observe occurs before the public announcement of the peer revenue restatement. Specifically, we identify industries with an announcement of a revenue restatement in year *t+1*, but no restatement announcement in year *t* or *t-1* (recall that year *t-1* is the year we capture peer restatements and cohort firms' response through abnormal revenue). In other words, we flag future restatement announcements and then replace *IND Rev Restate$_{t-1}$* with *Future IND Rev Restate$_{t+1}$* and the related interaction with *LowAbnRev$_{t-2}$* and *HighAbnRev$_{t-2}$*. As shown in column 2 of Table 7, we find an insignificant coefficient on *Future IND Rev Restate$_{t+1}$* as well as on the interactions *Future IND Rev Restate$_{t+1}$ × HighAbnRev$_{t-2}$* and *Future IND Rev Restate$_{t+1}$ × LowAbnRev$_{t-2}$*. This falsification test provides corroboratory support that the reduction in aggressive revenue reporting that we document in Tables 5 and 6 reflects a response to the public announcement of the peer revenue restatement(s).

5.2 Potential bias stemming from two-stage regressions

Chen et al. (2018) present evidence suggesting that two-step procedures used to examine determinants of a first-stage residual, such as discretionary revenue, can generate biased coefficients and standard errors that lead to incorrect inferences. According to Chen et al. (2018, 782), "the most basic solution is to simply estimate the coefficients for all the model regressors in a single-, as opposed to two-step, regression." To determine the robustness of our initial results to this particular concern, we implement the proposed correction by estimating equation (4) with the inclusion of industry-year fixed effects, interactions of the industry-year fixed effects with each of the first-stage regressors and all of the second-stage regressors in equation (4). Column 3 of Table 7 shows that the findings with respect to our variables of interest are qualitatively unchanged. The consistency of the results using the alternative specification suggests that the coefficients and standard errors generated from the two-stage procedure are not unduly affecting our inferences.

5.3 Limiting peer restatements to those that reduce net income

Our main tests include all restatements of revenue because we expect both income-decreasing and income-increasing revenue restatements to garner attention and possibly motivate cohorts to adjust their reporting practices. As a sensitivity test, we restrict our analysis to peer restatements that decrease revenue. Column 4 of Table 7 shows that our findings with this specification are similar to the findings presented in Table 5.

5.4 Peers within MSA rather than within industry

Our primary analyses focus on same-industry peers. Alternatively, firms operating in the same geographical area could also influence cohort firms' reporting practices (KKR). As such, we replicate the results reported in Table 5 after defining peers based on metropolitan statistical area (MSA) representation rather than industry representation. We report the results from this analysis

in column 5 of Table 7. The coefficient for the interaction term (*Rev Restate$_{t-1}$ × HighAbnRev$_{t-2}$*) in this model is not statistically significant, indicating that restatements within the same geographic area are not associated with a response from cohort firms.

5.5 Post-ASC 606 analyses

Although we cannot document the specific mechanism(s) associated with the changes in cohorts' responses to peer restatements relative to their responses in earlier years, it is possible that the updated revenue recognition standard (ASC 606) plays a role. To test this possibility, we estimate our model using only post-ASC 606 years (i.e., beginning in 2018) and report our findings in column 6 of Table 7. Although the coefficient for *IND Rev Restate$_{t-1}$ × HighAbnRev$_{t-2}$* is negative, the t-statistic indicates that the coefficient does not differ significantly from zero.

5.6 Accrual reversals

Although our models explicitly control for revenue accrual reversals (*LowAbnRev* and *HighAbnRev*), we perform additional tests to address the concern that the results we observe might be an artifact of mean reversion in accruals. First, using univariate tests, we compare average lagged abnormal revenue in industries with a revenue restatement to that of industries without a revenue restatement. If average abnormal revenue is higher in restatement industries in the year preceding a revenue restatement, then the negative association between current period abnormal revenue and the interaction of *IND Rev Restate$_{t-1}$ × HighAbnRev$_{t-2}$* that we observe in Table 5 may potentially reflect additional reversal of the revenue accruals. In untabulated analyses, however, we find that average lagged abnormal revenue is not significantly different between industries with a revenue restatement and industries without a revenue restatement.[13] Further, if the slowdown

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[13] Average lagged abnormal revenue is -0.0011 for industries with a peer revenue restatement and -0.0007 for industries without a peer revenue restatement. A two-sample t-test fails to reject the null that these groups' means are statistically different ($t = 0.722$).

effect we observe merely reflects accrual reversal, then we would expect the coefficient on the interaction *IND Rev Restate$_{t-1}$ × LowAbnRev$_{t-2}$* in equation (4) to be positive and significant. However, Table 5 shows that this interaction is not significant. Collectively, these results help alleviate concerns that the observed results are an artifact of accrual reversals.

5.7 Reflection concern

A common concern when evaluating cohort firm responses to peer restatements is that the same factors that cause peer firms to restate can influence cohort firms' decisions independent of the peer firm restatement (Manski 1993). This concern cannot be fully alleviated. Some aspects of our study, however, reduce the concern. First, while we identify peer treatment years using revenue restatements, our main analyses focus on abnormal revenue reported by the cohort firm. Although the factors influencing revenue reporting may correlate with factors influencing firms' propensity to misstate revenue, the factors are not fully overlapping. Second, we find that our results only manifest in the group with higher amounts of prior period abnormal revenue and that the reduction in abnormal revenue following a peer restatement exceeds the cohort firm's reduction in abnormal revenue following periods with similarly high levels of abnormal reporting, but without a peer restatement. These ideas notwithstanding, the reflection concern remains and our conclusions should be interpreted in light of this concern.

6 Conclusion

Prior research (KKR) concludes that peer restatements motivate cohort firms to begin an earnings manipulation strategy. Given that the restatements touted by KKR as being the most responsible for contagion (i.e., less egregious restatements) have increased over time, it would be reasonable to expect that contagion will be more prevalent in recent years. However, technological advances have made it easier for cohort firms to monitor peer firms' financial reporting and for auditors,

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investors, and regulators to monitor and detect aggressive reporting. As such, the cost-benefit calculus that cohort firm managers use to determine their response to peer restatements may have changed in the 15 years following KKR's sample period. Our purpose in this paper is to determine whether peer restatements still generate earnings manipulation contagion and, if not, to provide insights on current cohort firm reporting behavior.

We begin by replicating the results from KKR. Consistent with KKR, we find that peer firm restatements of revenue are associated with the initiation of a cohort firm revenue manipulation strategy through 2008. However, we do not find evidence that peer firm revenue restatements motivate cohort firms, on average, to begin a revenue manipulation strategy in subsequent years. We next explore whether cohort firms' responses to peer restatements in the post-KKR period differ based on the revenue management practices undertaken by the cohort firm *prior* to the peer restatement. If peer restatements cause cohort firms to *initiate* an earnings manipulation strategy, then the greatest change in behavior should be observed among firms exhibiting the least aggressive revenue reporting prior to the peer restatement(s). However, we find no evidence to suggest that cohorts with the most to learn, or any subset of cohorts, initiate revenue manipulation following peer restatements.

Although our initial tests suggest that contagion no longer appears to exist, it could be the case that cohort firm responses have become more measured (i.e., subtle or less material) in recent years. To explore this possibility, we examine changes in abnormal revenue, a less aggressive form of revenue management than manipulation leading to a material revenue misstatement. Using this alternative metric, we continue to find no evidence of contagion. Instead, we find that cohort firms reporting revenues more aggressively prior to the peer restatement *decrease* their reporting aggressiveness in the year the industry peer restatement is revealed. We also find that this

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"slowdown effect" is concentrated in peer restatements that are likely to receive more scrutiny from regulators and other monitors.

Overall, the results of our study provide important insights regarding cohort firm responses to peer restatements. Our results suggest that in more recent years, contagion no longer describes the average cohort firm's response to peer restatements of revenue. Instead, our findings suggest that any "learning" that is occurring results in a *reduction* in reporting aggressiveness among cohort firms that were reporting aggressively prior to the peer restatement. Our findings should be of particular interest to investors and regulators given the prominence of revenue reporting issues in restatements and SEC comment letters, the necessity of efficient regulatory monitoring, and the importance of high-quality financial reporting to capital market participants.

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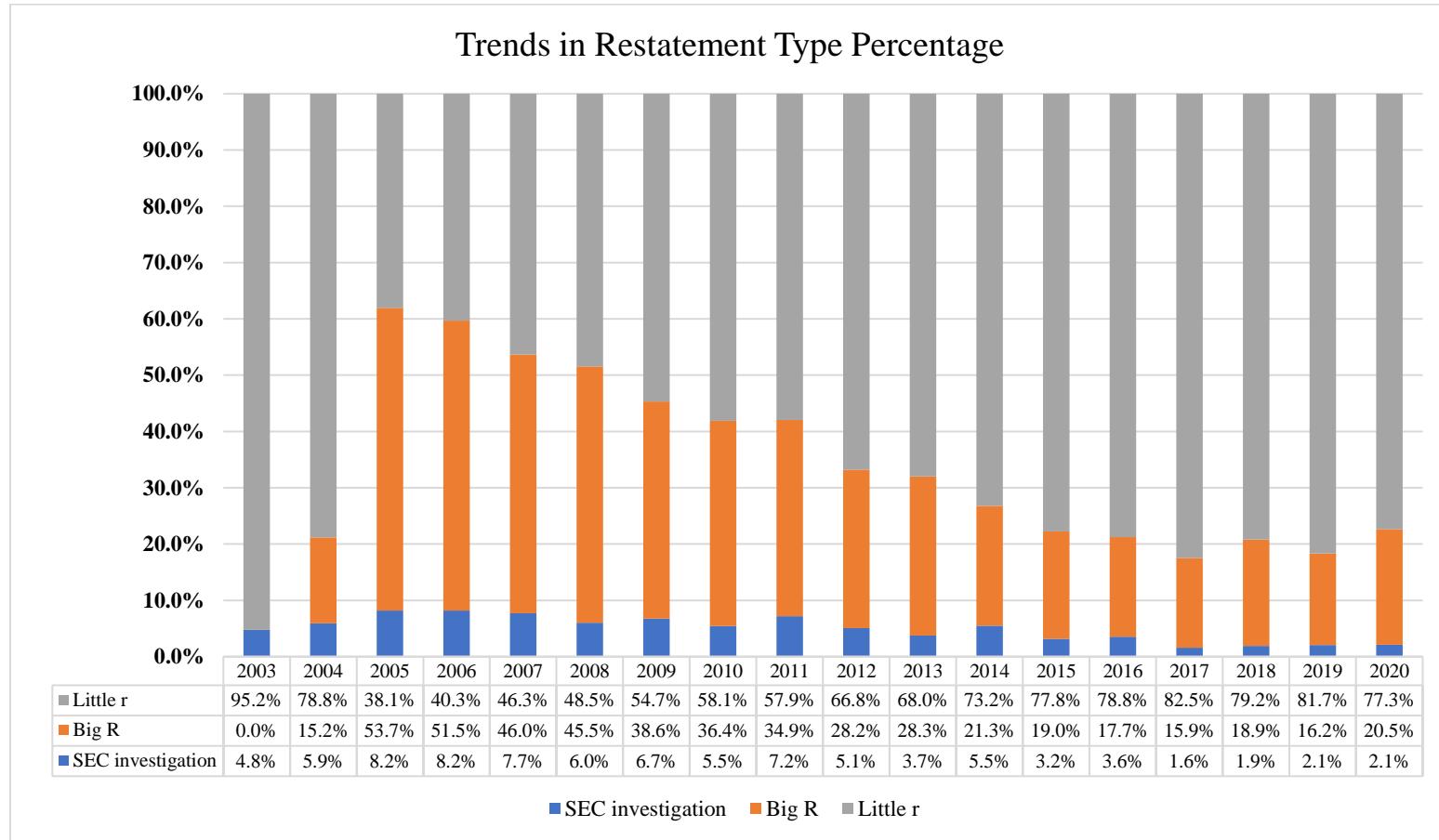
Appendix: Variable definitions

Variable	Definition
Revenue Reporting Measures	
Begin_Man_Rev	An indicator variable set equal to one if a firm begins misstating revenue during year t as evidenced by a subsequent restatement related to revenue, and zero otherwise;
AbnRev	Abnormal revenue estimated as the residual from equation (1) following Stubben (2010);
HighAbnRev	An indicator variable set equal to one if *AbnRev* is in the upper tercile of a given sample year, and zero otherwise;
LowAbnRev	An indicator variable set equal to one if *AbnRev* is in the bottom tercile of a given sample year, and zero otherwise;
Industry Peer Restatement Characteristics	
Public_IND%	The percentage of the industry that has announced a restatement related to revenue during the year. Industry is defined based on two-digit SIC codes
Contemp_IND%	The percentage of the industry that has started misstating revenue, but has not announced a restatement within a year prior to the firm's beginning its violation period; Industry is defined based on two-digit SIC codes
IND Rev Restate	An indicator variable set equal to one if there is at least one revenue restatement announced during the year within the cohort's two-digit SIC industry; zero otherwise;
MultipleYearsIndRestatments	An indicator variable set equal to one if there is at least one revenue restatement announced during the year and the previous year within the cohort's two-digit SIC industry; zero otherwise;
SingleYearIndRestatment	An indicator variable set equal to one if there is at least one revenue restatement announced during the year, but none announced in the previous year within the cohort's two-digit SIC industry; zero otherwise;
FraudIndRestatments	An indicator variable set equal to one if at least one revenue restatement announced during the year within the cohort's two-digit SIC industry that is related to fraud or an SEC investigation; zero otherwise;
OtherIndRestatment	An indicator variable set equal to one if none of the revenue restatements announced during the year within the cohort's two-digit SIC industry that is related to fraud or an SEC investigation; zero otherwise;
Primary Control Variables	
Lnassets	The natural logarithm of total assets;
Leverage	The ratio of long-term debt to total assets;
BMR	The book-to-market ratio (i.e., the book value of equity divided by the market value of equity);
ROA	Return on assets, defined as earnings before extraordinary items divided by beginning of the year total assets;

EARNVOL	Earnings volatility, computed as the standard deviation of earnings before extraordinary items for the 12 quarters ending with the year of observation;
RET	The contemporaneous 12-month buy-and-hold returns;
HERFINDEX	the Herfindahl index for the firm's industry, computed as the sum of the squares of the market shares (firm sales/industry sales) of the firms in the industry. Industry is defined based on two-digit SIC codes;
IND_BMR	The industry-level book-to-market ratio, computed by dividing the sum of all book values in the industry by the sum of market values in the industry. Industry is defined based on two-digit SIC codes;

Variables Used to Estimate Abnormal Revenue

ΔAR	The change in net accounts receivable from the prior year scaled by average total assets;
$\Delta Sales$	The change in sales revenue from the prior year scaled by average total assets;
LnAge	The natural log of the firm's age in years;
Age²	The square of firm age;
GRR_P	The industry-median-adjusted growth rate in revenues if positive, where the growth rate in revenues equals current-year revenue divided by prior-year revenue;
GRR_N	The industry-median-adjusted growth rate in revenues if negative, where the growth rate in revenues equals current-year revenue divided by prior-year revenue;
GRM	The industry-median-adjusted gross margin, where gross margin equals revenue less the cost of revenues, divided by revenues;
GRM²	The square of the industry-median-adjusted gross margin.

Fig 1 Trends in Restatement Types

Trends in Restatement Type Percentage

	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020
■ Little r	95.2%	78.8%	38.1%	40.3%	46.3%	48.5%	54.7%	58.1%	57.9%	66.8%	68.0%	73.2%	77.8%	78.8%	82.5%	79.2%	81.7%	77.3%
■ Big R	0.0%	15.2%	53.7%	51.5%	46.0%	45.5%	38.6%	36.4%	34.9%	28.2%	28.3%	21.3%	19.0%	17.7%	15.9%	18.9%	16.2%	20.5%
■ SEC investigation	4.8%	5.9%	8.2%	8.2%	7.7%	6.0%	6.7%	5.5%	7.2%	5.1%	3.7%	5.5%	3.2%	3.6%	1.6%	1.9%	2.1%	2.1%

■ SEC investigation   ■ Big R   ■ Little r

Source of data is Audit Analytics. SEC investigations indicate SEC involvement in the restatement process. The involvement can take the form of either a SEC comment letter that triggered the restatement or formal or informal SEC inquiry into the circumstances surrounding the restatement. Big R restatements require reissuance of the financial statements and are identified by the filing of an 8-K with the SEC under Item 4.02. Little r restatements occur when prior periods are assessed as materially correct on a standalone basis and do not require the reissuance of financial statements. Rather, prior period misstatements are corrected and revised in current periodic filings.

Fig 2 Timeline Clarifying the Measurement of Exposure to Peer Revenue Restatements and Cohort Firms' Responses
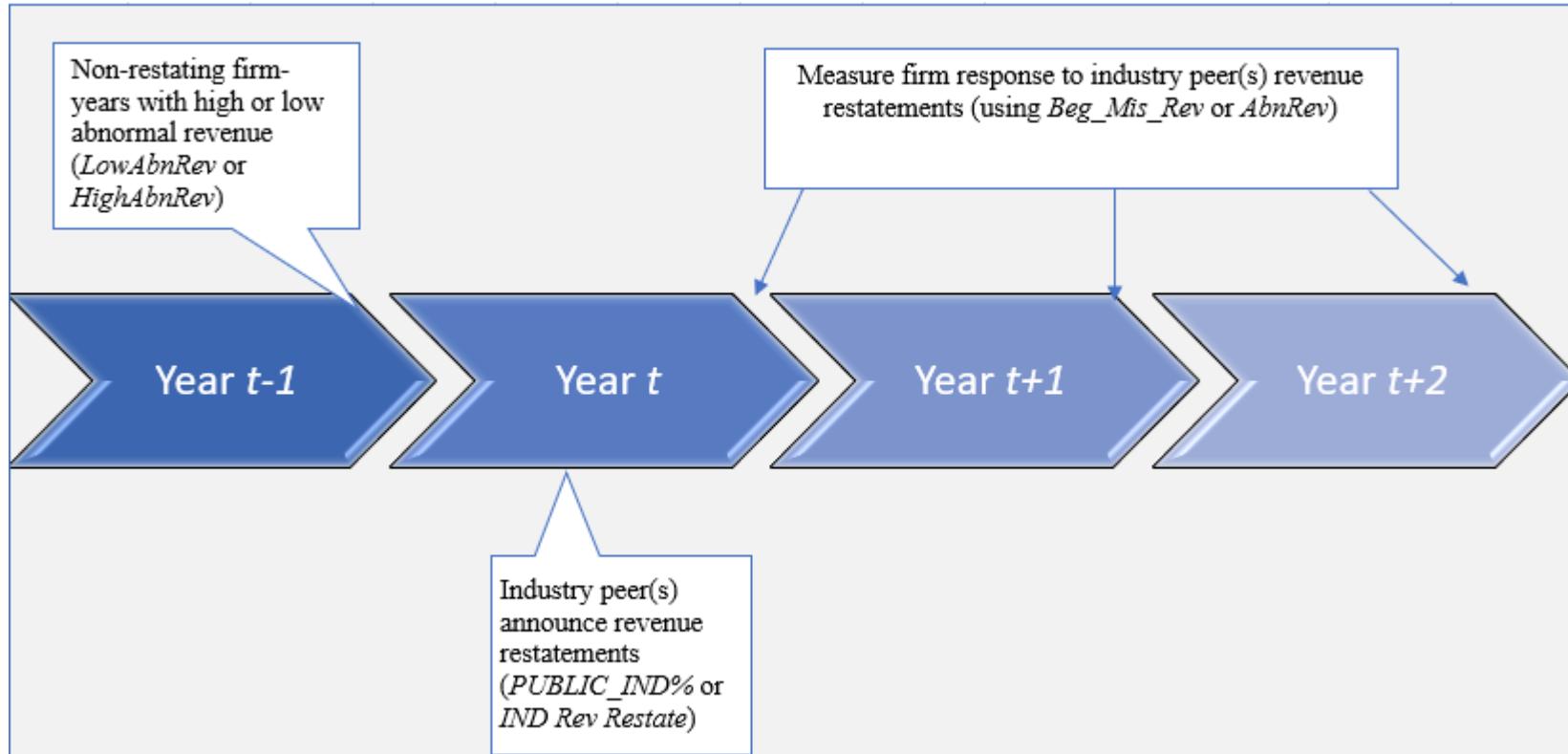
Table 1 Sample Selection

	N
Firm-years between 2003 and 2020, inclusive, with available data in Compustat and Audit Analytics	68,190
Less:	
Firm-years that announce a revenue restatement over the years 2003-2020	(790)
All misstated firm-years subsequent to the first misstated year of firms that eventually announce a restatement of revenue over the years 2003-2020	(633)
Sample for replication	**66,767**
Less:	
Firm-years in regulated industries (2-digit SIC 49, 60-69)	(17,307)
Firm-years with insufficient data to estimate *AbnRev*	(12,935)
Firm-years prior to 2009	(15,298)
Sample for tests considering year *t-1* and year *t*	**21,227**
Less:	
Firm-years with insufficient data in year *t+1*	(3,075)
Sample for tests considering year *t+1*	**18,152**

Table 2 Descriptive Statistics

Panel A: Number of Firms Announcing Revenue Restatements by Sample Year
(Sample Size = 66,767)

Calendar Year	Number of Peer Firms Announcing a Revenue Restatement		Number of Cohort Firms Beginning Revenue Manipulation Following a Peer Restatement		
	N	Percentage of Sample	N	Percentage of Sample	Percentage of Revenue Restatements
2003	66	0.10%	35	0.05%	53.03%
2004	85	0.13%	27	0.04%	31.76%
2005	79	0.12%	11	0.02%	13.92%
2006	63	0.09%	12	0.02%	19.05%
2007	42	0.06%	20	0.03%	47.62%
2008	37	0.06%	12	0.02%	32.43%
2009	26	0.04%	22	0.03%	84.62%
2010	24	0.04%	22	0.03%	91.67%
2011	25	0.04%	23	0.03%	92.00%
2012	29	0.04%	22	0.03%	75.86%
2013	47	0.07%	17	0.03%	36.17%
2014	42	0.06%	18	0.03%	42.86%
2015	32	0.05%	27	0.04%	84.38%
2016	38	0.06%	11	0.02%	28.95%
2017	37	0.06%	11	0.02%	29.73%
2018	42	0.06%	13	0.02%	30.95%
2019	48	0.07%	7	0.01%	14.58%
2020	28	0.04%	7	0.01%	25.00%
Sum	**790**	1.18%	**317**	0.47%	40.13%

Panel B: Statistics Regarding 2-digit SIC Industry Classifications

2-digit SIC	
Average number of industry peers	54.7
Total number of industry-years in sample	1,247
Total number of industry-years with a revenue restatement	393
Percentage of industry-years with a revenue restatement	31.5%

Panel C: Descriptive Statistics for Model Variables

Variable	N	Mean	STD	P25	P50	P75
COHORT'S REPORTING RESPONSES						
$Begin\_Man\_Rev_t$	66,767	0.005	0.069	0.000	0.000	0.000
$AbnRev_{t-1}$	21,227	−0.001	0.033	−0.013	−0.001	0.010
$AbnRev_t$	21,227	−0.001	0.032	−0.013	−0.001	0.010
$AbnRev_{t+1}$	18,152	−0.001	0.031	−0.012	−0.002	0.010
$HighAbnRev_{t-2}$	21,227	0.334	0.472	0.000	0.000	1.000
$LowAbnRev_{t-2}$	21,277	0.318	0.465	0.000	0.000	1.000
PEER RESTATEMENTS						
$Public\_IND\%_{t-1}$	66,767	0.025	0.029	0.004	0.019	0.036
$IND\ Rev\ Restate_{t-1}$	66,767	0.804	0.397	1.000	1.000	1.000
$Contemp\_IND\%_{t-1}$	66,767	0.016	0.019	0.000	0.010	0.023
CONTROL VARIABLES						
BMR_t	66,767	0.603	0.965	0.269	0.504	0.833
$Leverage_t$	66,767	0.237	0.270	0.036	0.177	0.360
$Lnassets_t$	66,767	6.780	2.242	5.226	6.809	8.275
ROA_t	66,767	−0.039	0.402	−0.016	0.021	0.063
$EARNVOL_t$	66,767	0.062	0.163	0.002	0.009	0.036
RET_t	66,767	0.115	0.165	0.073	0.130	0.207
$HERFINDEX_t$	66,767	0.068	0.066	0.033	0.041	0.075
IND_BMR_t	66,767	0.445	0.221	0.271	0.385	0.542

Notes: Variable definitions are provided in the Appendix.

Table 3 Do Cohort Firms Initiate a Manipulation Strategy in Response to Peer Restatements?

$Begin\_Mis\_Rev_{i,t} = \beta_1(\textbf{PUBLIC\_IND\%}_{i,t-1} \text{ or } \textbf{IND Rev Restate}_{i,t-1}) + \beta_2 CONTEMP\_IND\%_{i,t-1} + \beta_3 BMR_{i,t} + \beta_4 Leverage_{i,t} + \beta_5 LnAssets_{i,t} + \beta_6 ROA_{i,t}$
$+ \beta_7 STDEarn_{i,t} + \beta_8 Return_{i,t} + \beta_9 Herfindex_{i,t} + \beta_{10} IndBMR_{i,t} + \varepsilon_{i,t}$

	Dependent Variable = $Begin\_Mis\_Rev_{i,t}$					
	2003-2005	2006-2008	2009-2020	2003-2005	2006-2008	2009-2020
	(1)	(2)	(3)	(4)	(5)	(6)
PUBLIC_IND%$_{t-1}$	**4.473***	**3.016****	**3.709**			
	(1.93)	**(2.12)**	**(1.54)**			
IND Rev Restate$_{t-1}$				**1.158***	**0.360**	**0.313**
				(1.92)	**(0.63)**	**(1.55)**
CONTEMP_IND%$_{t-1}$	−4.170	6.964***	−6.647	−4.353	7.020***	−6.601
	(−0.95)	(2.81)	(−1.46)	(−0.98)	(2.77)	(−1.43)
BMR$_t$	−0.004	0.200	−0.103	−0.005	0.199	−0.102
	(−0.04)	(1.16)	(−1.56)	(−0.05)	(1.16)	(−1.55)
Leverage$_t$	0.562**	−0.056	−0.428	0.557**	−0.032	−0.423
	(2.36)	(−0.11)	(−1.51)	(2.32)	(−0.06)	(−1.49)
LnAssets$_t$	−0.066	−0.040	0.008	−0.061	−0.041	0.008
	(−1.05)	(−0.45)	(0.18)	(−1.00)	(−0.46)	(0.19)
ROA$_t$	0.738*	0.107	0.569**	0.770*	0.116	0.589**
	(1.77)	(0.20)	(2.09)	(1.82)	(0.22)	(2.15)
EARNVOL$_t$	−1.627	−0.179	−0.025	−1.569	−0.157	−0.020
	(−1.23)	(−0.13)	(−0.05)	(−1.22)	(−0.12)	(−0.04)
RET$_t$	0.568	0.692	−0.534	0.477	0.660	−0.519
	(0.57)	(0.97)	(−1.12)	(0.49)	(0.94)	(−1.08)
Herfindex$_t$	−2.556	2.427	0.670	0.637	3.341	1.592
	(−0.98)	(1.04)	(0.70)	(0.29)	(1.23)	(1.52)
IND_BMR$_t$	−2.160**	−1.321	−0.943***	0.146	−1.519	−0.900**
	(−2.10)	(−1.20)	(−2.70)	(0.18)	(−1.36)	(−2.51)
N	11,550	11,589	43,628	11,550	11,589	43,628
Pseudo R^2	0.014	0.019	0.008	0.017	0.018	0.009

Notes: We estimate cluster-robust standard errors. *T*-statistics are presented in parentheses below coefficient estimates. *, **, *** reflect statistical significance at the 10, 5, and 1 percent levels, respectively. Statistical significance is based on 2-tailed p-values. All variables are defined in the Appendix.

Table 4 Does Prior Reporting Aggressiveness Influence Cohort Firms' Propensity to Initiate a Manipulation Strategy in Response to Peer Restatements?

$Begin\_Mis\_Rev_{i,t} = \beta_1 IND\ Rev\ Restate_{i,t-1} + \beta_2 LowAbnRev_{t-2} + \boldsymbol{\beta_3 IND\ Rev\ Restate_{i,t-1} \times LowAbnRev_{t-2}} +$
$\quad \beta_4 HighAbnRev_{t-2} + \boldsymbol{\beta_5 IND\ Rev\ Restate_{i,t-1} \times HighAbnRev_{t-2}} + \beta_6 CONTEMP\_IND\%_{i,t-1} +$
$\quad \beta_7 BMR_{i,t} + \beta_8 Leverage_{i,t} + \beta_9 LnAssets_{i,t} + \beta_{10} ROA_{i,t} + \beta_{11} STDEarn_{i,t} + \beta_{12} Return_{i,t} + \beta_{13} Herfindex_{i,t}$
$\quad + \beta_{14} IndBMR_{i,t} + \varepsilon_{i,t}$

	Dependent Variable = $Begin\_Man\_Rev_t$
	Sample Year 2009-2020
	(2)
IND Rev Restate $_{t-1}$	0.054
	(0.15)
LowAbnRev$_{t-2}$	–0.642
	(–1.11)
IND Rev Restate $_{t-1}$ × LowAbnRev$_{t-2}$	**0.614**
	(0.97)
HighAbnRev$_{t-2}$	–0.140
	(–0.31)
IND Rev Restate $_{t-1}$ × HighAbnRev$_{t-2}$	**0.606**
	(1.21)
CONTEMP_IND%$_{t-1}$	–12.356**
	(–1.97)
BMR$_t$	–0.008
	(–0.08)
Leverage$_t$	–0.329
	(–0.91)
LnAssets$_t$	0.078
	(1.46)
ROA$_t$	0.248
	(0.74)
EARNVOL$_t$	–0.919
	(–1.20)
RET$_t$	–0.421
	(–0.79)
Herfindex$_t$	1.094
	(0.89)
IND_BMR$_t$	–0.480
	(–0.95)
N	21,227
Pseudo R^2	0.012
Test:	
IND Rev Restate $_{t-1}$ × LowAbnRev$_{t-2}$ =	Chi^2 = 0.00
IND Rev Restate $_{t-1}$ × HighAbnRev$_{t-2}$	Prob > Chi^2 = 0.991

Notes: This table presents the results of estimating Equations (3). We cluster standard errors by firm. Z-statistics are presented in parentheses below coefficient estimates. *, **, *** reflect statistical significance at the 10, 5, and 1 percent levels, respectively. Statistical significance is based on 2-tailed p-values. All variables are defined in the Appendix.

Table 5 Does Prior Reporting Aggressiveness Influence Cohort Firms' Revenue Management (*AbnRev*) in Response to Peer Restatements?

$AbnRev = \beta_1 IND\ Rev\ Restate_{i,t-1} + \beta_2 LowAbnRev_{t-2} + \boldsymbol{\beta_3 IND\ Rev\ Restate_{i,t-1} \times LowAbnRev_{t-2}} + \beta_4 HighAbnRev_{t-2} + \boldsymbol{\beta_5 IND\ Rev\ Restate_{i,t-1} \times}$
$\qquad \boldsymbol{HighAbnRev_{t-2}} + \beta_6 CONTEMP\_IND\%_{i,t-1} + \beta_7 BMR_{i,t} + \beta_8 Leverage_{i,t} + \beta_9 LnAssets_{i,t} + \beta_{10} ROA_{i,t} + \beta_{11} STDEarn_{i,t} + \beta_{12} Return_{i,t} +$
$\qquad \beta_{13} Herfindex_{i,t} + \beta_{14} IndBMR_{i,t} + \varepsilon_{i,t}$

	Dependent Variable =		
	$AbnRev_{t-1}$	$AbnRev_t$	$AbnRev_{t+1}$
	(1)	(2)	(3)
IND Rev Restate $_{t-1}$	0.001	0.001	0.000
	(0.75)	(1.02)	(0.18)
LowAbnRev$_{t-2}$	0.005***	0.004***	−0.000
	(3.69)	(2.81)	(−0.07)
IND Rev Restate $_{t-1}$ × LowAbnRev$_{t-2}$	**0.002**	**−0.002**	**0.001**
	(1.20)	**(−1.32)**	**(0.44)**
HighAbnRev$_{t-2}$	−0.005***	−0.002	−0.001
	(−4.28)	(−1.26)	(−1.12)
IND Rev Restate $_{t-1}$ × HighAbnRev$_{t-2}$	**−0.003****	**−0.002**	**0.001**
	(−2.18)	**(−1.24)**	**(0.54)**
CONTEMP_IND%$_{t-1}$	−0.011	−0.015	−0.009
	(−0.76)	(−1.05)	(−0.61)
BMR	−0.002***	−0.001**	−0.001**
	(−4.08)	(−2.35)	(−2.31)
Leverage	−0.007***	−0.006***	−0.005***
	(−4.02)	(−3.77)	(−2.75)
LnAssets	−0.000	0.000	−0.000
	(−0.12)	(0.35)	(−0.18)
ROA	0.005***	0.006***	0.009***
	(4.05)	(4.95)	(6.19)
EARNVOL	−0.003	−0.001	−0.003
	(−0.79)	(−0.46)	(−0.95)
RET	0.001	0.002	0.000
	(1.12)	(1.02)	(0.24)
Herfindex	0.003	0.010	0.004
	(0.36)	(1.09)	(0.43)
IND_BMR	0.002	−0.001	0.002
	(0.79)	(−0.28)	(0.53)

N	21,227	21,227	18,152
Adjusted R^2	0.071	0.066	0.058
Fixed Effects	Firm	Firm	Firm
Test:			
IND Rev Restate $_{t-1}$ × LowAbnRev$_{t-2}$ =	F = 10.41	F = 0.02	F = 0.00
IND Rev Restate $_{t-1}$ × HighAbnRev$_{t-2}$	(*p*-value = 0.001)	(*p*-value = 0.888)	(*p*-value = 0.944)

Notes: This table presents the results of estimating Equations 5) where the dependent variable is either *AbnRev* in year *t-1*, year *t* or year *t+1*. In each column the measurement of the control variables aligns with the measurement of the dependent variable (i.e., year *t-1*, year *t*, or year *t+1*). We cluster standard errors by firm. *T*-statistics are presented in parentheses below coefficient estimates. *, **, *** reflect statistical significance at the 10, 5, and 1 percent levels, respectively. Statistical significance is based on 2-tailed p-values. All variables are defined in the Appendix.

Table 6 Do Prior Reporting Aggressiveness and Peer Restatement Scrutiny Influence Cohort Firms' Responses to Peer Restatements?

Panel A: Consecutive vs. Non-Consecutive Years of Industry Peer Restatements

	Dependent Variable =		
	$AbnRev_{t-1}$	$AbnRev_t$	$AbnRev_{t+1}$
	(1)	(2)	(3)
$MultipleYearsIndRestatments_{t-1}$	0.001	0.000	0.001
	(0.75)	(0.32)	(0.75)
$SingleYearIndRestatment_{t-1}$	0.001	−0.000	−0.000
	(0.48)	(−0.31)	(−0.28)
$LowAbnRev_{t-2}$	0.007***	0.002***	−0.000
	(8.90)	(3.07)	(0.30)
$MultipleYearsIndRestatments_{t-1} \times LowAbnRev_{t-2}$	**−0.001**	**−0.000**	**0.000**
	(−0.51)	**(−0.22)**	**(0.02)**
$SingleYearIndRestatment_{t-1} \times LowAbnRev_{t-2}$	**−0.001**	**−0.000**	**0.002**
	(−0.77)	**(−0.11)**	**(0.95)**
$HighAbnRev_{t-2}$	−0.007***	−0.003***	−0.001
	(−9.25)	(−3.63)	(−1.41)
$MultipleYearsIndRestatments_{t-1} \times HighAbnRev_{t-2}$	**−0.005*****	**−0.002**	**0.001**
	(−3.19)	**(−1.18)**	**(0.53)**
$SingleYearIndRestatment_{t-1} \times HighAbnRev_{t-2}$	**0.000**	**0.001**	**0.001**
	(0.00)	**(0.46)**	**(0.28)**
N	21,227	21,227	18,152
Adjusted R^2	0.071	0.066	0.058
Fixed Effects	Firm	Firm	Firm
Controls Included	Yes	Yes	Yes
Test:			
$MultipleYearsIndRestatments_{t-1} \times HighAbnRev_{t-2} =$	F = 7.70	F = 0.98	F = 0.28
$MultipleYearsIndRestatments_{t-1} \times LowAbnRev_{t-2}$	(*p*-value = 0.006)	(*p*-value = 0.323)	(*p*-value = 0.596)
$MultipleYearsIndRestatments_{t-1} \times HighAbnRev_{t-2} =$	F = 5.64	F = 1.56	F = 0.03
$SingleYearIndRestatment_{t-1} \times HighAbnRev_{t-2}$	(*p*-value = 0.018)	(*p*-value = 0.212)	(*p*-value = 0.865)
$MultipleYearsIndRestatments_{t-1} \times HighAbnRev_{t-2} =$	F = 2.72	F = 0.55	F = 0.13
$SingleYearIndRestatment_{t-1} \times LowAbnRev_{t-2}$	(*p*-value = 0.099)	(*p*-value = 0.459)	(*p*-value = 0.722)

Panel B: Industry Peer Restatements with SEC investigations or fraud (Fraud) vs. Other Industry Peer Restatements (Other)

	Dependent Variable =		
	$AbnRev_{t-1}$	$AbnRev_t$	$AbnRev_{t+1}$
	(1)	(2)	(3)
$FraudIndRestatments_{t-1}$	0.000	0.001	0.001
	(0.20)	(0.37)	(0.63)
$OtherIndRestatment_{t-1}$	0.001	−0.000	−0.000
	(1.00)	(−0.02)	(−0.04)
$LowAbnRev_{t-2}$	0.006***	0.002***	0.000
	(8.90)	(3.05)	(0.28)
$FraudIndRestatments_{t-1} \times LowAbnRev_{t-2}$	**−0.000**	**−0.001**	**0.002**
	(−0.17)	**(−0.39)**	**(0.73)**
$OtherIndRestatment_{t-1} \times LowAbnRev_{t-2}$	**−0.001**	**−0.000**	**0.001**
	(−0.77)	**(−0.04)**	**(0.37)**
$HighAbnRev_{t-2}$	−0.007***	−0.003***	−0.001
	(−9.29)	(−3.66)	(−1.41)
$FraudIndRestatments_{t-1} \times HighAbnRev_{t-2}$	**−0.007****	**0.000**	**−0.002**
	(−2.57)	**(0.07)**	**(−0.86)**
$OtherIndRestatment_{t-1} \times HighAbnRev_{t-2}$	**−0.002**	**−0.001**	**0.001**
	(−1.50)	**(−0.65)**	**(1.03)**
N	21,227	21,227	18,152
Adjusted R^2	0.070	0.066	0.058
Fixed Effects	Firm	Firm	Firm
Controls Included	Yes	Yes	Yes
Test:			
$FraudIndRestatments_{t-1} \times HighAbnRev_{t-2} =$	F = 5.89	F = 0.22	F = 2.59
$FraudIndRestatments_{t-1} \times LowAbnRev_{t-2}$	(p-value = 0.015)	(p-value = 0.641)	(p-value = 0.107)
$FraudIndRestatments_{t-1} \times HighAbnRev_{t-2} =$	F = 2.87	F = 0.15	F = 1.72
$OtherIndRestatment_{t-1} \times HighAbnRev_{t-2}$	(p-value = 0.090)	(p-value = 0.697)	(p-value = 0.190)
$FraudIndRestatments_{t-1} \times HighAbnRev_{t-2} =$	F = 3.92	F = 0.01	F = 0.92
$OtherIndRestatment_{t-1} \times LowAbnRev_{t-2}$	(p-value = 0.048)	(p-value = 0.932)	(p-value = 0.338)

Notes: This table presents the results of estimating Equations 5) where the dependent variable is either *AbnRev* in year *t-1*, year *t* or year *t+1* and *IND Rev Restate* is replaced with indicators for one (*SingleYearIndRestatment*) versus consecutive years of industry peer restatements (*MultipleYearsIndRestatment*) in Panel A, and restatements associated with fraud or SEC investigations (*FraudIndRestatement*) versus other restatements (*OtherIndRestatments*). In each column the measurement of the control variables aligns with the measurement of the dependent variable (i.e., year *t-1*, year *t*, or year *t+1*). We cluster standard errors by firm. *T*-statistics are presented in parentheses below coefficient estimates. *, **, *** reflect statistical significance at the 10, 5, and 1 percent levels, respectively. Statistical significance is based on 2-tailed p-values. All variables are defined in the Appendix.

Table 7 Falsification and Robustness Tests

$AbnRev_{t-1} = \beta_1 Rev\ Restate_i + \beta_2 LowAbnRev_{t-2} + \boldsymbol{\beta_3 Rev\ Restate_{i,t-1} \times LowAbnRev_{t-2}} + \beta_4 HighAbnRev_{t-2} + \boldsymbol{\beta_5 Rev\ Restate_i \times HighAbnRev_{t-2}} +$
$\qquad \beta_6 CONTEMP\_IND\%_{i,t-1} + \beta_7 BMR_{i,t} + \beta_8 Leverage_{i,t} + \beta_9 LnAssets_{i,t} + \beta_{10} ROA_{i,t} + \beta_{11} STDEarn_{i,t} + \beta_{12} Return_{i,t} + \beta_{13} Herfindex_{i,t} +$
$\qquad \beta_{14} IndBMR_{i,t} + \varepsilon_{i,t}$

	Dependent Variable = $AbnRev_{t-1}$					
	Our Main Model (Reproduced from Table 5)	Falsification Test	Chen et al. Model	Only Inc. Decreasing Restatements	Restatements Defined within MSA	Post-ASC 606
	(1)	(2)	(3)	(4)	(5)	(6)
Rev Restate	0.001	–0.020	0.001	0.001	0.0007	0.002
	(0.75)	(–0.830)	(1.170)	(0.870)	(0.720)	(0.790)
LowAbnRev$_{t-2}$	0.005***	0.006***	0.005***	0.005***	0.0059***	0.013***
	(3.69)	(9.710)	(3.580)	(3.750)	(5.480)	(3.930)
IND Rev Restate$_{t-1}$ × LowAbnRev$_{t-2}$	0.002	0.001	0.002	0.002	0.005	0.000
	(1.20)	(0.380)	(1.230)	(1.110)	(0.400)	(–0.110)
HighAbnRev$_{t-2}$	–0.005***	–0.008***	–0.005***	–0.005***	–0.008***	–0.016***
	(–4.28)	(–12.050)	(–4.380)	(–4.150)	(–7.430)	(–5.300)
Rev Restate$_{t-1}$ × HighAbnRev$_{t-2}$	**–0.003****	**0.002**	**–0.003****	**–0.003****	**0.001**	**–0.002**
	(–2.180)	**(0.800)**	**(–2.130)**	**(–2.280)**	**(0.410)**	**(–0.550)**
Rev Restate =	*IND Rev Restate$_{t-1}$*	*Pseudo IND Rev Restate*	*IND Rev Restate$_{t-1}$*	*IND Rev Restate2$_{t-1}$*	*MSA Rev Restate$_{t-1}$*	*IND Rev Restate$_{t-1}$*
N	21,227	21,227	21,227	21,210	21,169	3,887
Adjusted R^2	0.071	0.070	0.089	0.070	0.070	0.112
Fixed Effects	Firm	Firm	Firm	Firm	Firm	Firm

Notes: This table presents the results of estimating Equations 5) where the dependent variable is either *AbnRev* in year *t-1*, year *t* or year *t+1*. In each column the measurement of the control variables aligns with the measurement of the dependent variable (i.e., year *t-1*, year *t*, or year *t+1*). We cluster standard errors by firm. *T*-statistics are presented in parentheses below coefficient estimates. *, **, *** reflect statistical significance at the 10, 5, and 1 percent levels, respectively. Statistical significance is based on 2-tailed p-values. *Pseudo IND Rev Restate* is an indicator set equal to one when *IND Rev Restate2$_{t-1}$* is defined the same way as *IND Rev Restate$_{t-1}$* expect that we only set *IND Rev Restate2$_{t-1}$* equal to 1 if the peer restatement results in the peer reducing revenue. MSA Rev Restate$_{t-1}$ is an indicator set to one if a peer, defined as a firm operating in the same MSA as the cohort reported a restatement of revenue in year t-1 and zero otherwise. All other variables are defined in the Appendix. Control variables are included, but not tabulated.