Post-litigation reporting conservatism[⋆]

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ABSTRACT

We investigate changes in financial reporting conservatism arising from shareholder lawsuits. We find that sued firms respond to 10b-5 litigation with increased accounting conservatism. Consistent with a spillover effect, we also find that non-sued peer firms that share an auditor increase accounting conservatism following litigation. Despite the FASB having eliminated conservatism as an essential qualitative characteristic from the conceptual framework in 2010, we find the post-suit increase in conservatism persists after 2010, suggesting a capital market demand for conservatism even without regulator intervention. Our results, which support the notion that litigation events induce accounting conservatism, extend prior studies examining the disclosure effects of litigation into the accounting choice effects of litigation.

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ABSTRACT

We investigate changes in financial reporting conservatism arising from shareholder lawsuits. We find that sued firms respond to 10b-5 litigation with increased accounting conservatism. Consistent with a spillover effect, we also find that non-sued peer firms that share an auditor increase accounting conservatism following litigation. Despite the FASB having eliminated conservatism as an essential qualitative characteristic from the conceptual framework in 2010, we find the post-suit increase in conservatism persists after 2010, suggesting a capital market demand for conservatism even without regulator intervention. Our results, which support the notion that litigation events induce accounting conservatism, extend prior studies examining the disclosure effects of litigation into the accounting choice effects of litigation.

1. Introduction

Academics posit that accounting conservatism, which has played an important role in financial reporting for centuries, helps mitigate litigation costs because shareholder litigation for overstating net assets is much more common than for understating net assets (Kellogg, 1984; Beaver, 1993; Watts, 1993, 2003). There is substantial research examining the relation between *ex ante* litigation risk and accounting conservatism and how shareholder litigation affects disclosure (e.g., see discussion by Billings et al., 2021).[1] In contrast, we are not aware of studies examining how firms' financial statement conservatism changes after being sued by shareholders. We address this deficiency by examining changes in financial statement conservatism following 10b-5 shareholder lawsuits.

Litigation may alter future behavior, much like governments intend that a speeding ticket discourages future speeding (Becker, 1968). Research from psychology supports that negative events increase risk aversion (e.g., Forgas, 1995; Lerner and Keltner, 2001). Because theory (e.g., Watts and Zimmerman, 1986; Skinner, 1994) and empirical evidence (e.g., Qiang, 2007; Ettredge et al., 2016) support that conservatism reduces *ex ante* litigation risk, the costs of litigation may become more salient to managers following lawsuits. Thus, they could become more conservative with financial reporting after being sued by shareholders.[2] In other words, litigation could motivate managers to increase their conservatism, much like drivers slow down, at least temporarily, following a speeding ticket.

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[1] Due to the vastness of the literature on *ex ante* litigation risk and accounting conservatism, we discuss only the most relevant of these studies.

[2] SEC Rule 10b-5 encourages timely disclosure of all material information. However, enforcement of Rule 10b-5 is *asymmetric*: untimely disclosure of *bad* news is more likely to result in SEC enforcement and litigation than untimely disclosure of *good* news (Kellogg, 1984; St. Pierre and Anderson, 1984). Prior research argues that litigation is one of the main explanations for the existence of accounting conservatism (Basu, 1997; Watts, 2003; Qiang, 2007).

However, there are reasons why managers might not increase conservatism after litigation. Prior studies associate conservatism with lower earnings quality and lower earnings informativeness (Dichev and Tang, 2008; Heflin et al., 2015). The FASB currently takes this view, as demonstrated by the recent elimination of conservatism as an essential qualitative characteristic of financial reporting (FASB, 2010). Managers will not increase conservatism if they view the costs of decreased earnings informativeness as too high relative to the benefits of reduced litigation risk. Further, managers may become more conservative with earnings guidance (Billings et al., 2021) which could substitute for accounting conservatism.

Examining litigation's effect on conservatism is important for at least two reasons. First, one intended consequence of shareholder litigation is to change managers' financial reporting behavior. Some prior research examines the effect of litigation on changes in managers' voluntary disclosure decisions. However, prior research does not examine the effect of litigation on managers' financial statement recognition decisions.

Second, examining managers' responses to shareholder litigation offers some identification advantages. For example, designs in early studies are limited to cross-sectional tests examining associations between litigation risk proxies and financial reporting/disclosure choices (e.g., Skinner, 1997; Field et al., 2005).[3] More recently, studies address endogeneity by exploiting shocks in disclosure regulation rules (e.g., Naughton et al., 2019; Manchiraju et al., 2021). However, such studies often face other limitations, including an alignment of event and calendar time, which increases the risk of confounds from concurrent economic events, shocks that do not

[3] Endogeneity is an issue with this approach because firms with higher *ex ante* litigation risk may disclose more, making it appear that firms that disclose more are sued more. Other studies adopt a cross-country comparison where the legal system differs from the U.S. Cross-country studies face the challenge of controlling for varying economic and cultural features.

materially change litigation risk, and potential violations of the parallel trends assumption (e.g., see discussions by Bertomeu et al., 2016, and Donelson et al., 2021). In contrast, our event study analysis exploits multiple lawsuits staggered through time. Further, our models include firm and quarter fixed effects and compare sued firms to control firms that are matched on *ex ante* litigation risk. Our setting and difference-in-difference design limit the possibility that the conservatism changes we identify are due to economy-wide events or some other firm characteristic, such as *ex ante* litigation risk, rather than the actual lawsuits. Another benefit of our approach is that, unlike many prior studies examining litigation risk and financial reporting/disclosure, we examine firms that are sued rather than relying on a model to identify treatment firms.[4]

We construct our sample from the Securities Class Action Services (SCAS) of Institutional Shareholder Services (ISS) dataset. We measure pre- and post-lawsuit conservatism levels by the widely used asymmetric timeliness (AT) of earnings metric (Basu, 1997) because we are interested in conditional conservatism and AT has been described as "the most direct implication of conditional conservatism" (Ryan, 2006). Though AT is widely used to identify conditional conservatism, we recognize that several studies suggest the measure is subject to biases. We include firm fixed effects in all our tests to minimize bias in the AT measure (Ball et al., 2013). We find that the financial statements of sued firms become significantly more conservative in the year of the litigation and remain more conservative for up to three years.

We next investigate potential spillover effects of litigation on the conservatism of non-sued firms. Lowry (2009) argues that to gain a broader understanding of the consequences of litigation requires examination of both sued and non-sued firms. She argues that because non-sued firms

[4] Recent research suggests caution when using predicted values from a regression in subsequent statistical tests. For example, see Chen et al. (2023) and Byzalov and Basu (2024).

may react differently to litigation, evidence from sued firms cannot be generalized to non-sued firms. We examine the post-litigation financial reporting changes of non-sued firms that are clients of the same audit office as sued firms. Auditors play an important governance role and evidence from Lennox and Li (2014) suggests that audit firms are sensitive to litigation and that litigation against auditors can affect their clients' financial reporting through improved audit quality. We find non-sued client firms of the same audit office as sued firms increase their conservatism after litigation. These spillover results imply litigation induces a change in reporting behavior in not only the sued firms but also in non-sued firms.

We also investigate litigation's influence on conservatism in a regime with decreased emphasis on conservatism. In recent years, regulators, standard setters, and academics have increasingly questioned the desirability of conservatism. The FASB removed conservatism from its Conceptual Framework because it violates neutrality (FASB, 2010). Results in several studies suggest that conservatism impedes the persistence and informativeness of accounting earnings (Givoly and Hayn, 2002; Dichev and Tang, 2008; Chen et al., 2014; Barth et al., 2014; Heflin et al., 2015). We examine whether litigation driven changes in conservatism persist after standard setters removed conservatism as a desired attribute from the Conceptual Framework (FASB, 2010). We find that the post-suit spike in conservatism persists post-2010. We infer that conservatism increases after shareholder litigation even when standard setters deemphasize conservatism.

We perform several robustness analyses. First, we compare post-suit conservatism to pre-suit conservatism in (1) the pre-damage period and (2) the combination of the pre-damage and damage periods and find consistent results with both.[5] Second, we perform a falsification test

[5] In section 3, we discuss the pros and cons of these two periods as benchmarks for pre-suit conservatism. In our opinion, the pre-damage period provides the cleanest pre-suit benchmark as it ensures the AT measure is unaffected

where we assign random pseudo litigation dates to the sued firms (such that the pseudo-pre and pseudo-post litigation periods are entirely either pre- or post-actual-litigation) to provide evidence that our results are driven by lawsuits rather than some other unidentified factor that leads to a decrease in conservatism. Third, we repeat our main analysis after removing observations where the sued firm faced multiple lawsuits resulting in overlapping observations and after removing restatement observations as prior research finds that firms that restate earnings report more conservatively (Ettredge et al., 2012). Results from all these analyses support our primary inference that litigation leads to more accounting conservatism.

We make several contributions. We contribute to research addressing managers' financial reporting responses to litigation. Various papers study either the *ex ante* or *ex post* effects of litigation on voluntary disclosure (e.g., Skinner, 1994 *ex ante*; Rogers and Van Buskirk, 2009 and Billings et al., 2021 *ex post*). We study the *ex post* effects of litigation on accounting recognition, specifically accounting conservatism in financial statements, rather than on voluntary disclosure. Our evidence provides a more complete picture of post-litigation changes in financial reporting by documenting that managers' responses to litigation involve accounting choices within mandatory financial reports in addition to the voluntary disclosure changes documented by prior studies. This distinction is important because managers face different benefits and costs regarding voluntary disclosure and mandatory financial reports (Noh et al., 2019), so results from voluntary disclosure studies may not extend to mandatory financial reporting choices. Further, though voluntary disclosure is an important part of the financial reporting process, understanding the factors influencing managers' financial recognition choices is of fundamental importance to accounting

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by both the large negative stock price declines firms typically experience prior to litigation and managers' alleged misrepresentation during the damage period.

researchers and regulators. For various reasons, including contracting and decision usefulness to investors and creditors, disclosure is likely not a complete substitute for financial statement recognition (e.g., FASB, 1984; Michels, 2017).

We also provide novel evidence for a primary explanation for the existence of accounting conservatism. Watts (2003) argues shareholder litigation is an important factor explaining the demand for conservatism. Prior research on litigation and accounting conservatism is largely limited to *ex ante* effects.[6] For example, some studies find more conservative firms are less likely to be sued and incur lower litigation costs (Qiang, 2007; Ettredge et al., 2016). We differ from these studies in that we study the consequences of litigation for conditional conservatism, rather than the consequences of conditional conservatism for litigation outcomes. Other studies conduct general association tests using proxies for litigation risk or legal regime changes (e.g., Qiang, 2007; Khan and Watts, 2009; Tan, 2018). Manchiraju et al. (2021) examine how firms' conservatism changes around staggered regime changes that decreased litigation risk. Somewhat surprisingly, they find that, on average, firms are more conservative after the change, but some firms become less conservative. They conclude that the relation between the litigation environment and reporting conservatism is complex. We also study the relation between litigation and conservatism in financial statements, but we differ in that we study changes in financial statement conservatism after firms are sued. We also differ from such studies because, instead of examining the influence of litigation risk, we attempt to control for the influence of litigation risk by comparing sued firms to control firms with similar *ex ante* litigation risk.

[6] In contrast, consider the contracting literature, which provides evidence supporting both *ex ante* (e.g., Zhang, 2008) and *ex post* (e.g., Tan, 2013) effects of contracting on conservatism. Watts (2003) argues that litigation and contracting are the two most important factors explaining the demand for conservatism.

Our study also speaks to litigation's effectiveness as a governance mechanism. Rogers and Van Buskirk (2009) conclude that regulatory and contractual intervention is needed given their finding that managers reduce disclosure following litigation. However, recent work by Billings et al. (2021) suggests that litigation does act as an effective governance mechanism, as they find that managers respond to litigation with more conservative voluntary disclosure. Our results add to this debate and provide evidence, within the accounting choice setting, that litigation is effective as a governance mechanism if an objective of litigation is to increase accounting conservatism.

Further, we extend the literature investigating the spillover of accounting choices amongst peer firms to a litigation setting. Recent studies provide compelling evidence of financial reporting spillover, such as earnings management contagion and audit quality network effects (Francis and Michas, 2013; Lennox and Li, 2014; Kedia et al., 2015; Dechow and Tan, 2021). We document a spillover in conservatism triggered by a peer firm litigation event. To our knowledge, we are the first to identify a spillover effect in financial reporting conservatism.

Finally, we add to debates on the diminishing relevance of conservatism. We counter the widespread belief that conservatism is becoming less relevant by finding that shareholder litigation triggers an increase in conservatism even when standard setters have deemphasized conservatism. Some prior research suggests adverse market consequences could result from diminished emphasis on conservatism. For example, LaFond and Watts (2008) argue that if conservatism were eliminated, information asymmetry between investors would increase. Chen et al. (2007) suggest earnings management could increase and contract efficiency with managers could decrease in the absence of conservatism. Our results highlight a shareholder-induced demand for accounting conservatism, which mirrors the evidence regarding conservatism demands of monitoring institutions (Ramalinegegowda and Yu, 2012). Given the results, including ours, suggesting a

market-driven demand for conservatism, we believe more research is warranted regarding the decline in regulatory emphasis on conservatism, particularly in the context of litigation.

2. Background and Hypotheses Development

2.1. Background

U.S. securities markets are highly litigious, and the legal costs associated with securities class action filings against U.S. listed firms are substantial.[7] SEC Rule 10b-5 governs securities class action lawsuits involving fraud or deceit in connection with the purchase or sale of a security. To establish a claim under Rule 10b-5, plaintiffs need to show: [i] an omission or misstatement (Manipulation) of [ii] a material fact (Materiality) [iii] made with intent (Scienter) [iv] which the plaintiff justifiably relied on (Reliance) [v] causing material damage in connection with the purchase or sale of a security (Loss Causation & Damages). Most 10b-5 cases are triggered by a disclosure event that reveals previously misstated or over-stated financial statements, a previous overly-optimistic voluntary disclosure, or the lack of a sufficiently pessimistic previous disclosure. The revealing disclosure triggers a large stock price decline (Skinner, 1994; Files, et al., 2009).[8]

It is important to note that the omission or misstatement of a material fact that plaintiffs rely on for claims under Rule 10b-5 can pertain to any form of communication from management. That is, Rule 10b-5 lawsuits can relate to managers' accounting recognition choices (e.g., reporting of GAAP earnings) as well as managers' voluntary disclosures. In other words, "omission of a

[7] According to a 2019 Cornerstone Research annual report, 8.9% of all U.S. listed companies were targets of securities lawsuits in 2019. The average settlement amount since the Private Securities Litigation Reform Act of 1995 (PSLRA) is about $50 million. Indirect costs, such as lost time and attention from management are also likely substantial.

[8] The fraud-on-the-market theory, which asserts that the price of a security is a function of all material (and relevant) information regarding the entity, heavily influences the civil enforcement of SEC Rule 10b-5. Thus, a common argument in securities litigation is that a sudden decline in stock price accompanied by a news disclosure is evidence of omitted or inaccurate prior disclosure.

material fact" includes providing financial statements that convey a financial performance or position that is (*ex post*) too positive.

Two examples help illustrate. The first involves revenue recognition by Symbiotic, Inc. Plaintiffs argued that Symbiotic implemented percentage-of-completion revenue recognition too aggressively in 2024 by recognizing expenses and corresponding revenue prematurely. Symbiotic also recognized revenue on some cost overruns that it later determined were not billable.[9] Another example is Regions Financial Corp. Plaintiffs argued that Regions did not establish adequate loan loss reserves in 2007-2008 and failed to write-down goodwill, despite indications of problems with the loan portfolio of a recently acquired bank holding company.[10] Both cases involve management failing, according to plaintiffs, to be adequately conservative with accounting recognition rules.[11]

2.2. Research on Financial Accounting Recognition and Ex Ante Litigation Risk

Accounting recognition is the incorporation of economic events or transactions into financial statements. Conservatism in accounting recognition results in a lower threshold for incorporation of bad news into financial statements than for good news (e.g., Basu, 1997). Positive accounting theory predicts that litigation threats induce accounting conservatism as a preemptive strategy. For example, Watts (2003) argues that understating net assets is less likely to precipitate

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[9] See the Sybmiotic News Release at https://ir.symbotic.com/news-releases/news-release-details/symbotic-provides-update-restatement-fiscal-year-2024-financial. Symbiotic has since restated its 2024 results. The case is pending resolution.

[10] See https://www.casemine.com/judgement/us/5914b085add7b04934753715. The case was subsequently dismissed.

[11] We note that not all lawsuits in our sample necessarily claim managers were insufficiently conservative with accounting choices. However, managers' accounting choices can plausibly be influenced by shareholder litigation, even if that litigation does not involve an accounting recognition issue if managers' risk aversion motivates them to be more conservative in multiple reporting dimensions, including accounting recognition. In this regard, we are similar to Billings et al. (2021) and Rogers and Van Buskirk (2009), who find managers change guidance decisions after shareholder lawsuits even though the suits do not always involve guidance. This is also similar to studies examining shocks to general litigation risk and specific financial reporting choices such as management guidance (Naughton et al., 2019), conservatism (Manchiraju et al., 2021), and real earnings management (Huang et al. (2020).

litigation than overstating net assets.[12] Prior research supports this hypothesis by finding that conservative financial reporting is associated with lower litigation risk (Qiang, 2007; Ettredge et al., 2016) and with less severe litigation outcomes (Ettredge et al., 2016). Additionally, LaFond and Watts (2008) predict that economic shocks triggering an increase in information asymmetry between claimholders of the firm (e.g., private lawsuits, restatement announcements) will lead to increased conservatism. In general, prior research suggests conservatism in accounting recognition (both by the reporting choices managers make and by mandated accounting standards) helps deter litigation and reduces expected litigation costs. In contrast to these prior studies examining how firms' recognition choices influence litigation, we examine how managers' accounting recognition changes in response to litigation events.[13]

A few recent studies examine firms' financial reporting decisions in response to *ex ante* changes in litigation risk. Manchiraju et al. (2021) examine how firms' conservatism changes around the staggered adoption of Universal Demand Laws (UDLs), which decrease litigation risk by making it harder to bring derivative lawsuits against directors and officers.[14] Somewhat surprisingly, they find an on average increase in reporting conservatism in response to the decrease in litigation risk. This increase in conservatism is driven by firms issuing equity and those with stronger governance characteristics. In contrast, they find that firms with incentives to report aggressively reduced conservatism. Basu and Liang (2019) examine the staggered adoption of

[12] Discussion of conservatism in accounting recognition and litigation risk predates Watts (2003). For example, see also Beaver (1993), Watts (1993), Basu (1997), and Holthausen and Watts (2001).

[13] The more developed litigation and disclosure literature examines both *ex ante* and *ex post* relations between litigation and disclosure and generally supports the notion that disclosure helps deter litigation (e.g., Skinner, 1994; Field et al., 2005; Donelson et al., 2012; Billings and Cedergren, 2015; Frost and Pownall, 1994; Johnson et al., 2001; Baginski et al., 2002), and exception being Francis et al. (1994). Most relevant to our study are studies examining the *ex post* effects of litigation on disclosure. These studies find that after litigation, firms provide less disclosure (Rogers and Van Buskirk, 2009) yet provide more conservative disclosure (Billings et al., 2021).

[14] Notably, Donelson et al. (2021) provide evidence that UDL adoptions had no meaningful effect on litigation, calling into question the use of UDLs as an exogenous shock to litigation risk and highlighting the benefit of our setting, which examines actual litigation events staggered over time.

laws that limited non-officer directors' litigation risk and find firms responded with decreased conditional conservatism. Huang et al. (2020) find that, following an unexpected U.S. Ninth Circuit Court ruling that reduced litigation risk, firms increased real earnings management.

We differ from these and other prior studies which find that changes in *ex ante* litigation risk influence firms' accounting choices, by examining how firms' accounting choices change *after* facing litigation. Examining the consequences of actual litigation rather than changes in litigation risk is important because, a priori, we cannot be certain that litigation increases managers' perceived probability of future litigation. Firms that are sued likely had higher pre-suit litigation risk and presumably established the level of reporting conservatism they believed appropriate considering the litigation risk they faced. Managers may expect a certain number of suits per five- or ten-year period, given their firm's accounting conservatism and other factors. Experiencing a suit may be consistent with their expectations and, if reporting conservatism is costly, managers may not increase reporting conservatism. Notably, we attempt to control for the influence of *ex ante* litigation risk in our analyses to help ensure we identify the effect of litigation rather than *ex ante* litigation risk. Thus, our results are incremental to the results from prior studies examining the influence of litigation risk.

2.3. Development of Hypotheses for Litigation and Ex Post Accounting Conservatism

Existing research suggests there are reasons why managers might either increase or decrease accounting conservation after shareholder litigation. For example, one theory is that litigation has a correctional effect. The correctional effect theory suggests managers believe their past disclosures were inadequate and increase disclosure to reduce the odds of future litigation. This is consistent with research from psychology suggesting that negative events increase risk aversion (e.g., Forgas, 1995; Lerner and Keltner, 2001). Indeed, Billings et al. (2021) find a

reduction in good-news management earnings forecasts and that bad-news forecasts (i.e., warnings) increase after litigation. The evidence of Billings et al. (2021) is consistent with litigation producing a correctional effect on disclosure.

Given the evidence that conservatism is, in part, a response by managers (and accounting standard setters) to *ex ante* litigation risk, extending the "correctional effect" theory of post-litigation disclosure suggests that managers would make more conservative accounting recognition choices after experiencing litigation. Consequently, managers with increased risk aversion (and perhaps encouraged by auditors or boards of directors), might become more conservative with revenue recognition or more quickly write-down inventories, fixed assets, and intangible assets. By making more conservative accounting recognition choices, managers reduce the stated values of net assets and therefore reduce future litigation risk (e.g., Watts, 2003).

However, there are several reasons why litigation might not induce an increase in accounting conservatism. First, there may be costs associated with additional conservatism. For example, evidence in some prior studies associates conservatism with lower earnings quality and lower informativeness of earnings (e.g., Givoly and Hayn, 2002; Dichev and Tang, 2008; Chen et al., 2014; Barth et al., 2014; Heflin et al., 2015).[15] Thus, managers might believe the information related costs of greater conservatism outweigh the litigation related benefits. The FASB takes a similar view, as demonstrated by the recent elimination of conservatism as an essential qualitative characteristic of financial reporting (FASB, 2010). Second, not all prior research finds the more expected positive relation between litigation risk and conservatism. One notable example is

[15] Givoly and Hayn (2002) suggest conservatism can create bias and noise in financial reporting, which increases asymmetry in the timely recognition of economic events into income. This is consistent with evidence from Heflin et al. (2015) that conservatism dampens earnings persistence and income smoothing, thereby compromising earnings informativeness. Similarly, Dichev and Tang (2008) and Chen et al. (2014) find conservatism lowers earnings persistence while increasing earnings volatility. Evidence from Barth et al. (2014) suggests conservative accounting practices lead investors to fixate more on negatively skewed earnings.

Manchiraju et al. (2021), who report an on-average *increase* in reporting conservatism when firms experience a decrease in litigation risk. However, they also find that firms facing incentives to be less conservative *reduce* their conservatism and conclude that the relation between the litigation environment and reporting conservatism is complex. Third, because conservatism is built into GAAP accounting recognition rules, managers of sued firms may have insufficient latitude to materially increase accounting conservatism.

Another reason we may not find an observable increase in conservatism after litigation relates to how firms change voluntary disclosure following litigation and how we identify conservatism. Billings et al. (2021) find that managers' bad news forecasts become more frequent and timelier following litigation, suggesting bad news reaches investors more quickly. In fact, if managers increase their accounting conservatism at the same rate they increase the timeliness of their disclosure of bad news, we could observe no change in the AT measure even though managers are becoming more conservative (with both accounting recognition and disclosure).

Given the competing arguments regarding the effects of litigation on post-litigation accounting conservatism, we state our first hypothesis, in null form, as the following:

Hypothesis 1. *Accounting conservatism does not change after a shareholder lawsuit filing*.

Another important policy goal of the regulation underlying the SEC's enforcement mechanism is to discourage other related firms from poor financial reporting practices (SEC, 2003, 2008). Consistent with this objective, we study spillover effects of litigation on non-sued firms that are related to the sued firm. Shareholder lawsuits citing financial reporting misconduct by other firms inform peer firms regarding the details of the allegations and the consequences of engaging in such practices. When managers observe litigation against peers, they likely increase

their assessment of the likelihood that they might also be sued. Further, observing the litigation costs borne by peer firms may make the costs of litigation more salient to the manager. Returning to the speeding ticket analogy, a driver who observes another driver pulled over is likely to respond by decreasing their own driving speed even though they are not the driver currently being pulled over. Thus, after observing the shareholder lawsuit against a peer, non-sued firms may increase their own accounting conservatism.[16]

However, litigation against sued firms may have no effect or even the opposite effect on the financial reporting of non-sued firms. Non-sued firms may simply view peer litigation as irrelevant. In a discussion of the effect of litigation on disclosure, Lowry (2009) argues that non-sued firms may respond to litigation in the opposite direction of sued firms. For example, managers of non-sued firms may conclude that their current disclosure practices helped them to avoid a lawsuit. As such, they may maintain or even increase their disclosure levels in the future. Lowry's (2009) reasoning could also apply directly to accounting recognition. Firms that were not sued may feel emboldened by the lawsuits against peer firms because their current accounting practices did not attract the attention of shareholders. As such, non-sued firms may feel that they have some leeway to increase the aggressiveness of their financial reporting before it reaches a level of aggressiveness that may attract the attention of shareholders in the form of litigation. Thus, non-sued firms may respond to peer firm litigation by decreasing their level of conservatism. We state our second hypothesis, in null form, as the following.

[16] Prior studies support the notion of financial reporting spillover effects across firms. Reppenhagen (2010) finds that firms' choice of accounting methods is affected by the accounting choices of related firms. Further, Gleason et al. (2008) find that some restatements cause investors to reassess the financial information of non-restating peer firms.

Hypothesis 2. *Non-sued peer firms do not alter their accounting conservatism in response to peer firm litigation*.

3. Research Design

3.1. Event Timeline

We identify four distinct periods for sued target firms: pre-damage, damage, revelation, and post-litigation. As depicted in Figure 1, we define the pre-damage period as the four quarters immediately preceding the beginning of the class action period.[17] We identify the damage period by the beginning and end dates of the class action filing. The damage period varies by lawsuit and can be more or less than four quarters. For each lawsuit, we identify one quarter as the revelation quarter. The revelation quarter is the quarter that ends within the 90 days following the federal filing date of the suit. We exclude the revelation quarter from our regressions to ensure our results are not influenced by the revelation of the lawsuit.[18] We define the post-litigation period as the four quarters following the lawsuit's filing date.

To infer the treatment effect of shareholder litigation on conservatism, we must identify a pre-event benchmark period against which to compare the post-litigation period. One option is the damage period. However, during the damage period, managers have allegedly misrepresented or omitted material facts and the firm typically experiences a large negative stock price decline. Both factors may influence measures of accounting conservatism. Consequently, changes in measured reporting conservatism from the damage period to the post-litigation period may be a result of

[17] The class action period is the period during which investors are alleged to have suffered losses due to actions by management. See https://www.investor.gov/introduction-investing/investing-basics/glossary/class-actions.
[18] Our approach is similar to Rogers and Van Buskirk (2009) in that they also exclude a revelation period. Rogers and Van Buskirk (2009) examine disclosures and define the revelation period as 10 days. We define the revelation period as a quarter because our unit of observation is a firm-quarter due to our need to use quarterly earnings information to measure conditional conservatism.

different economic situations rather than a change in accounting policy. Instead, we use the *pre*-damage period as our pre-event benchmark period. While using the pre-damage period as the benchmark avoids the potential influence of the damage period on our measure of accounting conservatism, a drawback is that some observations will have multiple years between the benchmark (pre-damage) and treatment (post-litigation) periods. This extended time difference may introduce noise into our tests to the extent a firm changes its accounting conservatism for reasons unrelated to the litigation. To mitigate this concern, we also present results where we use the combined pre-damage and damage periods as the benchmark period.

3.2. Identification Strategy

We employ a difference-in-difference design utilizing treatment and control firms. Our treatment sample consists of firms sued under Rule 10b-5. To address the possibility that changes in conditional conservatism are related to the level of litigation risk rather than the litigation, we construct a sample of control firms that are not sued but have litigation risk similar to the pre-suit litigation risk of the sued firms.[19] Specifically, we first estimate litigation risk using the Kim and Skinner (2012) model in the month prior to the start of the damage period for each sued firm and then for the same month for all potential non-sued control firms. For each sued firm, we identify the five non-sued firms with the closest litigation risk and use those five non-sued firms as control firms. We choose control firms without replacement. Thus, our non-sued control sample is similar in pre-suit litigation risk to our treatment sample of sued firms.[20] We estimate the change in

[19] The use of a control sample with similar litigation risk is important given the inter-temporal properties of accounting conservatism (Glover and Lin, 2018) and given lower conservatism is associated with a higher likelihood of litigation (Ettredge et al., 2016). Firm fixed effects would not control for within-firm, inter-temporal changes in conservatism.

[20] We use the Kim and Skinner (2012) litigation risk model that includes a high litigation risk industry proxy, lagged assets, lagged sales growth, and lagged stock return variables (market-adjusted return, return skewness, return standard deviation, and turnover). Diagnostics indicate the control firms are a good match to the treatment firms in terms of litigation risk. The mean distance between treated and matched firms is only about 0.02% and more than 99% of matches are within 1%, suggesting our treatment and control firms have similar levels of litigation risk.

conditional conservatism for treatment firms (first difference) and for control firms and then compare the change for treatment firms to control firms (second difference). We assign the same dates for the four periods (described in Section 3.1) for each treatment firm to its corresponding control firm to facilitate the difference-in-difference design.

Our primary measure of accounting conservatism is obtained from estimates of Basu (1997) asymmetric timeliness (AT) regressions. The AT regressions capture the differential timeliness in reported earnings of bad relative to good news by regressing earnings on returns, an indicator for negative returns, and an interaction of the negative return indicator with returns. We augment the Basu (1997) model with an indicator for the post-filing period.[21] We use size and book-to-market adjusted returns as our independent variables to control for variation in conditional conservatism attributable to firm size and growth opportunities (e.g., LaFond and Roychowdhury, 2008; LaFond and Watts, 2008). We include firm and quarter fixed-effects in all regressions. The inclusion of firm fixed-effects is particularly important in our setting as firm fixed-effects substantially reduce potential bias in the AT measure (Ball et al., 2013).

Specifically, we estimate the following regression:

$$\begin{aligned} NI = \; & b_0 + b_1 Ret + b_2 Neg + b_3 Post + b_4 Ret \times Neg + b_5 Ret \times Post + b_6 Neg \times Post \\ & + b_7 Ret \times Neg \times Post \\ & + d_0 Treat + d_1 Treat \times Ret + d_2 Treat \times Neg + d_3 Treat \times Post \\ & + d_4 Treat \times Ret \times Neg + d_5 Treat \times Ret \times Post + d_6 Treat \times Neg \times Post \\ & + d_7 Treat \times Ret \times Neg \times Post \\ & + c_j' \boldsymbol{Daint} + f_j' \boldsymbol{Fixed\ Effects} + Treat \times g_j' \boldsymbol{Fixed\ Effects} + e \end{aligned} \tag{1}$$

The unit of observation in equation (1) is a firm-quarter. *NI* is the firm's scaled (by book value of equity, Compustat data item *ceqq*) income before extraordinary items (Compustat data

[21] Estimating AT regressions in this setting requires quarters with both positive and negative returns to measure the differential reverse earnings response coefficient. In untabulated analyses, we find that the majority of our sample firms have quarters with both positive and negative returns in all defined lawsuit periods.

item *ibq*) for quarter *t*. *Ret* is the size and book-to-market adjusted cumulative monthly stock returns over quarter *t*, cumulated from one day following quarter *t-1*'s earnings announcement through quarter *t*'s announcement (Compustat data item *rdq*). *Neg* equals zero if *Ret* is positive and one if *Ret* is negative. *Damage* equals one if quarter *t* ends within the damage period as depicted in Figure 1. *Post* equals one if a firm-quarter ends in the four calendar quarters after the filing date of the suit, corresponding to the "Post-Filing Period" in Figure 1. *Treat* equals one if the firm is a treatment firm (i.e., has been sued) and zero if the firm is a control sample firm. *Damage* equals one if the firm-quarter ends in the damage period. ***Daint*** is a vector that includes *Damage* and its interactions with *Neg*, *Ret*, *Neg* × *Ret*, *Treat*, *Treat* × *Neg, Treat* × *Ret,* and *Treat* × *Neg* × *Ret.* $\boldsymbol{c_j}$ is a vector of the coefficients c_1 to c_8. ***Daint*** controls for possible changes in asymmetric timeliness during the damage period relative to the pre-damage period. $\boldsymbol{f_j}$ and $\boldsymbol{g_j}$ are coefficient vectors.

In equation (1), b_4 is the asymmetric timeliness coefficient associated with the asymmetric recognition of bad versus good news for control firms during the pre-damage period and b_7 captures the difference in asymmetric timeliness between the post-litigation and pre-damage period for control firms. d_4 captures the difference in asymmetric timeliness between treatment and control firms during the pre-damage period. Our main coefficient of interest is d_7, which captures the difference in asymmetric timeliness between treatment and control firms during the post-litigation period relative to the pre-litigation period (i.e., our difference-in difference coefficient). If sued firms become more (less) conservative with accounting recognition after being sued relative to control firms, we expect d_7 to be positive (negative).

3.3. Data and Sample Selection

To identify information regarding U.S. securities lawsuits, we rely on the Securities Class Action Services (SCAS) database archived by Institutional Shareholder Services (ISS). This dataset contains a comprehensive record of disclosure related shareholder allegations, and has been used by prior studies (e.g., Donelson et al., 2013). This dataset allows us to filter our sample based on identifiers of firms/cases, lawsuit filing dates, codes pertaining to specific allegations, the beginning and ending dates of class action damage period, and information on the current disposition status of each lawsuit. To control for confounds from the Private Securities Litigation Reform Act (PSLRA), we limit our sample to shareholder lawsuits filed after its enactment in 1995.

Table 1 summarizes our sample selection process. We begin with all securities-related class action lawsuits filed in federal court from 1996 to 2016. Next, we require the cases to have alleged fraud involving the price of common stock or failure to disclose (i.e., 10b-5 violations). We require sample firms be covered by CRSP, Compustat, and Audit Analytics. Our final treatment sample consists of 1,554 class-action securities lawsuits and 12,693 firm-quarter observations after gathering data for the pre-damage, damage, and post-suit periods.[22] Our control sample consists of 49,717 firm-quarter observations, more than our treatment sample because we match up to five control firms to each treatment firm.

4.Results

4.1. Descriptive Statistics

Panel A of Table 2 presents the distribution of lawsuits over time based on the year the suit was filed. The lawsuits are widely dispersed across the sample period, with several lawsuits occurring in each year of our sample period. The year with the fewest lawsuits is 2006 (33 suits)

[22] The number of quarterly observations varies by lawsuit because the damage period varies by lawsuit.

and the year with the most is 2001 (150 suits). Table 2 also provides summary statistics for the main variables used in our analysis (*NI*, *Ret*, *Neg*, *Post*, *Damage*). Panel B provides summary statistics for our treatment firms and matched control firms. Panel C provides summary statistics for our sample of auditor-based, non-sued peer firms and their matched control firms.

4.2. Lawsuit Filings and Subsequent Changes in Financial Reporting Conservatism

Table 3 presents results for tests of our first hypothesis regarding whether shareholder litigation is associated with increased accounting conservatism. Columns 1 and 2 present results for a simplified specification where the pre-damage and damage periods are pooled and we drop *Damage* and its interactions. To facilitate presentation of the results from estimating equation (1), we present the results in two columns. In column 1, we present estimates from estimating equation (1) on just the treatment (i.e., sued) firms, dropping *Treat* and its interactions from the model. In column (2), we show estimates from estimating equation (1) for just the control firms (*Treat* and its interactions drop out because *Treat* is zero for the control firms).[23] Our coefficient of interest is on *Ret* × *Neg* × *Post*, which, in both columns 1 and 2, captures the difference in accounting conservatism between the post-litigation period and the combination of the pre-damage and damage periods. Columns 3 and 4 separately identify the pre-damage and damage periods. In columns 3 and 4, the coefficient on *Ret* × *Neg* × *Post* compares conservatism in the post-litigation period to conservatism in just the pre-damage period.

Consistent with shareholder litigation resulting in greater conditional conservatism, we find that the coefficient on *Ret* × *Neg* × *Post* is significantly positive, at the 0.05 level or better, for treatment firms in both columns 1 and 3, suggesting that post-litigation reporting conservatism is

[23] Because the model is fully interacted (including interactions between *Treat* and the fixed effects), estimates of the coefficient on *Ret* × *Neg* × *Post* using just the treated firms are identical to the sum of the coefficients on *Ret* × *Neg* × *Post* and *Treat* × *Ret* × *Neg* × *Post* when we estimate the full version of equation (1).

greater than the level of conservatism in the pooled pre-damage and damage periods (column 1) and is greater than the level of conservatism in just the pre-damage period (column 3). Columns 2 and 4 show that the coefficient on *Ret* × *Neg* × *Post* is not significantly different from zero for the matched control firms (i.e., b_7 in equation (1)) in either specification, suggesting no change in conservatism for matched firms around the lawsuit.

The bottom panel of Table 3 shows results for tests comparing the difference between the *Ret* × *Neg* × *Post* coefficients for treatment firms versus the control firms. We assess statistical significance by estimating the full model in equation (1) and obtaining the statistical significance on the four-way interaction *Treat* × *Ret* × *Neg* × *Post* (i.e., d_7 in equation (1), the difference-in-differences estimator). For both specifications, the difference-in-differences coefficient is statistically significant, at the 10 percent level (two-sided) when we combine the pre-damage and damage periods (columns 1 and 2) and at the five percent level when we separately identify the damage period (columns 3 and 4). These results provide evidence that sued firms increase their conservatism following lawsuits significantly more than non-sued control firms that have similar *ex ante* litigation risk.

Our ability to identify the effect of litigation on firms' conservatism by examining changes in AT between sued and non-sued firms around lawsuits relies on the parallel trends assumption. We perform additional analyses to assess the validity of this assumption. Specifically, we examine the differences in AT between treated and control firms in the four quarters leading up to the lawsuit. We plot these differences in Figure 2. Visual inspection does not suggest there is any trend in the difference in AT between the two groups of firms in the quarters preceding the lawsuit. Further, the difference in AT between the treated and control firms is not statistically significant

in any of the four quarters prior to the lawsuit. This analysis supports the appropriateness of our control firms despite some differences in characteristics noted in Table 2, Panel B.

In summary, the results in Table 3 suggest that firms increase their level of accounting conservatism in response to 10b-5 shareholder lawsuits.

4.3. Lawsuits of Firms with the Same Auditor and Subsequent Changes in Conservatism

To test our second hypothesis (effect of litigation on non-sued firms) we seek firms that are likely to alter their financial reporting in response to another firm being sued. Prior research provides reasons to suspect that litigation against a firm will affect financial reporting decisions by clients of the same auditor or audit office as the sued firm. Prior studies provide evidence suggesting that firms' financial reporting is affected by the specific audit office performing the audit (e.g., Reynolds and Francis, 2001). Francis and Michas (2013) find that when an audit office has one client with misreporting, its other clients are more likely to have future restatements, suggesting that there are auditor office characteristics that affect the financial reporting choices of all client firms of that office. Further, evidence suggests that audit firms are sensitive to litigation and that litigation against auditors can affect their clients' financial reporting quality (Lennox and Li, 2014).[24] Together, these studies suggest that, when a firm is sued, the sued firm's auditor may influence the financial reporting of that auditor's other clients.

To assess whether same-audit-office clients of sued firms increase their conservatism, we identify auditor-based peer firms using the auditor and audit-office of each of our sued firms via Audit Analytics. We then select firms that are audited by the same audit office as the sued firms,

[24] Lennox and Li (2014) find that when auditors are sued their clients exhibit fewer misstatements. Their study differs from ours in the following ways. We examine disclosure related litigation against the reporting firms, while they examine litigation against audit firms. We examine whether companies become more conservative within-GAAP. Lennox and Li (2014) study whether the reporting companies of sued audit firms are less likely to violate GAAP.

resulting in a sample of 8,716 firm-quarters for audit-based peers. We then identify matched firms for the auditor-based peer firms using the same procedure as for our sued firms. To identify pseudo-event dates for the peer firms, we use the lawsuit and damage period dates from the corresponding sued firm.

Table 4 presents results. Columns 1 and 3 present results for the auditor-based peer firms. Columns 2 and 4 present results for the matched control firms for the auditor-based peer firms. We find that the coefficient on *Ret* × *Neg* × *Post* is positive and statistically different from zero (one percent level) for both specifications (columns 1 and 3), suggesting that conditional conservatism increases for firms that are audited by the same auditor as the sued firms after the litigation of the sued firm. Columns 2 and 4 show that the coefficient on *Ret* × *Neg* × *Post* is much smaller in magnitude than in columns 1 and 3 and is not significantly different from zero. These results indicate that we fail to find that the matched control firms change their conservatism after the sued firm's litigation. The bottom of Table 4 shows results for tests comparing the *Ret* × *Neg* × *Post* coefficients for the audit-based peer firms versus the *Ret* × *Neg* × *Post* coefficients for their matched control firms. For both specifications, the *Ret* × *Neg* × *Post* coefficient is significantly greater (5% and 1%) for the audit-based peer firms.

In summary, the results in Table 4 suggest that some non-sued firms increase their accounting conservatism in response to litigation of firms that are clients of the same audit office and provide support for hypothesis 2. These results provide further insights into the role litigation plays in the existence of accounting conservatism. Further, because these firms were not sued, the documented change in conservatism cannot be explained by confounds related to the events surrounding the lawsuit that may affect the AT measure. As such, they provide strong evidence that litigation events lead to the existence of more conservatism.

4.4. Post-litigation Conservatism Changes following FASB (2010)

Watts (2003) argues that the main explanations for conservatism are contracting, shareholder litigation, taxation, and accounting regulation. We next explore the role of one of these explanations, accounting regulation, in the relation between litigation and conservatism. In recent years, there is evidence that accounting regulation is likely to be less of an explanation for accounting conservatism because standard setters have taken the view that accounting conservatism compromises reporting quality and eliminated it as a desirable qualitative characteristic of financial accounting (FASB, 2010; DeFond et al., 2016). The FASB's view is consistent with the large volume of recent studies documenting an association between accounting conservatism and less informative financial reports (Givoly and Hayn, 2002; Dichev and Tang, 2008; Chen et al., 2014; Barth et al., 2014). Therefore, from a valuation perspective, an increase in conservatism may not necessarily be perceived as a desirable change of conduct following shareholder lawsuits filed in recent periods. In this regard, firms' response of increased conservatism when facing litigation may decrease in more recent years when standard setters no longer view accounting conservatism as a desirable characteristic.[25] However, shareholder lawsuits could act as a governance mechanism inducing conditional conservatism, offsetting standard setters' weakened emphasis on conservatism. If private litigation substitutes for public regulation in demanding accounting conservatism, lawsuits might continue to trigger conservatism.

In Table 5, we present results from our examination of the effect of litigation on reporting conservatism in the period subsequent to the FASB removing conservatism as a desirable

[25] The effect of the FASB's removal of conservatism as a desirable characteristic in 2010 on conservatism in financial statements could have started before or continued after 2010 because changes in standards and changes in managers' reporting behavior may have started before or continued after 2010 as new standards are passed and enforced and managers' reporting decisions change.

qualitative characteristic of financial accounting (FASB, 2010). Specifically, we repeat the analysis performed in Table 3 but limit the sample period to the years following 2010. Columns 1 and 3 (2 and 4) present results for the treatment (control) firms.

The coefficient on *Ret* × *Neg* × *Post* for treatment firms is not significantly different from zero in column 1 but is in column 3. Thus, in the specification where we isolate the pre-damage period as the pre-litigation period, we find evidence that conditional conservatism increased for the sued firms post-litigation. Columns 2 and 4 present results for the matched control firms. The coefficient on *Ret* × *Neg* × *Post* is insignificantly different from zero in both columns. The *Ret* × *Neg* × *Post* coefficient for the treatment firms (column 3) is significantly greater than for the control firms (column 4). Thus, we find some evidence that even in a period when conservatism is not viewed an important characteristic by standard setters, it arises as a result of managers responding to private litigation from shareholders. However, we note that support for that inference is limited to comparing the post-litigation period to the pre-damage period, only and not the combination of the pre-damage and damage periods. As we note in section 3.1, we think that comparing the post-litigation period to the pre-damage period provides the cleaner identification of the two approaches.

4.6. Additional Analyses

4.6.1. Placebo Test

One strength of our design is that sample firms' litigation events are scattered across calendar time. Therefore, it is unlikely that our results are driven by concurrent events because there would have to be multiple concurrent events occurring at various dates. Our use of a matched-firm control sample makes this even less likely. However, to provide further confidence that the effect we identify is driven by litigation events, we perform a placebo test where we assign random pseudo-litigation dates to each of the sued firms. Specifically, we randomly "shift" key dates

associated with each litigation event between 18 and 36 months backward or forward. We choose the pseudo-event dates such that, for an individual firm, both the pseudo-pre period and the pseudo-post period are entirely either pre-actual event or post-actual event.[26] We then repeat our main analysis using the pseudo dates. As these dates are randomly assigned and do not correspond to actual litigation events, we expect that we should observe no significant increase in conditional conservatism following the pseudo litigation dates.

Table 6 presents results. We find that the coefficient on *Ret* × *Neg* is significantly positive at a 1% level in all specifications. This is consistent with firms exhibiting conservatism throughout the sample period. However, the coefficient on *Ret* × *Neg* × *Post* is insignificant in all specifications. This indicates that sued firms do not increase their conservatism following the randomly assigned event dates. This analysis provides further evidence that the main results of increased post-litigation conservatism we document are driven by the litigation events rather than some other unidentified factor.

4.6.2. Restatements and Overlapping Litigation Events

One potential competing hypothesis is that our results are driven by restatements. Some of the sued firms in our sample also had restatements related to the claims in the lawsuit and prior research documents that firms that restate earnings become more conservative (Ettredge et al., 2012). Although it may be accurate to attribute changes in conservatism to litigation even with the concurrence of a restatement because the litigation may have triggered the restatement, we assess whether the effect we document is independent of that caused by the occurrence of a restatement.

[26] Across firms, we choose pseudo-event dates randomly. Therefore, some firms have pseudo-pre and pseudo-post periods that are entirely before their actual litigation date and some firms have pseudo-pre and pseudo-post periods that are entirely after their actual litigation date. However, there is no overlap of the actual litigation event period and the pseudo-event period for any firm to ensure these results are not affected by the litigation.

To do this, we repeat our main analyses from Table 3 after removing observations related to sued firms that also had corresponding restatements.

Additionally, a feature of our setting that may add noise to our analysis is that some firms are sued more than once within the time frames we observe. These lawsuits are related to separate litigation events rather than multiple allegations within a single lawsuit, which could result in the post-litigation period for one observation being the pre-litigation period for another observation. To rule out this possibility, we repeat the analyses from Table 3 excluding observations related to repeated lawsuits against the same firm that have overlapping time windows.

Table 7 reports results from these analyses. Panel A excludes lawsuits that involved restatements. Panel B excludes firms with overlapping lawsuits. Panel C excludes both lawsuits involving restatements and firms with overlapping lawsuits. In each panel, results for treatment firms are in columns 1 and 3 and results for control firms are in columns 2 and 4. Results across these three panels generally support our main inferences. When we compare the post-litigation period to the combined damage and pre-damage period (column 1), the coefficient on *Ret* × *Neg* × *Post* is not significantly different from zero. However, when we compare the post-litigation period to just the pre-damage period (column 3), the coefficient on *Ret* × *Neg* × *Post* is positive and significant. The corresponding coefficient on *Ret* × *Neg* × *Post* for the control firms is not significantly different from zero in any column or panel. As reported at the bottom of each panel, the increase in conservatism for treatment firms is significantly greater than for peer firms for the specifications that compare the post-litigation period to the pre-damage period (columns 3 and 4). It is not surprising the results in columns 3 and 4 are stronger for these tests because, as we discuss in Section 3.1, the approach used in columns 3 and 4, provides the cleaner identification of the

change in conservatism around litigation. Overall, these results suggest the result we document is incremental to restatements and is not due to noise from overlapping lawsuits.

4.6.3. Persistence Tests

Next, we examine the persistence of litigation's effect by analyzing the length of time the increase in conditional conservatism persists after the litigation. We do this by expanding our sample to include fiscal quarters from up to two years prior to the litigation event through five years following the litigation event. We augment our model with a series of indicators to capture when the fiscal year is relative to the litigation event and interact these variables with the rest of the terms in the model. For instance, the first four quarters following the litigation are denoted year 1, the next four quarters year 2, and so forth. We present these results graphically in Figure 3. The *y*-axis in Figure 3 measures the total amount of asymmetric timeliness as captured by the regression coefficients from our augmented regression, and the *x*-axis represents the year in which the conservatism is measured, where $T = 0$ is the year of the litigation event with respect to suit filing dates. The top line (orange) is the sued firms, and the bottom line (blue) is the control firms.

Figure 3 helps to inform our understanding of post-litigation changes in conservatism and to further validate our inferences that the change in conservatism we identify is driven by firms responding to litigation. We find that the level of conservatism increases in year T, the year of the litigation event, and more for the sued firms than for the control firms. Conservatism declines slightly in year $T + 1$ and again slightly in $T + 2$. It declines more in $T + 4$ and, at that point, is back down to (or below) the pre-litigation level.

Overall, Figure 3 suggests the effect of litigation on increasing firms' conditional conservatism persists for up to three years following the litigation. This effect is similar in length to that documented by Tan (2013), who finds firms increase their conservatism for two years

following a debt covenant violation. Notably, that the level of conservatism first increases in year *T*, the year of the litigation event, and not before provides further evidence that the main result we document is driven by the litigation event. Also, Figure 3 provides additional visual evidence that the parallel trends assumption holds in our setting as the difference in AT does not increase prior to the lawsuit. Finally, that the relation we document persists for more than one year provides evidence the main result we document is not explained by investors' initial reaction to the litigation or the firm recording an expected loss as a result of the litigation. Rather, it is consistent with our inference that firms respond to litigation by becoming more conservative with their accounting.

4.6.4. Litigation Severity

In this section, we investigate whether litigation severity is associated with stronger post-litigation conservatism. On one hand, more severe litigation outcomes could provide a stronger motivation for managers to become more conservative, similar to arguments that more severe criminal and civil penalties deter crime (Becker, 1968). On the other hand, prior research provides little guidance as to whether the relation between financial reporting outcomes and litigation severity is linear over the range of litigation severity that we can observe or that there is a relation between litigation severity and financial reporting outcomes. In other words, over the range of litigation outcomes we can observe, the relation between litigation severity and reporting conservatism may be the flatter portion of a concave function.

Following prior studies, we limit our sample period to the post-PSLRA time-period. Congress intended the PSLRA to make it more difficult to initiate securities litigation, particularly frivolous security lawsuits. Thus, the post-PSLRA period likely contains a higher concentration of more severe suits. Following prior litigation studies (e.g., Rogers and Van Buskirk, 2009), we measure the treatment period beginning with the filing of the suit. Thus, an assumption we make

is that managers can assess, at the time of filing, the severity of the suit. To the extent managers err in making this assessment, our analysis suffers from measurement error that biases our tests toward the null of no relation between suit severity and reporting conservatism.

We use five proxies for litigation severity: (1) whether the lawsuit was dismissed (or dismissed early), (2) settlement amount, (3) whether the CEO was fired, (4) whether the suit named the auditor, and (5) the overall loss in investor wealth (i.e., class period stock returns). None of the five proxies produce a statistically significant relation between litigation severity and change in financial reporting conservatism from pre- to post-litigation (not tabulated). The results of our litigation severity analyses are consistent with (1) we can only observe the flatter portion of a concave relation between litigation severity and reporting conservatism, as we note earlier, and (2) our litigation severity proxies capturing severity with too much error to detect a relation.[27]

5. Conclusion

Although positive accounting theory asserts that litigation risk prompts accounting conservatism as a preemptive deterrent, surprisingly little is known about the correctional effect of litigation events on subsequent financial reporting practices. Using litigation-risk matched control firms, we conduct a difference-in-difference analysis of changes in financial reporting conditional conservatism for 1,554 disclosure-related shareholder lawsuits, that are staggered throughout time. We also include firm and quarter fixed effects and conduct a placebo analysis for sued firms. These design features increase our confidence that the changes in financial reporting conditional conservatism we identify are attributable to the litigation events rather than other time-

[27] Our failure to find a relation between litigation severity and post-litigation changes in conservatism is perhaps not surprising in the sense that other studies either fail to find meaningful inferences based on lawsuit outcome variables (e.g., Donelson et al., 2012) or do not perform such tests (e.g., Rogers and Van Buskirk, 2009). Donelson et al. (2012, pg. 1272) note that it is not surprising that most variables in their model predicting lawsuit dismissals are insignificant, as prior studies find that few variables help predict suit outcomes (Pritchard and Sale, 2005; Johnson et al., 2007).

invariant firm characteristics or time-variant economy-wide events. Our analyses provide three main empirical findings regarding the effect of shareholder litigation on managers' financial reporting choices with respect to conservatism.

Our primary finding is that firms that are the targets of shareholder lawsuits respond with more conditionally conservative post-litigation accounting. This result is consistent with private litigation having a correctional effect on managers, serving as an effective governance mechanism that influences managers' accounting recognition choices. Second, we find evidence that non-sued firms (i.e., peer firms) audited by the same audit firm office as a sued firm significantly increase their conservatism following a peer firm's lawsuit filing event. This suggests a spillover effect from shareholder lawsuits. Third, we provide evidence suggesting that post-litigation increases in conditional conservatism persist past the reduced emphasis on conservatism by U.S. accounting standard setters. Although standard setters in the U.S. have recently dropped conservatism as an essential qualitative characteristic of the conceptual framework of financial reporting (FASB, 2010), our evidence suggests a private market demand for conditional conservatism through shareholder litigation channels, which counters the common belief that accounting conservatism has lost its relevance in contemporary financial reporting practices.

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Figure 1
Lawsuit timeline.

This figure illustrates the event dates in our sample. The original "Event Dates" are collected from the SCAS ISS lawsuit dataset and pertain to the actual event dates from the class action lawsuit.

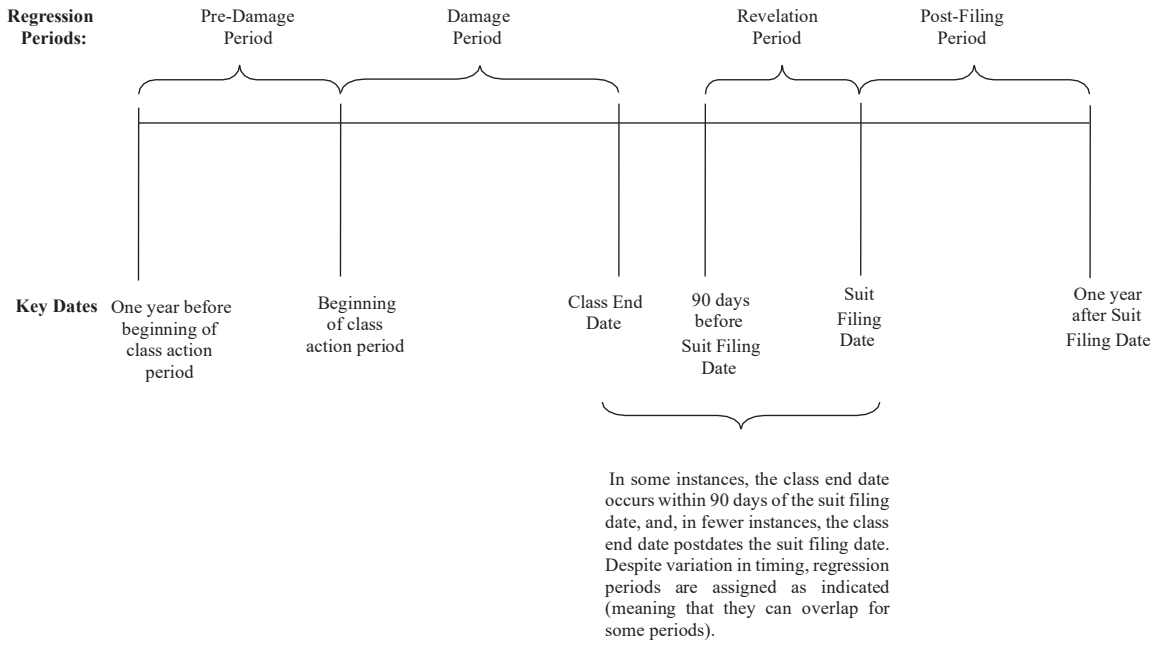

Figure 2
Parallel trends analysis.

This figure examines whether there are significant differences in asymmetric timeliness of earnings (AT) between the treatment and control firms in the four quarters leading up the treatment date. The figure presents the mean difference in the asymmetric timeliness between the treatment and control firms and the 95% confidence interval for the difference for each quarter. We do not observe a significant difference in asymmetric timeliness between the two groups of firms for any quarter. Further, there does not appear to be a trend during the four quarters leading up to the lawsuit. The results from this figure provide support that the assumption of parallel trends holds in our setting.

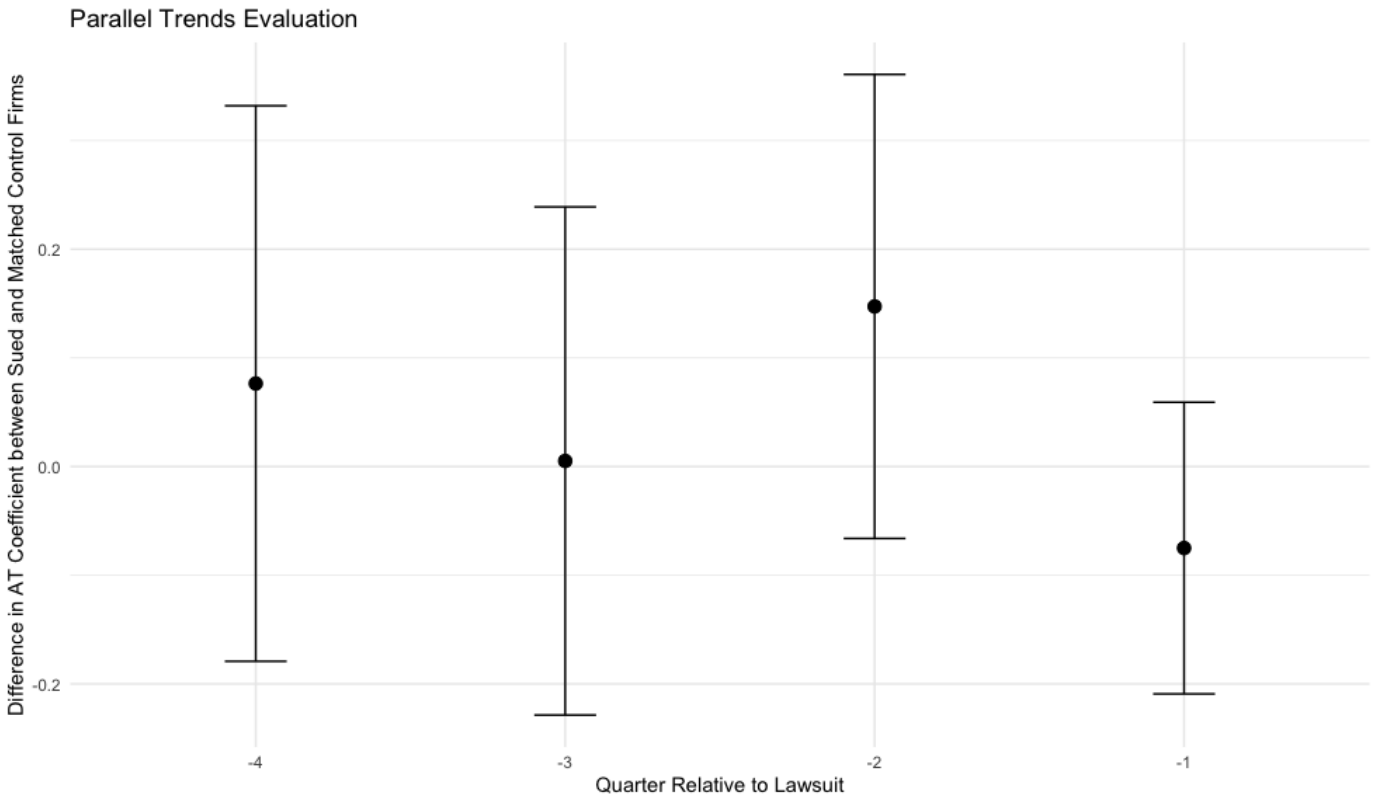

Figure 3
Persistence tests.

This figure examines firms' asymmetric timeliness of earnings around litigation in the years around litigation. This figure presents visual evidence of the persistence of the effect documented by our main analyses.

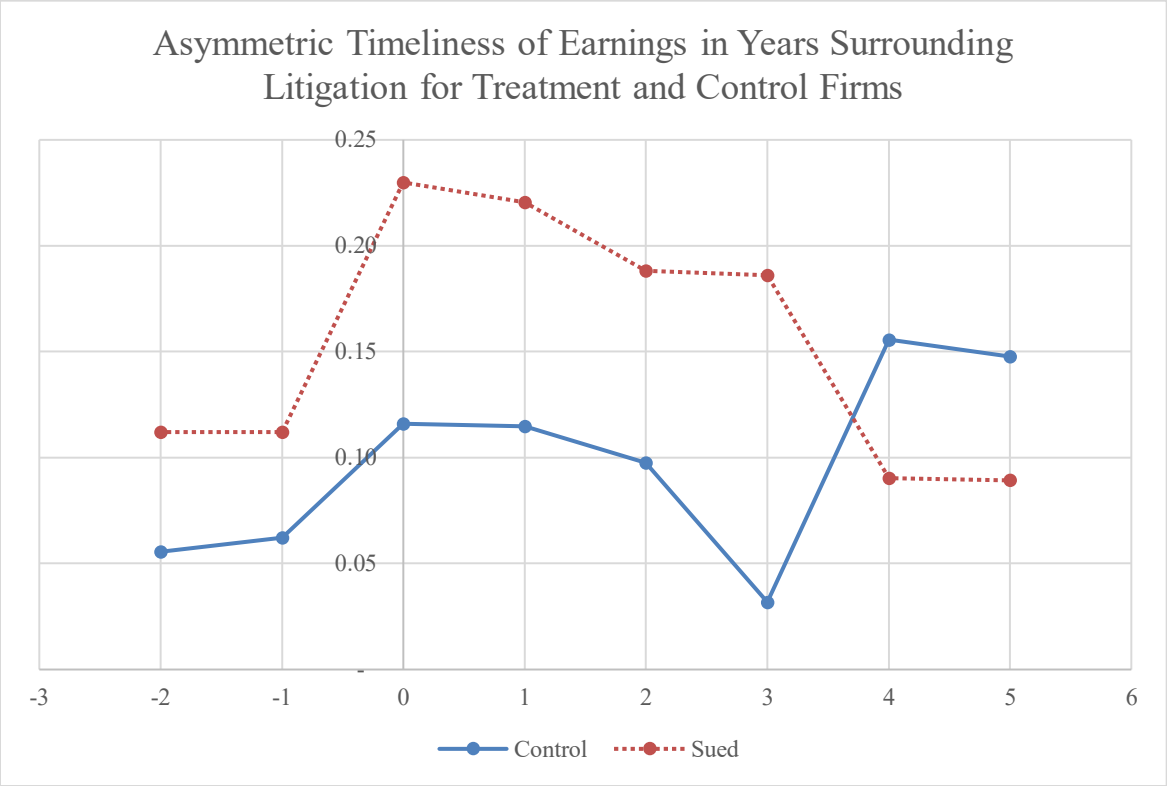

Table 1
Sample selection.

Criteria	
Unique federal court lawsuits in SCAS	5,722
Lawsuits missing beginning or ending class period date	(463)
Lawsuits missing ticker	(193)
Lawsuits not linked to CRSP and Compustat	(2513)
Lawsuits not 10b-5 related	(336)
Lawsuit filing date precedes end of class action period	(10)
Lawsuit missing key data	(328)
Lawsuit overlaps with a preceding suit	(36)
Lawsuit with no "post-suit" period data	(43)
Lawsuits filed before 1996	(232)
Lawsuits identifying more than 5 unique tickers	(14)
Total number of unique lawsuits used in tests	**1,554**

This table summarizes our sample selection process. The initial sample begins with U.S. shareholder lawsuits identified by the SCAS-ISS database filed in federal court. Primary tests begin with the sample of 1,554 unique disclosure-related class action securities lawsuits. Samples may be smaller in other tests due to other data requirements.

Table 2
Descriptive statistics.

Panel A: Lawsuit characteristics by year

Calendar Year	Number of Suits	Average Number of Days in Class Action Period	Percent Settled	Percent Dismissed[a]	Percent Active as of 4/30/2025[b]	Percent Citing GAAP Violation
1996	43	371.19	58%	42%	0%	58%
1997	81	339.63	79%	21%	0%	58%
1998	112	371.71	67%	33%	0%	54%
1999	105	315.17	55%	45%	0%	58%
2000	96	356.16	63%	37%	0%	64%
2001	150	343.91	77%	23%	0%	31%
2002	49	404.37	65%	35%	0%	59%
2003	74	403.82	51%	49%	0%	43%
2004	46	335.93	52%	48%	0%	52%
2005	44	206.66	45%	55%	0%	64%
2006	33	256.24	52%	48%	0%	52%
2007	63	349.30	45%	55%	0%	51%
2008	77	321.65	45%	55%	0%	44%
2009	64	903.22	43%	57%	0%	45%
2010	57	569.53	40%	56%	4%	35%
2011	57	464.23	39%	60%	1%	26%
2012	64	432.09	31%	67%	2%	9%
2013	92	506.84	30%	67%	3%	11%
2014	94	451.23	28%	68%	4%	6%
2015	106	436.88	27%	68%	5%	7%
2016	47	427.91	28%	64%	8%	4%
Total	1554	407.53	48%	42%	1%	38%

[a] Includes dismissals, partial dismissals, tentative dismissals, and dismissals on appeal

[b] Includes both active and "class certified" cases.

Panel B: Summary statistics for lawsuit sample and matched control sample

	Sued firms				Matched control firms				Differences	
Variables	N	Mean	Median	StdDev	N	Mean	Median	StdDev	Diff. in Means	t-stat.
NI	12,693	-0.011	0.019	0.204	49,717	-0.002	0.021	0.180	-0.009	-4.35
Ret	12,693	-0.002	-0.021	0.294	49,717	0.006	-0.013	0.256	-0.0089	-2.861
Neg	12,693	0.541	1	0.498	49,717	0.531	1	0.499	0.010	2.087
Post	12,693	0.322	0	0.467	49,717	0.304	0	0.460	0.018	3.899
Damage	12,693	0.380	0	0.485	49,717	0.3654	0	0.481	0.015	3.146

Panel C: Summary statistics for non-sued peer firms and matched control sample

	Non-sued peer firms				Matched control firms				Differences	
Variables	N	Mean	Median	StdDev	N	Mean	Median	StdDev	Diff. in Means	t-stat.
NI	8,716	-0.003	0.022	0.216	36,690	-0.001	0.020	0.185	-0.002	-1.555
Ret	8,716	0.007	-0.013	0.283	36,690	0.008	-0.011	0.253	-0.001	-1.020
Neg	8,716	0.527	1	0.499	36,690	0.527	1	0.499	0.000	0.402
Post	8,716	0.296	0	0.456	36,690	0.300	0	0.458	0.004	0.525
Damage	8,716	0.374	0	0.484	36,690	0.366	0	0.482	0.005	0.955

This table summarizes our descriptive statistics for lawsuits and firms included in the final sample. Panel A summarizes the time-series distribution of lawsuits by year. Panel B provides summary statistics for the sample of lawsuit firms. Panel C provides summary statistics for the sample of auditor-based peer firms. *NI* is income before extraordinary items (ibq), scaled by lagged book value of equity (ceqq). *Ret* is size and book-to-market adjusted cumulative monthly stock return for quarter *t*. *Neg* equals one if *Ret* is negative and zero otherwise. *Damage* equals one for quarters during the damage period and zero otherwise. *Post* equals one for quarters ending within 365 days following the lawsuit filing and zero otherwise.

Table 3

Difference-in-difference analysis of post-litigation changes in conditional conservatism of sued firms vs. matched control group.

	Dependent variable: NI			
	(1) Sued	(2) Matched	(3) Sued	(4) Matched
Ret	-0.002	-0.005	0.014	-0.015
	t = -0.117	t = 0.638	t = 0.769	t = -1.472
Neg	0.004	0.005**	0.001	0.004
	t = 0.706	t = 2.029	t = 0.148	t = 1.217
Post	-0.022**	0.0001	-0.024**	-0.001
	t = -2.568	t = 0.043	t = -2.342	t = -0.181
Damage			-0.001	-0.002
			t = -0.136	t = -0.538
Ret × *Neg*	0.123***	0.091***	0.037	0.086***
	t = 4.503	t = 5.719	t = 0.796	t = 4.437
Ret × *Post*	-0.029	-0.001	-0.043	0.008
	t = -0.964	t = -0.049	t = -1.356	t = 0.609
Ret × *Damage*			-0.033	0.021
			t = -0.970	t = 1.295
Neg × *Post*	0.012	-0.007	0.015	-0.007
	t = 1.140	t = -1.510	t = 1.200	t = -1.235
Neg × *Damage*			0.001	0.002
			t = 0.082	t = 0.374
Ret × *Neg* × *Post*	0.101**	0.002	0.179***	0.007
	t = 2.125	t = 0.090	t = 2.980	t = 0.225
Ret × *Neg* × *Damage*			0.116*	0.007
			t = 1.916	t = 0.250
Difference Tests (*Treat* × *Ret* × *Neg* × *Post*)				
	Estimated Difference (without damage period)		Estimated Difference (including damage period)	
Incremental Change for Sued Firms	0.099* (t = 1.810)		0.172** (t = 2.505)	
Observations	12,693	49,717	12,693	49,717
R^2	0.362	0.285	0.363	0.285
Adjusted R^2	0.275	0.223	0.276	0.223

This table presents results examining changes in the asymmetric timeliness of firms' financial reporting following lawsuit events. All models include firm and quarter fixed effects. Standard errors are clustered by firm and quarter. The superscripts ***, **, and * indicate significance at 0.01, 0.05, and 0.10 levels, respectively. *NI* is income before extraordinary items (ibq), scaled by lagged book value of equity (ceqq). *Ret* is size and book-to-market adjusted cumulative monthly stock return for quarter *t*. *Neg* equals one if *Ret* is negative and zero otherwise. *Damage* equals one for quarters during the damage period and zero otherwise. *Post* equals one for quarters ending within 365 days following the lawsuit filing and zero otherwise. *Treat* equals one for sued firms.

Table 4

Difference-in-differences analysis of post litigation conditional conservatism of non-sued peer firms with same auditor vs. matched control group.

	Dependent variable: NI			
	(1) Peer	(2) Matched	(3) Peer	(4) Matched
Ret	-0.010	0.005	0.016	-0.006
	t = -0.453	t = 0.462	t = 0.678	t = -0.561
Neg	-0.001	0.006**	-0.0004	0.005
	t = -0.136	t = 2.114	t = -0.034	t = 1.305
Post	-0.013	0.002	-0.016	-0.00001
	t = -1.303	t = 0.433	t = -1.298	t = -0.003
Damage			-0.002	-0.004
			t = -0.183	t = -0.914
Ret × Neg	0.110***	0.075***	0.032	0.075***
	t = 2.745	t = 3.635	t = 0.557	t = 3.428
Ret × Post	-0.062*	-0.021	-0.087**	-0.010
	t = -1.804	t = -1.287	t = -2.497	t = -0.638
Ret × Damage			-0.057	0.025
			t = -1.485	t = 1.305
Neg × Post	0.019	-0.009	0.019	-0.008
	t = 1.602	t = -1.442	t = 1.273	t = -1.146
Neg × Damage			-0.005	0.002
			t = -0.448	t = 0.453
Ret × Neg × Post	0.190***	0.032	0.262***	0.032
	t = 2.942	t = 0.935	t = 3.347	t = 0.848
Ret × Neg × Damage			0.116	-0.004
			t = 1.570	t = -0.104
Difference Tests (*Treat* × *Ret* × *Neg* × *Post*)				
	Estimated Difference (without damage period)		Estimated Difference (including damage period)	
Incremental Change for Peer Firms	0.158** (t = 2.127)		0.231*** (t = 2.675)	
Observations	8,716	36,690	8,716	36,690
R^2	0.342	0.277	0.343	0.277
Adjusted R^2	0.263	0.213	0.264	0.213

This table presents results examining changes in the asymmetric timeliness of firms' financial reporting following lawsuit events for non-sued client firms of the same audit office as the sued firms (i.e., peer firms). All models include firm and quarter fixed effects. Standard errors are clustered by firm and quarter. The superscripts ***, **, and * indicate significance at 0.01, 0.05, and 0.10 levels, respectively. *NI* is income before extraordinary items (ibq), scaled by lagged book value of equity (ceqq). *Ret* is size and book-to-market adjusted cumulative monthly stock return for quarter *t*. *Neg* equals one if *Ret* is negative and zero otherwise. *Damage* equals one for quarters during the damage period and zero otherwise. *Post* equals one for quarters ending within 365 days following the lawsuit filing and zero otherwise. *Treat* equals one for peer firms.

Table 5

Difference-in-difference analysis of post-litigation changes in conditional conservatism of sued firms vs. matched control group following FASB (2010).

	Dependent variable: NI			
	(1)	(2)	(3)	(4)
	Sued	Matched	Sued	Matched
Ret	-0.046**	0.012	-0.014	-0.039
	t = -2.319	t = 0.498	t = -0.243	t = -1.219
Neg	-0.013	0.009*	-0.037	0.006
	t = -0.964	t = 1.754	t = -1.652	t = 0.929
Post	-0.018	0.005	-0.030	-0.001
	t = -1.205	t = 0.773	t = -1.386	t = -0.122
Damage			-0.012	-0.011*
			t = -0.599	t = -1.739
Ret × Neg	0.115**	0.045	-0.113	0.080
	t = 2.395	t = 1.019	t = -0.810	t = 1.448
Ret × Post	-0.002	-0.036	-0.031	0.013
	t = -0.028	t = -1.154	t = -0.329	t = 0.338
Ret × Damage			-0.059	0.100**
			t = -0.597	t = 2.288
Neg × Post	0.029	-0.016	0.053*	-0.013
	t = 1.524	t = -1.643	t = 1.953	t = -1.268
Neg × Damage			0.033	0.006
			t = 1.510	t = 0.932
Ret × Neg × Post	0.155	0.045	0.361**	0.011
	t = 1.554	t = 0.677	t = 2.044	t = 0.141
Ret × Neg × Damage			0.300*	-0.073
			t = 1.729	t = -0.983
Difference Tests (*Treat* × *Ret* × *Neg* × *Post*)				
	Estimated Difference (without damage period)		Estimated Difference (including damage period)	
Incremental Change for Sued Firms	0.110 (t = 0.948)		0.350* (t = 1.934)	
Observations	4,050	16,756	4,050	16,756
R^2	0.386	0.298	0.388	0.299
Adjusted R^2	0.284	0.218	0.286	0.218

This table presents results examining changes in the asymmetric timeliness of firms' financial reporting following lawsuit events. The observations are limited to those occurring after the FASB removed conservatism as a desirable characteristic in 2010. All models include firm and quarter fixed effects. Standard errors are clustered by firm and quarter. The superscripts ***, **, and * indicate significance at 0.01, 0.05, and 0.10 levels, respectively. *NI* is income before extraordinary items (ibq), scaled by lagged book value of equity (ceqq). *Ret* is size and book-to-market adjusted cumulative monthly stock return for quarter *t*. *Neg* equals one if *Ret* is negative and zero otherwise. *Damage* equals one for quarters during the damage period and zero otherwise. *Post* equals one for quarters ending within 365 days following the lawsuit filing and zero otherwise. *Treat* equals one for sued firms.

Table 6

Placebo test of post-litigation changes in conditional conservatism of sued firms.

	Dependent variable: NI	
	(1)	(2)
Ret	-0.012	0.010
	t = -0.561	t = 0.273
Neg	0.004	0.010
	t = 0.835	t = 1.244
Post	0.010	0.014
	t = 1.184	t = 1.433
Damage		0.008
		t = 0.824
Ret × Neg	0.111***	0.101*
	t = 2.672	t = 1.882
Ret × Post	-0.040	-0.062
	t = -1.199	t = -1.377
Ret × Damage		-0.041
		t = -0.974
Neg × Post	0.004	-0.001
	t = 0.354	t = -0.128
Neg × Damage		-0.010
		t = -0.782
Ret × Neg × Post	0.005	0.016
	t = 0.074	t = 0.226
Ret × Neg × Damage		0.017
		t = 0.029
Observations	11,882	11,882
R^2	0.301	0.302
Adjusted R^2	0.223	0.223

This table presents results examining changes in the asymmetric timeliness of sued firms' financial reporting following randomly assigned placebo lawsuit events. All models include firm and quarter fixed effects. Standard errors are clustered by firm and quarter. The superscript ***, **, and * indicate significance at 0.01, 0.05, and 0.10 levels, respectively. *NI* is income before extraordinary items (ibq), scaled by lagged book value of equity (ceqq). *Ret* is size and book-to-market adjusted cumulative monthly stock return for quarter *t*. *Neg* equals one if *Ret* is negative and zero otherwise. *Damage* equals one for quarters during the damage period and zero otherwise. *Post* equals one for quarters ending within 365 days following the lawsuit filing and zero otherwise.

Table 7

Difference-in-difference analysis of post-litigation changes in conditional conservatism of sued firms vs. matched control group (excluding restatements and overlapping lawsuits).

Panel A: Excluding restatements

	Dependent variable: NI			
	(1) Sued	(2) Matched	(3) Sued	(4) Matched
Ret	0.000	-0.008	0.008	-0.017
	t = 0.003	t = -0.849	t = 0.375	t = -1.505
Neg	0.007	0.004*	0.002	0.005
	t = 1.031	t = 1.665	t = 0.164	t = 1.190
Post	-0.029***	-0.001	-0.033***	-0.0005
	t = -2.986	t = -0.236	t = -2.863	t = -0.144
Damage			-0.007	0.0002
			t = -0.649	t = 0.075
Ret × Neg	0.132***	0.092***	0.046	0.090***
	t = 4.304	t = 5.705	t = 0.784	t = 4.568
Ret × Post	-0.019	0.004	-0.026	0.013
	t = -0.591	t = 0.325	t = -0.747	t = 0.866
Ret × Damage			-0.016	0.022
			t = -0.410	t = 1.245
Neg × Post	0.012	-0.006	0.017	-0.006
	t = 1.031	t = -1.191	t = 1.215	t = -1.109
Neg × Damage			0.004	-0.0001
			t = 0.367	t = -0.015
Ret × Neg × Post	0.074	-0.009	0.153**	-0.007
	t = 1.404	t = -0.308	t = 2.160	t = -0.226
Ret × Neg × Damage			0.107	0.001
			t = 1.447	t = 0.024
Difference Tests (*Treat* × *Ret* × *Neg* × *Post*)				
	Estimated Difference (without damage period)		Estimated Difference (including damage period)	
Incremental Change for Sued Firms	0.082 (t=1.398)		0.160** (t=2.039)	
Observations	10,296	44,059	10,296	44,059
R^2	0.387	0.308	0.388	0.308
Adjusted R^2	0.287	0.241	0.288	0.241

Panel B: Excluding overlapping lawsuits

	Dependent variable: NI			
	(1) Sued	(2) Matched	(3) Sued	(4) Matched
Ret	-0.003	-0.005	0.015	-0.014
	t = -0.168	t = -0.622	t = 0.838	t = -1.390
Neg	0.004	0.005**	0.004	0.005
	t = 0.627	t = 2.045	t = 0.463	t = 1.369
Post	-0.022**	0.001	-0.023**	0.000
	t = -2.588	t = 0.185	t = -2.292	t = 0.028
Damage			-0.000	-0.001
			t = -0.001	t = -0.336
Ret × Neg	0.124***	0.091***	0.064	0.086***
	t = 4.573	t = 5.635	t = 1.256	t = 4.291
Ret × Post	-0.029	-0.002	-0.047	0.007
	t = -0.982	t = -0.134	t = -1.519	t = 0.513
Ret × Damage			-0.039	0.021
			t = -1.161	t = 1.188
Neg × Post	0.010	-0.008	0.010	-0.008
	t = 0.984	t = -1.612	t = 0.814	t = -1.393
Neg × Damage			-0.004	0.001
			t = -0.421	t = 0.154
Ret × Neg × Post	0.081	0.003	0.142**	0.008
	t = 1.628	t = 0.099	t = 2.116	t = 0.249
Ret × Neg × Damage			0.086	0.008
			t = 1.436	t = 0.268
Difference Tests (*Treat* × *Ret* × *Neg* × *Post*)				
	Estimated Difference (without damage period)		Estimated Difference (including damage period)	
Incremental Change for Sued Firms	0.079 (t=1.379)		0.134* (t=1.796)	
Observations	12,552	49,157	12,552	49,157
R^2	0.362	0.285	0.363	0.286
Adjusted R^2	0.275	0.223	0.275	0.223

Panel C: Excluding restatements and overlapping lawsuits

	Dependent variable: NI			
	(1) Sued	(2) Matched	(3) Sued	(4) Matched
Ret	-0.001	-0.007	0.013	-0.017
	t = -0.033	t = -0.826	t = 0.600	t = -1.444
Neg	0.006	0.004*	0.005	0.005
	t = 0.967	t = 1.711	t = 0.472	t = 1.390
Post	-0.028***	-0.0005	-0.031***	0.0001
	t = -2.906	t = -0.138	t = -2.680	t = 0.040
Damage			-0.004	0.001
			t = -0.383	t = 0.226
Ret × Neg	0.134***	0.092***	0.069	0.092***
	t = 4.358	t = 5.652	t = 1.149	t = 4.492
Ret × Post	-0.025	0.004	-0.038	0.013
	t = -0.790	t = 0.270	t = -1.093	t = 0.804
Ret × Damage			-0.031	0.022
			t = -0.769	t = 1.169
Neg × Post	0.010	-0.007	0.012	-0.008
	t = 0.903	t = -1.312	t = 0.878	t = -1.303
Neg × Damage			-0.001	-0.001
			t = -0.100	t = -0.278
Ret × Neg × Post	0.063	-0.011	0.128*	-0.011
	t = 1.154	t = -0.382	t = 1.680	t = -0.329
Ret × Neg × Damage			0.088	-0.004
			t = 1.206	t = -0.112
Difference Tests (*Treat* × *Ret* × *Neg* × *Post*)				
	Estimated Difference (without damage period)		Estimated Difference (including damage period)	
Incremental Change for Sued Firms	0.074 (t = 1.230)		0.139* (t = 1.682)	
Observations	10,191	43,560	10,191	43,560
R^2	0.388	0.308	0.389	0.308
Adjusted R^2	0.288	0.240	0.288	0.241

This table presents results examining changes in the asymmetric timeliness of firms' financial reporting following lawsuit events. Panel A excludes lawsuits that involved restatements. Panel B excludes firms with overlapping lawsuits. Panel C excludes both lawsuits involving restatements and firms with overlapping lawsuits. All models include firm and quarter fixed effects. Standard errors are clustered by firm and quarter. The superscript ***, **, and * indicate significance at 0.01, 0.05, and 0.10 levels, respectively. *NI* is income before extraordinary items (ibq), scaled by lagged book value of equity (ceqq). *Ret* is size and book-to-market adjusted cumulative monthly stock return for quarter *t*. *Neg* equals one if *Ret* is negative and zero otherwise. *Damage* equals one for quarters during the damage period and zero otherwise. *Post* equals one for quarters ending within 365 days following the lawsuit filing and zero otherwise. *Treat* equals one for sued firms.