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Accounting choices and the legal environment: The impact of the ex post loss rule

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Accounting Choices and the Legal Environment: the Impact of the *Ex Post* Loss Rule☆

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Abstract

Using a landmark Supreme Court decision as a natural experiment, I examine the impact of a fundamental requirement in securities litigation, the *ex post* loss rule, on income-decreasing accounting choices. *Dura Pharmaceuticals v. Broudo* (2005) established that plaintiffs must show that the alleged misrepresentations caused an actual economic loss. The case resolved a circuit split, allowing me to identify a treatment jurisdiction affected by *Dura*, and control jurisdictions in which the rule was already the prevailing legal standard. Motivated by legal analyses suggesting that *Dura* incentivizes firms to delay negative corrections, I hypothesize and find that treatment firms in high-litigation industries became more likely to delay write-downs and income-decreasing accrual error reversals at the firm level after *Dura*, relative to matched control firms. This paper sheds light on the relationship between securities law and accounting practices, and informs policy makers on the accounting impact of a key feature of the legal environment.

Keywords: Write-downs; Accruals; Supreme Court; Securities litigation

JEL: K22, M41

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1 Introduction

> *"As one might expect, securities lawyers regularly counsel their clients that, if they must disclose a piece of bad news, they should wait if necessary so as to be able to release good news at the same time. Finally, when firms have bad news to report that can be delayed no longer, they may as well report as much bad news as possible.* Dura*'s elevation of the* ex post *loss rule may exacerbate such trends."*
>
> (Spindler, 2007, p. 684-685)

> *"the US Supreme Court will hear what legal experts say is the most important securities case in a decade."*
>
> (Waldmeir, 2005, *The Financial Times*)

Accounting choices and disclosures play important roles in securities litigation: about a third of securities class action lawsuits between 1996 and 2017 allege financial statement misrepresentations, and almost a quarter allege GAAP or GAAS violations.[1] In recent years, for example, Harley-Davidson, HP, IBM, Xerox, and Yahoo! were sued following accounting changes related to write-downs or working capital.[2]

In a securities class action lawsuit, the plaintiff investors must demonstrate *loss causation* by showing that the firm's alleged misconduct caused a corrective disclosure that resulted in a stock price decline. This is known as the *ex post* loss rule (see Spindler, 2007). This rule links securities litigation inextricably with accounting because downward corrections are fundamental to the accounting process. For example, accounting standards such as ASC 320 and ASC 350 generally require an impairment or further impairment tests when an asset's carrying value exceeds its fair value, and income-increasing working capital accrual estimation errors should reverse downwards within a year at least at the transaction level (Allen et al., 2013).

However, legal analysis suggests that the *ex post* loss rule increases incentives to withhold or delay bad news. Spindler (2007) concludes that the rule incentivizes firms to "obscure or delay negative information" (p. 656–657) because a turnaround in fortunes, news about

[1] Based on lawsuits recorded in Stanford Law School's Securities Class Action Clearinghouse database. The database recorded over 4,700 lawsuits filed between 1996 and 2017.

[2] See *In re Harley Davidson*, 660 F. Supp. 2d 969 (E.D. Wis. 2009); *In re HP Securities Litigation*, 2013 U.S. Dist. LEXIS 168292 (N.D. Cal. 2013); *Int'l Ass'n of Heat & Frost Insulators & Asbestos Workers Local #6 Pension Fund, et al. v. IBM, et al.*, 2016 U.S. Dist. LEXIS 120641 (S.D.N.Y. 2016); *Russell Carlson, et al. v. Xerox, et al.*, 392 F. Supp. 2d 267 (D. Conn. 2005); *Oklahoma Firefighters Pension and Retirement System, et al. v. Xerox Corporation, et al.*, 2017 U.S. Dist. LEXIS 28445 (S.D.N.Y. 2017); *In re Yahoo!*, 2012 U.S. Dist. LEXIS 113036 (N.D. Cal. 2012).

other projects, or exogenous events can disrupt the causal link between alleged misconduct and a price decline, or prevent a price decline altogether:

> "A firm may choose to undertake multiple projects (e.g., conglomerate), and can then lie about one of the projects in the hope that the other project will ultimately make up for it. Similarly, exogenous events, such as market fluctuations, can interrupt the chain of causation and deny plaintiffs a recovery. Or a firm may fraudulently withhold news of bad performance in the hopes of turning it around in the future, preventing an *ex post* market decline." (Spindler, 2007, p. 657)

I argue in this study that the analysis in Spindler (2007) applies to the choice of recognizing required income-decreasing write-downs and accrual reversals, both of which can trigger litigation but are subject to discretion at the firm level. Write-downs are subject to managerial judgment for example in the estimation of asset fair values, and the literature (see Allen et al., 2013) suggests that working capital accrual errors may not reverse within one year at the firm level despite generally needing to do so at the transaction level. The primary prediction of this study is therefore that the rule causes firms at high risk of litigation to delay write-downs and income-decreasing working capital accrual error reversals at the firm level.

To identify the causal impact of the *ex post* loss rule on these accounting practices, I exploit *Dura Pharmaceuticals v. Broudo*, 544 U.S. 336 (2005), henceforth *Dura*, a landmark Supreme Court case that established the rule in one legal jurisdiction, with little or no expected impact on most other jurisdictions. Before *Dura*, the Ninth Circuit did not require plaintiffs in securities lawsuits to show that the firm's stock price declined. Instead, it allowed plaintiffs to plead that the firm's misconduct caused stock prices to be artificially inflated at the time they purchased the shares. In contrast, the majority of the circuit courts required plaintifs to plead that the firm's misconduct caused a price decline. In *Dura*, the Supreme Court overturned the Ninth Circuit, establishing that a causal link between the misconduct and a price decline is required in order to demonstrate loss causation. *Dura* was described by the *Financial Times* as "the most important securities case in a decade" (Waldmeir, 2005); I provide a summary of *Dura* and the lower court cases leading to it at Appendix A.

I use difference-in-difference designs to examine whether firms in the Ninth Circuit became more likely to delay required write-downs and income-decreasing accrual error reversals after *Dura*, relative to firms in control circuits. For the post-*Dura* period relative to the pre-*Dura* period, and for treatment relative to control firms, I examine the extent to which a high book-to-market predicts a write-down the following year, and the extent to which a highly positive working capital accrual error predicts a highly negative working capital accrual error the following year. I find that firms in high-litigation industries in the Ninth Circuit became more likely to delay required write-downs and income-decreasing working capital accrual error reversals at the firm level after *Dura* relative to matched control firms. In addition, when write-downs do occur, they are larger in treatment firms after *Dura* relative to control firms, consistent with Spindler (2007, p. 684-685), who suggested that *Dura* may increase delays of bad news until they "can be delayed no longer".

In addition to sensitivity analyses, I carry out two falsification tests. First, I replicate my analyses using an alternative Supreme Court decision, *Tellabs, Inc. v. Makor Issues & Rights, Ltd.*, 551 U.S. 308 (2007), henceforth *Tellabs*, that also affected litigation in the Ninth Circuit, but that concerned a different legal issue to *Dura*. Across specifications, I find no evidence that *Tellabs* affected the delaying of accounting corrections in a direction that would falsify my interpretation of my main results. Second, I replicate my analyses using firms not in high-litigation industries, and find no evidence that *Dura* had an effect on this sample. This is consistent with firms having less incentive to take potentially costly actions to avoid litigation if they have a low probability of facing litigation in the first place.

By exploiting *Dura*'s resolution of a circuit split to examine the causal impact of the *ex post* loss rule on accounting choices, my study contributes to the growing literature on the impact of court rulings on financial reporting, and more generally to the literature examining the relationship between the legal environment and accounting decisions (please see Section 2 for a literature review). My study also contributes to the literature on litigation risk and financial reporting. In particular, it builds on Watts' (2003) influential *litigation explanation* by providing evidence that in the current legal environment, managers attempt to reduce the litigation costs of overstatements by disrupting the causal relationship between the overstatement and a subsequent *ex post* loss. Finally, this paper informs academics, policy makers, and practitioners by shedding light on how a feature of the legal environment affects accounting practices that are fundamental to GAAP.

2 Prior literature

In this study I contribute to two overlapping streams of research. The first is the growing literature on the impact of court rulings on financial reporting, a rich arena for testing fundamental questions at the intersection of accounting and law. The second is the literature examining the relationship between litigation and financial reporting, particularly the stream of research relying on the *litigation explanation* spelled out by Watts (2003)—the idea that managers understate net assets because "expected litigation costs of overstatement are higher than those of understatement" (p. 216).

2.1 Accounting and changes in the legal environment

Both these streams of accounting research—the literatures examining court rulings and litigation respectively—examine the relationship between the legal environment and financial reporting. A firm's legal environment is the all-encompassing "sea of law" through which it navigates, as described by Edelman & Suchman (1997):

> "Modern organizations are immersed in a sea of law. They are born through the legal act of incorporation, and they die through the legal act of bankruptcy. In between, they raise capital under securities law, hire employees under labor and antidiscrimination law, exchange goods and services under contract law, develop public identities under trademark law, innovate under patent and copyright law, and engage in production under environmental, and health and safety law." (p. 480)

In particular, I have in mind what Edelman & Suchman (1997) call the *facilitative* facet of the legal environment, in which the law "appears as a system of procedural rules" (p. 483) that are exogenous to the firm.

Taking this definition of the legal environment to its broadest extent, the accounting literature has long examined the relationship between financial reporting and the legal environment. The many streams of research examining accounting policies and methods are of fundamental importance to the profession (e.g. Sunder, 1973; Biddle, 1980; Barth et al., 1996; Beatty & Weber, 2006; Barth et al., 2012), as are the closely-related studies examining other regulatory changes, such as those introduced by the Private Securities Litigation Reform Act (e.g. Johnson et al., 2001), the Sarbanes-Oxley Act (e.g. Zhang, 2007; Cohen et al., 2008) and the Dodd-Frank Act (e.g. Christensen et al., 2017).

The recent literature has also begun examining the impact of the microfoundations of the legal environment, including individuals' legal expertise (e.g. Krishnan et al., 2011), general counsel (e.g. Hopkins et al., 2015), external counsel (e.g. Bozanic et al., 2016), and law firms (e.g. Dechow & Tan, 2018). Krishnan et al. (2011) and Hopkins et al. (2015) respectively find that financial reporting quality increases with audit committee legal expertise and decreases with general counsel compensation. In particular, Hopkins et al. (2015) find evidence that firms with highly-paid general counsel are significantly more likely to meet analysts' forecasts due to a change in the litigation reserve. Bozanic et al. (2016) find evidence that firms involve external legal counsel in the SEC comment letter process to "resist disclosing new information" (p. 4). Furthermore, in a recent study Shen & Tan (2018) find evidence that resistance to the comment letter process varies between individual lawyers, and that measures of resistance increase with the retention of external counsel formerly employed by the SEC.

Finally, Dechow & Tan (2018) find that a company is more likely to backdate its stock option grants if it is represented by a law firm that had backdating clients, suggesting that the practice of stock option backdating spread via law firms.

A growing stream of research has also examined the impact of court rulings on accounting decisions. Court rulings can have profound and sweeping effects on the legal environment. Decisions by the United States courts of appeals (that is, the *circuit courts*) can establish legal precedent across multiple states at once, while Supreme Court decisions in turn often resolve circuit splits, establishing precedent across one or more circuit courts' jurisdictions at once. Shapiro (2006) writes that the Supreme Court's "primary mechanism for maintaining uniformity is to resolve circuit splits—areas of law in which different federal courts of appeals (and state supreme courts) disagree about what rule or standard governs. In resolving these circuit splits, the Court often announces rules and standards to be applied by the lower courts" (p. 272–273, internal quotation marks omitted).

Earlier accounting studies examining court rulings include Simon (1956), Little et al. (1995), and Dhaliwal & Erickson (1998), and in recent years a growing number of studies have exploited court rulings to answer questions at the intersection of accounting and law. Bliss et al. (2018), for example, examine the impact of *Dura* on litigation outcomes from bundling restatement news with other news, and their findings provide support for the arguments by Spindler (2007) that motivate my study.[3] Hopkins (2017) and Cazier et al. (2017) use *In re Silicon Graphics*, 183 F.3d 970 (9th Cir. 1999) as an exogenous shock to examine the impact of changes in litigation risk on earnings management and non-GAAP reporting respectively. Finally, Chy & Hope (2018) exploit court decisions that changed auditor legal liability to examine the impact of auditor conservatism on myopic underinvestment in R&D.

2.2 Accounting and securities litigation

Litigation is "perhaps the most frequently studied aspect of the facilitative legal environment" (Edelman & Suchman, 1997, p. 485), and a large body of research examines the relationship between securities litigation and financial reporting. I identify three major subsets of this literature—studies that examine guidance, accounting quality, and conservatism, respectively.

In one of the earliest empirical studies of the relationship between accounting and securities litigation, Kellogg (1984) found in a sample of accounting-related class action lawsuits that stock returns were negative around and before the time of public discovery of the firms'

[3] See also Furchtgott & Partnoy (2015), a prior version of Bliss et al. (2018), that also examines the impact of *Dura* on the propensity to bundle restatement news.

misrepresentations. Kellogg (1984) therefore provided early evidence that bad news triggers litigation, and the goal of much of the subsequent literature on the relationship between accounting and litigation risk has been to examine the frequency, timing, and other characteristics of disclosures that potentially trigger litigation.

2.2.1 Guidance

A large stream of research on accounting and litigation examines earnings guidance as a determinant of shareholder litigation, supported by evidence on the high cost of negative earnings news.

Skinner (1994), in particular, finds that highly negative earnings announcements are more likely to be preempted by guidance than other earnings announcements, consistent with managers using guidance to reduce litigation costs when earnings news is negative.[4] However, early empirical studies found mixed evidence for a negative relationship between guidance and litigation. Francis et al. (1994), for example, find in a sample of 45 shareholder lawsuits triggered by negative earnings news that the defendant firms issued guidance in 28 of the cases, suggesting that guidance does not fully deter litigation. Francis et al. (1994) also find that 46 of their sample of 53 firms at risk of litigation due to severe earnings and sales declines did not provide guidance. Furthermore, Skinner (1997) finds that the timeliness of earnings disclosure is positively related to litigation. More recent studies find support for a negative relation between guidance and litigation (e.g. Field et al., 2005; Donelson et al., 2012; Billings & Cedergren, 2015). Field et al. (2005), in particular, find that guidance is negatively related to lawsuits when they use a simultaneous equations framework to control for endogeneity, and after excluding dismissed lawsuits.

Several recent studies have also examined the consequences of litigation or litigation risk on guidance (e.g. Rogers & Van Buskirk, 2009; Houston et al., 2010; Billings et al., 2016). Rogers & Van Buskirk (2009), for example, find evidence that the frequencies of earnings-related conference calls and earnings guidance decrease after firms face securities lawsuits. In addition, the literature has also examined the relationship between litigation and disclosure tone: Rogers et al. (2011) find a positive relation between earnings announcement optimism and litigation, and find evidence that plaintiffs *target* optimistic language, and Cazier et al. (2016) find that tone is only associated with litigation risk for non-forward-looking statements, consistent with safe harbor protection for forward-looking statements.

[4]Another interpretation Skinner (1994) offered is that "managers may incur reputational costs if they fail to disclose bad news in a timely manner." (p. 39)

2.2.2 *Accounting quality*

Next, a line of research has examined the relationship between accounting quality and litigation risk; for example, DuCharme et al. (2004), Gong et al. (2008), Grimm (2009), and Chalmers et al. (2012) find evidence suggesting that litigation risk increases with abnormal accruals or decreases with accruals quality.

DuCharme et al. (2004) find that abnormal accruals in the year prior to a secondary equity offering are higher in firms that are eventually sued in relation to their offerings, relative to firms that are not. Gong et al. (2008) find, in the context of merger announcements, that the market does not fully price the variation in litigation risk attributable to abnormal accruals and other observables. And more recently, Chalmers et al. (2012) find that firms facing securities lawsuits related to financial reporting have significantly lower accruals quality in the four quarters before the sued quarter, relative to matching observations.

2.2.3 *Conservatism*

Finally, a line of research examines the relationship between litigation and conservatism. Basu (1997, see p. 26–30) provided initial evidence by showing that conditional conservatism is greater in periods of high auditor legal liability, and Watts (2003) subsequently spelled out a *litigation explanation for conservatism*:

> "Since the expected litigation costs of overstatement are higher than those of understatement, management and auditors have incentives to report conservative values for earnings and net assets." (p. 216)

Motivated in part by Watts (2003), the subsequent literature has found that conservatism is increasing in litigation risk at the firm level (e.g. Qiang, 2007; Khan & Watts, 2009), and that firms with greater conditional conservatism have more favorable litigation outcomes (Ettredge et al., 2016). Furthermore, in a well-identified cross-country study, Huijgen & Lubberink (2005) find that conservatism is greater in the jurisdiction with greater exposure to legal liability, consistent with the litigation explanation for conservatism.

Conceptually, my paper builds on Watts' (2003) *litigation explanation*. I explain that in the current legal environment, it is the *ex post* loss when an overstatement is corrected that gives rise to the litigation costs borne by defendants, and not the overstatement *per se* (see Section 3.1). I next present arguments due to Spindler (2007) that the costs of an overstatement may not be fully internalized if managers can distort the causal relationship between the overstatement and a price decline (Section 3.2). My empirical tests then examine whether there is evidence that firms use accounting choices to distort the causal relationship between overstatements and *ex post* losses.

3 Legal background

3.1 The circuit split in the interpretation of loss causation

In securities class action lawsuits, plaintiffs are required to demonstrate *loss causation*—that is, that the defendant's violation caused the plaintiff's loss. The requirement was codified in the Private Securities Litigation Reform Act of 1995, henceforth PSLRA:

> "the plaintiff shall have the burden of proving that the act or omission of the defendant alleged to violate this chapter caused the loss for which the plaintiff seeks to recover damages." (15 U.S.C. §78u-4(b)(4))[5]

Despite the PSLRA's codification of the requirement of loss causation, until the *Dura* decision in 2005, the United States Courts of Appeals (that is, the *circuit courts*) were divided in their interpretations of the requirement. Please see Appendix B for a map showing the jurisdictions of each of the circuit courts.

The differing interpretations of loss causation can be divided into a *majority view*—the interpretation held by the majority of circuits—and a *minority view* (see Escoffery 2000 and Prezioso et al. 2004). The majority view held that loss causation was established only if plaintiffs could demonstrate a causal link between the defendant's misconduct and actual economic losses to the plaintiff. In contrast, the minority view held that loss causation was established if plaintiffs could demonstrate that the fraud caused the defendant's stock price to be inflated at the time of purchase. Spindler (2007) refers to the contrasting interpretations as the *ex post* and *ex ante* loss rules respectively, similar to Ferrell & Saha (2007):

> "Has not such an investor suffered a loss, in an economic sense, from the fraudulent statement? The answer turns on whether one looks at the situation ex post or ex ante" (p. 172).

By 2003, the minority view was held almost exclusively by the Ninth Circuit. In *Broudo v. Dura Pharmaceuticals*, 339 F.3d 933 (9th Cir. 2003), henceforth *Broudo*, the Ninth Circuit affirmed that plaintiffs do not need to show a price decline in order to establish loss causation. Instead, showing that the firm's misconduct inflated stock prices at the time of purchase is sufficient:

[5] For detailed discussions on the legal context and rationale of, and the debate surrounding the PSLRA see, for example, Avery (1996), Cox (1997), Fisch (1997), Walker & Seymour (1998), Zelensky (1998), and Ramirez (1999). See also Black (1987), Merritt (1988), Kaufman (1991), and Escoffery (2000) for discussions on the loss causation requirement and its development prior to the PSLRA.

> "Ninth Circuit cases hold that: in a fraud-on-the-market case, the plaintiffs establish loss causation if they have shown that the price on the date of purchase was inflated because of the misrepresentation." (p. 934)[6]

The Ninth Circuit contrasted its view with the majority view, for example saying that "other circuits [...] do require demonstration of a corrective disclosure followed by a stock price drop to be alleged in the complaint." (*Broudo*, footnote 4) Please see Appendix A for a summary of *Broudo*, including background information on the case.

The defendants in *Broudo* appealed to the Supreme Court, who heard the case in *Dura*. I also provide a summary of *Dura* at Appendix A. The Supreme Court argued in *Dura* that the Ninth Circuit's perspective was unique, and that "other Courts of Appeals have rejected the Ninth Circuit's inflated purchase price approach to proving causation and loss" (p. 344, internal quotation marks omitted). In its decision, the Supreme Court reversed the Ninth Circuit's decision in *Broudo*. The Supreme Court held that it is insufficient for a plaintiff only to prove that the defendant's security price was inflated at the time of purchase, arguing that the plaintiff "has suffered no loss" at the time of the transaction (p. 342), and that "the Ninth Circuit's holding lacks support in precedent" (p. 343). In addition, the Supreme Court expressed agreement with the majority view, for example citing the plaintiffs' "failure to claim that Dura's share price fell significantly after the truth became known" (p. 347).

The Supreme Court therefore resolved the circuit split, establishing definitively that plaintiffs in securities class action lawsuits need to allege that the defendant's violation caused an actual economic loss to the plaintiffs. *Dura* was described as "the most important securities case in a decade" (Waldmeir, 2005). The courts began applying the *ex post* loss rule immediately, interpreting the rule to mean that a stock price decline is required: "cases [since *Dura*] appear to be almost universally in line with the interpretation that Dura requires a market decline" (Spindler, 2007, p. 671). Table 1 summarizes the legal environments pre- and post-*Dura* by legal jurisdiction.

A counterargument, however, is that the Supreme Court's decision in *Dura* could have little impact since plaintiffs can allege a causal link between misconduct and price decline even if it is not required. I test this argument empirically by examining whether litigation declined in the Ninth Circuit after *Dura*: the extent to which litigation declined is an estimate

[6] Prezioso et al. (2004) point out that the Ninth Circuit had several decisions "that are consistent with the majority view" (p. 9), but they note that the decisions were prior to *Broudo*. In the *Broudo* ruling, the Ninth Circuit cited one of its prior decisions, the Second Circuit's decision in *Suez Equity*, and the Eighth Circuit's decision in *Gebhardt v. ConAgra Foods*, 335 F.3d 824 (8th Cir. 2003) in support of its position on loss causation. However, the Second Circuit clarified in 2003 (in *Emergent*) that *Suez Equity* was not in support of the minority opinion, and an amicus brief for *Dura* characterized the Eighth Circuit's position as "less than clear" (Prezioso et al., 2004, p. 9).

Table 1: Loss causation standards by jurisdiction before and after *Dura*

<table>
<tr><th>Jurisdiction</th><th>Pre-Dura</th><th></th><th>Post-Dura</th></tr>
<tr><td>Ninth Circuit</td><td>Sufficient to show price inflation on purchase</td><td>→</td><td>Must show causal link to actual economic loss</td></tr>
<tr><td>Majority opinion</td><td colspan="3">Must show causal link to actual economic loss</td></tr>
</table>

Before *Dura*, the circuit courts were divided in their interpretations of the loss causation standard in securities class action lawsuits. The Ninth Circuit held that plaintiffs were only required to establish that the defendant's stock price was inflated, while the majority opinion was that plaintiffs must demonstrate a causal link between the misconduct and actual economic losses to the plaintiff. In *Dura*, the Supreme Court agreed with the majority opinion. Please see Appendix B for a map showing the jurisdiction of each circuit court.

of the likelihood that shareholders in the pre-*Dura* regime brought litigation without relying on a causal link from misconduct to a corrective disclosure. In Figure 1, I document that the proportion of firm-years implicated in securities litigation declined after *Dura* from 6.5 percent to 3.9 percent (chi-squared p-value < 0.01) in high-litigation industries in the Ninth Circuit, with no evidence for a statistically significant decline in control circuits or in other industries.

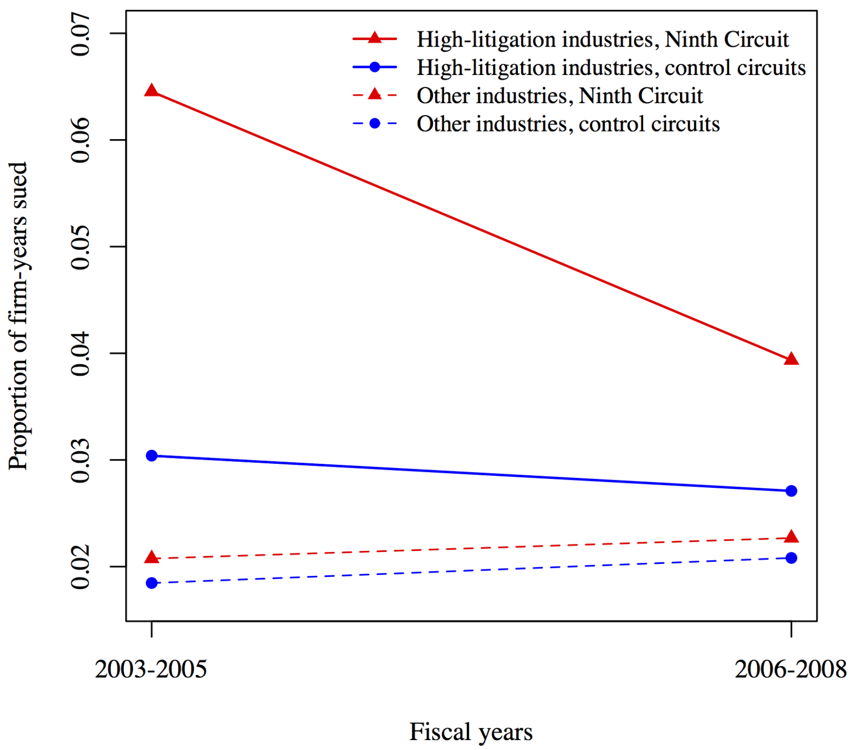
3.2 Impact of the *ex post* loss rule on negative corrections

Dura sparked a series of studies in the legal academia that examine the legal context, potential consequences, and criticisms of the decision (e.g. Fox, 2005; Dutton, 2006; Olazabal, 2006; Cross, 2007; Ferrell & Saha, 2007; Spindler, 2007). Spindler (2007), in particular, argues that the rule "fails to adequately internalize the costs of fraud onto the firm" (p. 657). Given an overstatement, a firm can bundle the negative correction with other news, or an exogenous event or turnaround in fortunes could occur. These can distort the causal relationship between the overstatement and subsequent price declines, or allow a firm to avoid a price decline altogether. Without an *ex post* loss that is attributable to alleged fraud, plaintiffs would not be able to successfully sue for damages.

Specifically, Spindler (2007) examines three settings—when a firm has multiple projects, when exogenous events occur, and when there are multiple periods—and analyses in each setting a hypothetical project with some probability of success (see p. 677–685). To summarize his arguments, focusing on the case in which an overvalued project has failed and the manager is contemplating a negative correction that could cause a stock price decline:

Multiple projects: The losses from the project's failure can be made up by gains from other projects, potentially avoiding *ex post* losses entirely. Even if multiple projects

Figure 1: Impact of *Dura* on litigation



This figure documents the proportions of firm-year observations that were implicated in securities class action lawsuits between fiscal years 2003 and 2005 inclusive, and 2006 and 2008 inclusive, for different subsamples. The observations comprise Compustat firms headquartered in the United States and with state data available. High-litigation industries and control circuits are as defined in the empirical analyses of this paper (see Section 5.1). A firm-year is implicated in a securities class action lawsuit if the firm appears in Stanford Law School's Securities Class Action Clearinghouse database, and either the first or reference class period overlaps with the fiscal year.

fail sufficiently for plaintiffs to suffer *ex post* losses, determining the amount of damages attributable specifically to the overstatement may be problematic.

Exogenous events: The correction can be bundled with a negative exogenous event, minimizing legal liability. Because a firm is not liable for *ex post* losses that investors would have suffered even in the absence of fraud, legal liability may be reduced if an exogenous event occurred that would have caused a stock price decline even if the project succeeded.

Multiple periods: If the project spans multiple periods, a negative correction in one period can be delayed in order to bundle it with future information about the project: "the marginal benefit of good news [in the second period] is quite high, because it reduces fraud liability, whereas the marginal cost of additional bad news is zero, since the loss causation rule limits fraud liability to the damages actually caused by the fraud." (p. 684)

Incentives to delay bad news over multiple periods are particularly important because firms often have projects and assets that span multiple reporting periods. In his discussion of the multiple-period setting, Spindler (2007, p. 684) in fact suggests examining whether the likelihood of delaying bad news varied by legal jurisdiction before the *Dura* decision unified the circuits:

> "It would be interesting to see whether evidence of delays is greater in jurisdictions utilizing *ex post* loss causation rules prior to *Dura*. In any event, *Dura* should exacerbate the tendencies to delay that already exist."

Furthermore, he suggests that when the bad news can no longer be delayed, firms "may as well report as much bad news as possible" (p. 685), consistent with big bath incentives.

To apply these arguments to accounting choices in broad terms (see Section 4 for a detailed discussion), the *ex post* loss rule incentivizes managers to avoid or delay income-decreasing reversals of inflated financials. For example, income-increasing working capital accrual errors should reverse at the transaction level, but the reversals can be bundled with other income-increasing working capital accruals at the firm level (e.g. Allen et al., 2013), obscuring the impact of the reversal of the positive error specifically. In addition, assets like goodwill span multiple fiscal periods, allowing managers to delay required write-downs until they can be bundled with other news or until an exogenous event or a turnaround occurs.

3.3 Supporting evidence and tension

The opinions of *Dura* and subsequent courts on loss causation, and some empirical evidence from the literature, generally support the argument that the causal link between alleged misconduct and a subsequent price decline can be distorted. In *Dura* itself the Supreme Court discussed the strength of the relationship between inflated share prices and subsequent price declines, noting that even if there is a price decline,

> "that lower price may reflect, not the earlier misrepresentation, but changed economic circumstances, changed investor expectations, new industry-specific or firm-specific facts, conditions, or other events, which taken separately or together account for some or all of that lower price." (p. 343)

Subsequent courts' opinions support the arguments in *Dura* and Spindler (2007). To cite several examples from dismissed lawsuits:

In *Leykin v. AT&T Corp.*, 423 F. Supp. 2d 229 (S.D.N.Y. 2006), the court dismissed the complaint in part because the plaintiffs did not show that the price decline was due to the alleged misconduct rather than an industry-wide price decline, illustrating the point that defendants are not liable for *ex post* losses that investors would have faced in the absence of fraud.

In *Lattanzio v. Deloitte*, 476 F.3d 147 (2nd Cir. 2007), the Second Circuit said that the plaintiffs would have had to allege facts that allow "some rough proportion" (p. 26) of the *ex post* loss to be attributed to the defendant's misstatements, as opposed to the misstatements by another party.

In *Wilamowsky v. Take-Two*, 818 F. Supp. 2d 744 (S.D.N.Y. 2011), the court said that in order for the plaintiff to rely on certain misstatements to show loss causation, he would have had "to disaggregate their impact on his loss from prior misstatements and legitimate news affecting Take-Two stock prices" (p. 34)

In *Kuriakose v. Freddie Mac*, 2011 U.S. Dist. LEXIS 34285 (S.D.N.Y. 2011), the court said that "there is a decreased probability that Plaintiffs' losses were caused by fraud" (p. 42) because the defendant's stock was "clearly linked" to marketwide conditions.

These cases support the idea that under the *ex post* loss rule, loss causation requires price declines to be disentangled from marketwide conditions and other news.[7]

[7] I note, as a caveat, that these cases were not dismissed on the basis of loss causation alone.

The argument in Spindler (2007) that bundling bad news with other information reduces legal liability is also supported empirically in the restatements setting. The findings in Bliss et al. (2018) suggest that bundling of restatement disclosures with other pieces of information became more effective at reducing post-restatement litigation cost after *Dura*. In addition, a prior version of Bliss et al. (2018) using a narrower definition of bundling, Furchtgott & Partnoy (2015), shows that firms became more likely to bundle restatement disclosures after *Dura* (see Table 6). Intriguingly, however, another recent study found some evidence to the contrary. Donelson & Hopkins (2016) show that the courts do not fully disentangle large market declines and the impact of firm-specific news: they find that litigation and settlements after earnings disclosures are more likely when the disclosures occurred during large short-window market declines. This suggests that bundling with other bad news may in fact *increase* litigation risk. Furthermore, additional tests in Donelson & Hopkins (2016) suggest that this phenomenon may be driven by judicial expertise: when a judge has more specialized experience in securities litigation, large market declines are more likely to lead to dismissals, and vice versa.

The results in Donelson & Hopkins (2016) highlight an area of tension in motivating this study: that the justice system is not perfect. In a study of district and appellate court civil cases over 1988 to 2000, Eisenberg (2004) finds that 21 percent of cases with definitive judgments are appealed, and that defendants achieve reversals about 40 percent of the time if the appeal is not settled or withdrawn. These figures indicate that mistakes (at least by district courts) are not uncommon. A manager making a costly accounting choice to avoid a lawsuit would therefore have to consider the possibility of being sued anyway, and of the suit not being dismissed despite his or her best efforts.

An additional layer of tension is that delaying bad news may itself increase litigation costs in the long run if a lawsuit is not successfully avoided or dismissed. An exogenous event or a turnaround of fortunes may not happen, for example. Delaying a correction increases litigation risk because it is likely to lengthen the time during which the firm's stock price was allegedly inflated by misconduct. This increases the number of statements that would be scrutinized for misrepresentations, and increases the number of putative class members. Field et al. (2005) writes, for example, that the "longer the stock trades at too high a price, the greater the potential damages from a lawsuit and the more likely the firm is to be sued" (p. 496, internal quotation marks omitted).

As a counterargument, however, Spindler (2007) suggests that it is commonplace for lawyers to recommend delaying bad news, suggesting that the benefits to the firm of delaying must usually outweigh the costs:

> "As one might expect, securities lawyers regularly counsel their clients that, if

they must disclose a piece of bad news, they should wait if necessary so as to be able to release good news at the same time." (p. 684)

Given the legal and empirical evidence for and against, whether managers exploit the *ex post* loss rule affirmed by *Dura*, and if so, to what extent, are empirical questions.

4 Hypothesis development

My overall research question is whether the *ex post* loss rule incentivizes firms at high risk of litigation to withhold or delay income-decreasing accounting choices. Specifically, I hypothesize that firms in highly-litigated industries in the legal jurisdiction most affected by *Dura*—the Ninth Circuit—became more likely to delay write-downs and downward accrual error reversals at the firm level after *Dura*, relative to control firms.

My hypotheses are motivated primarily by the arguments in Spindler (2007) suggesting that under an *ex post* loss regime, managers can reduce the costs of overstated financial results by distorting the causal relationship between the overstatement and subsequent price declines. As discussed in detail in Section 3.2, this distortion can be achieved by bundling downward corrections with other news, by the impact of exogenous events, or by withholding bad news in the hopes of a positive turnaround. In an *ex ante* loss regime, in contrast, these actions would be less effective at reducing litigation risk because plaintiffs are able to show loss causation on the basis of the alleged overstatement.

Dura would therefore have an impact on income-decreasing accounting choices that are closely linked to prior periods of observably inflated financials. In addition, *Dura* would have an impact on income-decreasing accounting choices that are expected to lead to litigation, but that are subject to managerial discretion.

4.1 Write-downs

GAAP rules require that "most non-financial assets must be written down when their fair values drop sufficiently below their carrying values" (Lawrence et al., 2013, p. 112). For example, accounting standards such as ASC 320 Investments–Debt and Equity Securities and ASC 350 Intangibles–Goodwill and Other generally require an impairment or further impairment tests when an asset's carrying value exceeds its fair value.

Write-downs are charged against earnings, and frequently contribute substantially to losses. Among loss firms over the past decade that recorded write-downs, the write-downs accounted for more than half of losses in about 40% of cases, and were *greater than total* *losses* in about 20% of cases.[8] Write-downs expose firms to securities litigation risk not only because they contribute to losses, but in particular because they reveal that financial results were inflated in the past. Companies that have been sued in recent years following write-downs include HP, IBM, Xerox, and Yahoo!.[9] In *In re HP Securities Litigation*, plaintiffs sued HP after the firm announced an $8.8 billion impairment due in part to accounting fraud inflating financial results.

Because a write-down is usually required when an asset's fair value drops below its carrying value, an asset book-to-market ratio above one—*market-implied impairment*—can be used as an indicator that a write-down is very likely to be required.[10] I note that market-implied impairment is not necessarily grounds for litigation *per se* even if it is accompanied by declining stock prices, and a book-to-market of less than one does not necessarily imply that a write-down is not required. In *In re HP Securities Litigation*, for example, HP's stock price declined substantially after the acquisition of Autonomy, driving HP's book-to-market close to one, but shareholders sued only after the write-down announcement.

Finally, write-downs have long been known to be subject to managerial discretion (e.g. Elliott & Hanna, 1996; Francis et al., 1996; Wilson, 1996; Ramanna & Watts, 2012; Lawrence et al., 2013). Elliott & Hanna (1996) write that

> "consistent with claims made in the business press, management might be using write-offs to accomplish strategic earnings management objectives." (p. 154)

Judgment is often required even within the constraints of GAAP, and even if strict fair value tests are required. For example, SFAS 142 (now codified as ASC 350) requires goodwill to be tested for impairment at least annually in two steps: first, potential impairment is determined by comparing the fair value of a reporting unit with its carrying value; second, the implied fair value of goodwill within the reporting unit is compared with its carrying value (see FASB, 2001). Ramanna & Watts (2012) argue that managerial discretion exists in

[8] These statistics do not include write-downs recorded in cost of goods sold. These descriptives are based on comparing the sum of non-goodwill pretax write-downs and impairments (*wdp*) and pretax goodwill impairments (*gdwlip*) against net income (*ni*), for all Compustat firm-years with strictly negative *ni* and strictly negative *wdp* or *gdwlip* between 2007 and 2016 inclusive. *wdp* and *gdwlip* are set to zero if missing in Compustat.

[9] See *In re HP Securities Litigation*, 2013 U.S. Dist. LEXIS 168292 (N.D. Cal. 2013), *Int'l Ass'n of Heat & Frost Insulators & Asbestos Workers Local #6 Pension Fund, et al. v. IBM, et al.*, 2016 U.S. Dist. LEXIS 120641 (S.D.N.Y. 2016), *Oklahoma Firefighters Pension and Retirement System, et al. v. Xerox Corporation, et al.*, 2017 U.S. Dist. LEXIS 28445 (S.D.N.Y. 2017), and *In re Yahoo!*, 2012 U.S. Dist. LEXIS 113036 (N.D. Cal. 2012).

[10] Although the threshold may effectively be slightly above one for certain assets for which the fair value test is less stringent. See Lawrence et al. (2013, p. 115) for a summary of accounting principles on write-downs by asset class.

the allocation of goodwill to reporting units, the estimation of the reporting units' fair values, and the measurement of the fair values of net assets within reporting units.[11] Furthermore, Graham et al. (2005, see Table 6) find that 21.3% of CFOs surveyed would postpone taking an accounting charge within the constraints of GAAP to meet an earnings target.

In summary, write-downs may trigger securities litigation, but are subject to managerial discretion. The arguments in Spindler (2007) that the *ex post* loss rule may incentivize managers to delay bad news therefore applies: by exercising discretion in delaying a required write-down, a manager can wait for a positive turnaround (thus avoiding the write-down entirely), or wait for additional value-relevant news that can be bundled with the write-down announcement. My first hypothesis, stated formally in alternate form, is therefore as follows:

Hypothesis 1. *Firms at high risk of litigation under the jurisdiction of the Ninth Circuit became more likely to delay required write-downs post-*Dura*, relative to matching control firms.*

In addition, assuming that not all firms are able to delay a write-down indefinitely, a delay is likely to lead to a larger write-down when it is eventually recorded. This would be consistent with firms being able to delay write-downs only up to a point, or firms reporting "as much bad news as possible" (Spindler, 2007, p. 685) by taking larger write-downs or by writing-down more assets, once they are unable to delay a write-down.

4.2 Downward accrual error reversals

More generally, write-downs are reversals of positive accruals. An inventory write-down, for example, is a correction of positive inventory accrual errors in the past, and goodwill impairments reverse previously-recognized goodwill as its estimated fair value decreases. My second set of tests focuses on downward reversals of working capital accrual estimation errors. This provides additional evidence on the impact of the *ex post* loss rule because income-decreasing changes in working capital can trigger litigation, but their effect would generally not be reflected in my tests of write-downs.[12]

As in the examples in Section 4.1 of lawsuits following write-downs, there are recent examples of class action lawsuits following downward corrections related to working capital.

[11] In addition, ASU 2011-08 has introduced additional room for discretion by allowing managers to bypass the two-step impairment test entirely depending on the outcome of a qualitative assessment.

[12] For example, inventory write-downs reverse inventory accruals (Allen et al., 2013), but would not be tested under Hypothesis 1 because they are usually reflected in costs of goods sold (Lawrence et al., 2013).

For example, Xerox was sued in 2000 after recording a $78 million charge in part for improper recognition of uncollectible receivables, among other issues, and Harley-Davidson was sued in 2005 for channel stuffing after it announced a reduction in production and shipment targets.[13]

Accrual estimation errors "arise from lack of perfect foresight or from the application of aggressive or conservative accounting mandated under generally accepted accounting principles" (p. 118), or from earnings management. (Allen et al., 2013) Examining estimation errors specifically in working capital accruals has the advantage that under the constraints of GAAP they generally reverse within one year at the transaction level. This follows from the definition of accrual estimation errors in Allen et al. (2013):

> "An accrual estimation error is an *ex post* characterization of an accrual based on the difference between the accrual and the subsequently realized benefit." (p. 115)

Since the benefits of positive working capital accruals are intended to be realized within one year, associated estimation errors should also reverse at the transaction level within a year. This allows me to use a highly positive error in the previous year as an indicator that a downward reversal is likely to be required in the current year.

While the estimation errors should reverse within one year at the transaction level, they may not do so at the firm level. Allen et al. (2013, see Table 3, Panel B), for example, find the surprising result that firm-level working capital accrual estimation errors—the component of working capital accruals not attributable to growth or temporary fluctuations in working capital requirements—are weakly positively autocorrelated. They suggest that the reversals may be offset by new estimation errors, or simply that estimation errors "often take longer than one year to reverse" (p. 115).

I therefore hypothesize that the arguments in Spindler (2007) apply to working capital accrual estimation errors; specifically, that the *ex post* loss rule incentivizes managers to bundle income-decreasing reversals with new income-increasing errors. This obscures the relationship between inflated financial results in the previous period and downward corrections in the current period, and potentially avoids a price decline, reducing the risk of litigation in an *ex post* loss regime. My second hypothesis, stated formally in alternate form, is therefore as follows:

[13] See *Russell Carlson, et al. v. Xerox, et al.*, 392 F. Supp. 2d 267 (D. Conn. 2005) and *In re Harley Davidson*, 660 F. Supp. 2d 969 (E.D. Wis. 2009).

Hypothesis 2. *Firms at high risk of litigation under the jurisdiction of the Ninth Circuit became more likely to delay required income-decreasing working capital accrual estimation errors at the firm level post-*Dura*, relative to matching control firms.*

I also examine the extent to which the impact of the *ex post* loss rule on the timeliness of income-decreasing accrual error reversals drives its impact on *overall* accrual error reversals (income-decreasing *or* income-increasing). This would provide evidence for a potential relationship between a key feature of the legal environment and overall accrual reversals, which has been a focus of much of the prior literature on, for example, earnings management and fraud detection (e.g. Dechow et al., 1995, 2011, 2012).

5 Research design

5.1 Treatment and control samples

I test Hypotheses 1 and 2 using regression models that exploit the fact that *Dura* reversed the legal precedent of the Ninth Circuit. As discussed in Section 3.1, the split between the Ninth Circuit and other circuits was evident by 2003, with the minority view on loss causation being held almost exclusively by the Ninth Circuit. In *Dura*, the Supreme Court ruled in favor of the majority view, reversing the Ninth Circuit and resolving the circuit split.

Consistent with prior literature (e.g. Cazier et al., 2017; Bliss et al., 2018), I assign firms to Circuit Court jurisdictions based on the state in which the firm is headquartered. Appendix B shows a map of states under each Circuit Court jurisdiction. The use of headquarters location is grounded in legal reasoning: Cazier et al. (2017), for example, write that a suit filed outside the district in which a firm is headquartered is "highly vulnerable" to dismissal or removal to the headquarters district because

> "Due to the nature of the specific types of claims in private securities class action litigation and the fraud on the market presumption [...], substantially all of the witnesses and evidence are likely to be located at the firms headquarters." (p. 9)

Nevertheless, because *Dura* resolved a circuit split, there would be a bias *against* rejecting my null hypotheses to the extent that firms headquartered in the Ninth Circuit are expected to be sued in control circuits, and vice versa, during the pre-*Dura* period. I examine this further in untabulated analyses using data from Stanford's Class Action Lawsuit Clearinghouse. I find that the substantial majority—about 91 percent— of lawsuits against firms located in treatment or control states were tried in treatment or control states respectively.[14]

I therefore use firms headquartered in states under the jurisdiction of the Ninth Circuit in the treatment sample. I begin my analysis in 2003, the year the Ninth Circuit stated its legal position on loss causation in *Broudo* and contrasted it with that of other circuits (as outlined in Section 3.1). I define the pre-treatment period as 2003 to 2005 and the post-treatment period as 2006 to 2008 in order to use the same number of years pre- and post-treatment.[15]

My control sample comprises firms headquartered in states under the jurisdictions of all circuits other than the Eighth and Ninth Circuit. I omit firms under the Eighth Circuit because there is disagreement over its legal position on the *ex post* loss rule. On the one hand the Ninth Circuit cited the Eighth Circuit in support of its interpretation (*Broudo v. Dura Pharmaceuticals*, 339 F.3d 933, 9th Cir. 2003), but on the other hand an amicus brief for *Dura* authored in part by the General Counsel of the Securities and Exchange Commission and the Solicitor General Counsel of Record of the Department of Justice stated that "the Eighth Circuit's position on this issue is less than clear" (Prezioso et al., 2004, p. 9).

I restrict the samples to firms in highly litigated industries, defined based on data on securities lawsuit filings by industry prior to *Dura*, and industry definitions from the prior literature (e.g. Francis et al., 1994; Kasznik & Lev, 1995; Kim & Skinner, 2012; Donelson & Hopkins, 2016). Specifically, the high-litigation industries are biotechnology (SIC codes 2833–2836 and 8731–8734); computer manufacturing and software (SIC 3570–3577 and 7370–7374); electronics manufacturing (SIC 3600–3674); and telecommunications and electric services (SIC 4810–4813, 4911, and 4931). Please see Appendix C for further discussion, including details on industry representation in securities lawsuit filings prior to *Dura*, and the probability of securities litigation by industry.

5.2 Testing Hypothesis 1

Hypothesis 1 concerns the timing of required asset write-downs; in other words I need to examine the changes in the likelihood of a write-down conditional on a write-down being

[14]Based on lawsuit filings in the three years pre-*Dura* with recent Compustat data available. The proportion is even higher (about 93%) for firms in control states, despite the reduced hurdles to plaintiffs in the treatment jurisdiction pre-*Dura*. For analyses in the legal literature on lawsuit filing location, see, for example, Cox et al. (2009) and Cain & Solomon (2015).

[15] *Dura* was decided in April 2005, which would have been in the second or fourth quarter of fiscal 2005 for most firms. If firms changed their accounting policies immediately, within fiscal 2005 itself, as a result of the decision, this would bias against rejecting my null hypotheses. In sensitivity analyses described in Sections 7.3 and 8.3, I replicate my main tests using two-year windows, 2003-2004 and 2006-2007, and find that my main inferences are unchanged.

required.

My strategy is to examine the extent to which an asset book-to-market greater than one predicts a write-down during the year, because GAAP rules generally require a write-down when an asset's carrying value exceeds its fair value. However, there is some noise in operationalizing this empirically because in the case of tangible assets, the threshold may not be strict or Compustat may not capture the write-down separately.[16] I mitigate this potential source of noise by focussing on firms with material intangible assets when testing Hypothesis 1. I also examine the impact of varying the threshold value in sensitvity analyses at Section 7.3.

A highly positive relationship between write-downs and a high book-to-market in a firm with material intangible assets indicates that the firm records GAAP-required write-downs in a timely manner.[17]

The regression model for Hypothesis 1 is as follows:

$$\text{logit}(wdd_t) = \alpha + \beta \times abtmd_{t-1} \times post_t \times treat_t + \gamma \times \Gamma_t + e_t \tag{1}$$

where wdd_t is a dummy variable indicating a write-down during t, $abtmd_{t-1}$ is a dummy variable indicating an asset book-to-market ratio greater than one at the start of t, $post_t$ is a dummy variable indicating the post-treatment period, $treat_t$ is a dummy variable indicating firms in the Ninth Circuit, and Γ_t is a vector of second-order interactions, main effects, and controls.

A significantly negative estimate of β would reject the null hypothesis, and I would interpret this as evidence evidence that firms in the treatment sample avoid required write-downs to a greater extent after *Dura*, relative to firms in the control sample.

Similarly, I examine the impact of *Dura* on the level of write-downs *when they occur* by examining the relationship between the level of write-downs and the level of firms' beginning asset book-to-market, for firms that recorded a write-down. This model estimates the amount of write-down a firm records, controlling for the extent to which its assets are overvalued relative to the market.

[16] See Lawrence et al. (2013, p. 115-118) for a detailed analysis of GAAP rules and write-downs. Lawrence et al. (2013) show that there is a nonlinearity in the relation between write-downs and book-to-market ratios around a book-to-market ratio of one. They also show that the relationship between write-downs and beginning book-to-market increases with intangibles intensity. See also Beaver & Ryan (2005) and Ramanna & Watts (2012).

[17] In untabulated analyses, I find a significantly negatively average stock price reaction to earnings announcements in write-down quarters in my sample even when the asset book-to-market is greater than one at the start of the year. This suggests that the market-implied impairment is incomplete on average, or that the market overreacts to impairment announcements.

The regression model is as follows:

$$wd_t = \alpha + \beta \times abtm_{t-1} \times post_t \times treat_t + \gamma \times \Theta_t + e_t \tag{2}$$

where wd_t is the scaled level of write-downs at t, $abtm_{t-1}$ is the asset book-to-market ratio at the start of t, and Θ_t is a vector of second-order interactions, main effects and controls.

Because write-downs are coded as negative values in Compustat, a significantly negative estimate of β would indicate that treatment firms record larger write-downs relative to beginning book-to-markets after *Dura*, relative to firms in the control sample.

5.3 Testing Hypothesis 2

To test Hypothesis 2, I examine the extent to which income-increasing accrual errors in the previous period predict income-decreasing accrual errors in the current period.

Similar to Allen et al. (2013), I decompose working capital accruals into components attributable to growth, temporary fluctuations in working capital requirements, and errors. Please see Appendix E for details on the construction of the accrual error variables. I then assign the signed error components for the current and previous years to quintiles, and examine the extent to which high-quintile accruals in the previous year predict accruals in the lowest quintile in the current year. I estimate two regression models for Hypothesis 2, as follows:

$$\begin{aligned} \text{logit}(I(q\_acc_t = 1)) = \alpha + \beta \times I(q\_acc_{t-1} \geq 4) \times post_t \times treat_t \\ + \gamma \times \Lambda_t + \epsilon_t \end{aligned} \tag{3}$$

$$\begin{aligned} \text{logit}(I(q\_acc_t = 1)) = \alpha + \beta \times I(q\_acc_{t-1} = 5) \times post_t \times treat_t \\ + \gamma \times \Lambda_t + \epsilon_t \end{aligned} \tag{4}$$

where q_acc_t is the quintile of the signed accrual error relative to the distribution at t, and Λ_t is a vector of second-order interactions, main effects, and controls.

The estimates of β then measure the impact of *Dura* on income-decreasing accrual reversals. Specifically, it captures the likelihood that a firm records highly negative accruals in the current year when accruals were highly positive in the previous year, relative to the likelihood when accruals was not highly positive. This is important because the underlying motivation of this study is that *Dura* incentivizes treatment firms specifically to avoid *corrective* disclosures—disclosures that correct firm performance downwards.

Next I examine the impact of *Dura* on accrual reversals in general using the following regression model:

$$q\_acc_t = \alpha + \beta \times q\_acc_{t-1} \times post_t \times treat_t + \gamma \times \Pi_t + \epsilon_t \tag{5}$$

where Π_t is a vector of second-order interactions, main effects, and controls. Significantly *positive* estimates of β would suggest that relative to control firms, year-on-year accrual reversals declined in treatment firms after *Dura*.

5.4 Matching methodology

I use two matching methods to control for differences between the treatment and control subsamples:

- *Industry matching*: The control group comprises all firm-years in the same high-litigation industries as the treatment group.
- *Propensity matching*: The treatment and control observations are also from the same industries, but they are propensity-matched each year to minimize the difference in estimated probability of treatment given a set of confounders.

The propensity matching is carried out in three steps. First, I use logistic regressions to estimate the propensity that an observation is in the treatment subsample given a set of potential confounding variables. Second, I discard treatment and control observations that are outside the support of the propensity score. Third, I match treatment and control observations using full matching (Rosenbaum, 1991).

For the write-downs analyses, I match along $abtm_{t-1}$, int_{t-1}, roa_t, log_age_t, log_at_{t-1}, log_mv_{t-1} and two-digit SIC fixed effects. Matching by intangibles intensity (int_{t-1}) and performance (roa_t) is particularly important since Lawrence et al. (2013) find that the relationship between write-downs and book-to-market is more negative when intangibles intensity is higher and performance is poorer; and matching by age (log_age_t) controls for the possibility that accounting conservatism changes with firm age (e.g. as suggested by Khan & Watts, 2009).

For the accruals reversals analyses, I match along acc_{t-1}, roa_t, $growth_{t-1}$, $ebtm_{t-1}$, log_age_t, log_mv_{t-1} and two-digit SIC fixed effects. Matching by performance (roa_t) is particularly important due to the relationship between performance and accruals (e.g. Dechow et al., 1995; Kothari et al., 2005), and matching by growth and age ($growth_{t-1}$, $ebtm_{t-1}$, and log_age_t) controls for the potential relationship between accruals persistence and growth (e.g. Fairfield et al. 2003; see also Richardson et al. 2006).[18]

I match treatment and control observations using the full matching procedure introduced by Rosenbaum (1991) (see also Rosenbaum, 2002; Hansen, 2004), which uses all available observations to generate an optimal match between treatment and control groups. The procedure "is optimal in terms of minimizing a weighted average of the estimated distance measure between each treated subject and each control subject within each subclass." (Ho et al., 2011, p. 7).[19]

Specifically, the procedure matches treatment and control observations within subclasses in which a treatment observation is matched to one or more control observations, or a control observation is matched to one or more treatment observations. A set of weights is produced that I use in the subsequent analyses. I show (in Table 4) that the differences in mean propensity of treatment each year becomes insignificant and close to zero after weighting, I report the impact of the propensity matching on covariate balance at Table 5, and I use weighted regressions when estimating the models under propensity matching.

6 Sample and descriptives

6.1 Sample selection

My sample period begins in 2003 and ends in 2008. As I discuss in Section 3.1, the split between the Ninth Circuit and other circuits was clear by 2003, when the Ninth Circuit stated its position in *Broudo* and contrasted it with the majority view. Beginning the sample in 2003 also allows me to avoid the impact of changes that may be attributable to the Sarbanes-Oxley Act or the adoption of SFAS 142 Goodwill and Other Intangible Assets. Using the post-SFAS 142 period is critical because prior to the adoption of the standard (after March 15, 2001 for early adopters and after December 15, 2001 for other firms; see FASB, 2001, p. 7), write-downs were rare because firms amortized intangible assets.[20]

Table 2 details the sample selection procedure of this study.[21] Beginning with unique Compustat firm-years between 2003 and 2008 inclusive, I require firms to be headquartered

[18]Please refer to Appendix D for variable definitions. I carry out the matching procedure after sample attrition and after winsorizing. I winsorize accrual estimation errors at ±50%, and other non-discrete variables are winsorized each year at the top and bottom percentiles.

[19]See also Hansen (2004, p. 609): "Among matching techniques for observational studies, full matching is in principle the best, in the sense that its alignment of comparable treated and control subjects is as good as that of any alternate method, and potentially much better." I implement the propensity matching using the MatchIt package (Ho et al., 2007, 2011) in R.

[20]In untabulated tests, I find that less than one percent of Compustat firm-years in the 1996 to 2000 period recorded a write-down (negative *wdp* or *gdwlip*), compared to over 17 percent in the 2001 to 2005 period.

[21]Please see Sections 9.1 and 9.2 for details on the samples used in the falsification tests.

in the United States and located in states in either the treatment or control jurisdictions. The treatment sample comprises firms in states under the jurisdiction of the Ninth Circuit, and the control sample comprises firms in states under the jurisdictions of circuits other than the Eighth or Ninth Circuit.[22] I restrict the sample to firms in high-litigation industries as defined at Section 5.1 (see also Appendix C). I also omit firms with beginning assets strictly less than $1 million because Hypothesis 1 examines asset write-downs, and to reduce the frequency of small denominators in the accrual measures used to test Hypothesis 2.

Panels B and C of Table 2 detail the further attrition steps specific to the write-downs analyses and accrual reversals analyses respectively. For the write-downs analyses I require firms to have material beginning intangibles, defined as beginning intangibles of at least 1% of beginning assets, because the fair value threshold for tangible asset write-downs may not be strict, or Compustat may not capture their write-downs separately (see Section 5.2 for details). For each set of tests I require availability of key variables used in the main and additional analyses. These steps result in a sample of 1,882 treatment firm-years and 3,499 control firm-years for the write-downs analyses and 2,383 treatment firm-years and 4,188 control firm-years for the accruals reversals analyses. These are the industry-matched samples, in which treatment and control firm-years are from the same set of industries.

I construct the propensity-matched samples by applying the matching methodology described in Section 5.4 to the industry-matched samples. Extreme treatment and control observations—i.e. observations outside the support of the propensity score—are dropped, resulting in a slight decline in the sample sizes for the propensity-matched sample. I apply full matching (Rosenbaum, 1991), which results in a set of weights that are used in the subsequent analyses. After propensity-matching, there are 1,854 treatment firm-years and 3,410 control firm-years for the write-downs analyses, and 2,359 treatment firm-years and 4,147 control firm-years for the accrual reversals analyses. I provide descriptive statistics on the impact of the matching procedure on the propensity scores and covariate balance at Section 6.3.

6.2 Descriptive statistics

Table 3 documents descriptive statistics for the variables used in the main regression analyses. Panel A is based on the sample used in the write-downs analyses. About 26 percent of the firm-years recorded a write-down. The average write-down over all the firm-years is about three percent of beginning market value, and among only the firms that recorded a write-

[22] I omit firms under the jurisdiction of the Eighth Circuit because there is disagreement over its legal position on the *ex post* loss rule (see Section 5.1).

Table 2: Sample selection

	All		Treatment		Control	
	Firm-yrs.	Firms	Firm-yrs.	Firms	Firm-yrs.	Firms
Panel A: Initial sample						
Firm-years between 2003 and 2008	66,141	14,867	-	-	-	-
US-headquartered & with state data	49,673	11,066	-	-	-	-
Treatment & control jurisdictions	47,377	10,580	10,732	2,417	36,645	8,163
High-litigation industries	11,233	2,445	3,916	874	7,317	1,571
Minimum starting assets	9,621	2,187	3,348	774	6,273	1,413
Panel B: Write-downs sample						
Initial sample of firm-years	9,621	2,187	3,348	774	6,273	1,413
Material starting intangibles	5,911	1,558	1,992	539	3,919	1,019
Require variable availability	**5,381**	1,415	**1,882**	513	**3,499**	902
Propensity-matched sample	**5,264**	1,397	**1,854**	506	**3,410**	891
Panel C: Accrual reversals sample						
Initial sample of firm-years	9,621	2,187	3,348	774	6,273	1,413
Require variable availability	**6,571**	1,611	**2,383**	597	**4,188**	1,014
Propensity-matched sample	**6,506**	1,600	**2,359**	591	**4,147**	1,009

This table details the sample selection for the main tests of this study; please see Sections 9.1 and 9.2 for details on the samples used in the falsification tests. I begin with unique Compustat firm-year observations between 2003 and 2008 inclusive that are headquartered (Compustat: *loc*) in the United States and located in either the treatment or control jurisdictions. Treatment firms are located in states (Compustat: *state*) under the jurisdiction of the Ninth Circuit, while control firms are located in states under the jurisdictions of circuits other than the Eighth and Ninth Circuits. I then reduce the sample to firms in high-litigation industries—biotechnology (SIC codes 2833–2836 and 8731–8734); computer manufacturing and software (SIC 3570–3577 and 7370–7374); electronics manufacturing (SIC 3600–3674); and telecommunications and electric services (SIC 4810–4813, 4911, and 4931)—and omit firms with beginning total assets (*at*) less than $1 million. Panel B details the further sample attrition specific to the write-downs analyses. I restrict the sample to firm-years with beginning intangibles (*intan*) of at least 1% of beginning total assets, and require availability of variables used in the main regression analyses and $abtm_t$ for the additional analyses. This results in an industry-matched sample of 5,381 firm-years; I also construct a propensity-matched sample (see Section 5.4) comprising 5,264 firm-years. Panel C details the further sample attrition specific to the accrual reversals analyses. After requiring availability of variables for the main regression analyses the sample comprises 6,571 firm-years, and the propensity-matched sample comprises 6,506 firm-years.

down, the average write-down is about ten percent of beginning market value. The treatment and control samples are significantly different along several variables; in particular, treatment firms are younger, have higher valuations, and have lower intangibles intensity. The most statistically significant difference is in firm age: treatment firms have an average logged age of 2.45 while control firms have an average logged age of 2.67, corresponding to 11.6 and 14.4 years respectively, and the difference in means has a t-statistic of −13.32.

Panel B of Table 3 is based on the sample used in the accrual reversals analyses, and documents descriptive statistics for the variables used in the main regression analyses. The average working capital accrual errors at the current and previous years are close to zero, with treatment firms having only slightly more negative accrual errors than control firms. Treatment firms are significantly larger and have significantly higher growth than control firms: on average, treatment firms have sales growth of 27 percent and average logged market values of 5.53, while control firms have sales growth of 20 percent and average logged market values of 5.23. As in the write-downs sample, the most statistically significant difference is in firm age: on average, treatment firms have a logged age of 2.47 while control firms have a logged age of 2.70, corresponding to 11.8 and 14.9 years respectively.

Panel C of Table 3 documents the mean current-year (t) and previous-year ($t-1$) accrual errors within each quintile bin. For current-year accruals, the extreme quintile bins have average working capital accrual errors of −14.3 percent and 12.4 percent respectively. For previous-year accruals, the extreme quintile bins have average working capital accrual errors of −14.8 percent and 13.0 percent respectively

6.3 Matching descriptives

6.3.1 Impact of matching on propensity scores

Table 4 summarizes the impact of the propensity matching procedure described at Section 5.4. Each year I estimate the propensity that an observation is in the treatment subsample given the potential confounding variables, and I drop treatment and control observations outside the support of the propensity score, resulting in slightly smaller samples: 5,264 firm-years for the write-downs analyses, and 6,506 for the accrual reversals analyses. I use a full matching procedure (Rosenbaum, 1991) that weights the observations to minimize the difference in propensity scores.

Columns 2 to 5 of Table 4 document the differences in mean propensity scores each year between the treatment and control subsamples, after dropping observations outside the support of the propensity scores each year but before weighting. The treatment firms have significantly higher propensity scores every year as expected, with the estimated probability

Table 3: Descriptive statistics

Panel A: Write-downs sample

	Pooled		Treatment		Control		Difference	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	T−C	t-stat.
wdd_t	0.26	0.44	0.27	0.45	0.26	0.44	0.02	1.23
wd_t	-0.03	0.11	-0.03	0.11	-0.03	0.11	-0.003	-0.86
$abtm_{t-1}$	0.57	0.31	0.55	0.31	0.57	0.31	-0.03***	-3.01
$abtmd_{t-1}$	0.08	0.27	0.07	0.26	0.08	0.27	-0.003	-0.37
int_{t-1}	0.23	0.20	0.23	0.19	0.24	0.20	-0.01**	-2.04
roa_t	-0.15	0.43	-0.16	0.38	-0.15	0.45	-0.004	-0.34
log_age_t	2.59	0.64	2.45	0.55	2.67	0.67	-0.23***	-13.32
log_at_{t-1}	5.18	2.29	5.20	2.14	5.16	2.36	0.04	0.66
log_mv_{t-1}	5.51	2.27	5.63	2.22	5.44	2.30	0.19***	3.03
Firm-yrs.	5,381		1,882		3,499			
Firms	1,415		513		902			

Panel B: Accrual reversals sample

	Pooled		Treatment		Control		Difference	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	T−C	t-stat.
acc_t	-0.005	0.11	-0.01	0.11	-0.003	0.11	-0.005*	-1.69
acc_{t-1}	-0.01	0.12	-0.01	0.11	-0.004	0.12	-0.004	-1.47
roa_t	-0.17	0.44	-0.17	0.40	-0.16	0.45	-0.01	-0.81
$growth_t$	0.22	0.80	0.27	0.90	0.20	0.74	0.07***	3.08
$ebtm_{t-1}$	0.36	0.83	0.37	0.79	0.36	0.86	0.02	0.74
log_age_t	2.62	0.64	2.47	0.57	2.70	0.67	-0.22***	-14.39
log_mv_{t-1}	5.34	2.29	5.53	2.19	5.23	2.33	0.30***	5.29
Firm-yrs.	6,571		2,383		4,188			
Firms	1,611		597		1,014			

Panel C: Accrual error quintiles

acc_t quintile	Q1	Q2	Q3	Q4	Q5
Observations	1,314	1,315	1,313	1,315	1,314
Mean acc_t	-0.143	-0.026	-0.001	0.023	0.124
acc_{t-1} quintile	Q1	Q2	Q3	Q4	Q5
Observations	1,314	1,315	1,313	1,315	1,314
Mean acc_{t-1}	-0.148	-0.029	-0.003	0.024	0.130

Panels A and B of this table document descriptive statistics for the variables used in the main regression analyses, for the write-downs analyses and the accrual reversals analyses respectively. The accrual error variables are winsorized at ±50 percent and other non-discrete variables are winsorized each year at the top and bottom percentiles. The last two columns document the difference in means between treatment and control firms, with p-values labeled as follows: *p<0.1; **p<0.05; ***p<0.01. Panel C documents the average current-year (t) and previous-year ($t-1$) accrual errors within quintile bins, based on the accrual reversals sample.

Table 4: Impact of propensity matching

Panel A: Write-downs analyses (N = 5,264)

	Mean unweighted propensity scores (%)				Mean weighted propensity scores (%)			
Year	Treatment	Control	$T - C$	t-stat.	Treatment	Control	$T - C$	t-stat.
2003	39.53	32.24	7.29***	8.03	39.53	39.54	-0.01	-0.01
2004	41.45	31.76	9.69***	9.25	41.45	41.46	-0.01	-0.01
2005	40.46	32.02	8.44***	9.15	40.46	40.47	-0.01	-0.01
2006	40.86	31.01	9.85***	9.22	40.86	40.84	0.02	0.02
2007	42.72	33.33	9.38***	8.51	42.72	42.69	0.03	0.03
2008	42.05	31.70	10.35***	9.03	42.05	42.05	0.003	0.003
Pooled	41.16	32.00	9.16***	22.59	41.16	41.13	0.04	0.01

Panel B: Accrual reversals analyses (N = 6,506)

	Mean unweighted propensity scores (%)				Mean weighted propensity scores (%)			
Year	Treatment	Control	$T - C$	t-stat.	Treatment	Control	$T - C$	t-stat.
2003	41.18	33.20	7.99***	10.09	41.18	41.17	0.02	0.02
2004	41.94	32.24	9.69***	10.63	41.94	41.97	-0.03	-0.03
2005	42.91	32.41	10.49***	10.80	42.91	42.91	-0.001	-0.001
2006	41.95	33.21	8.74***	9.53	41.95	41.96	-0.01	-0.01
2007	42.91	32.72	10.19***	10.14	42.91	42.93	-0.02	-0.02
2008	43.14	33.46	9.68***	9.57	43.14	43.14	-0.01	-0.01
Pooled	42.29	32.86	9.43***	25.91	42.29	42.29	0.001	-0.02

This table documents the impact of the propensity matching procedure described at Section 5.4. Each year I estimate the propensity that an observation is in the treatment subsample using a logit model with predictors as listed in Section 5.4. Each year, extreme treatment and control observations, i.e. those outside the support of the propensity score, are dropped. I use a full matching procedure (Rosenbaum, 1991) that weights the observations to minimize the difference in propensity scores. The columns on the left document the differences in propensity scores each year after dropping extreme observations but before weighting, and the columns on the right document the differences in propensity scores each year after applying the weights. I document the t-statistics for the differences in mean propensity score each year, and the t-statistic for the pooled sample is adjusted for year fixed effects. The p-values of the t-tests of the differences in propensity scores are labeled as follows: *p<0.1; **p<0.05; ***p<0.01.

of treatment about seven to ten percentage points higher in the treatment subsamples each year. Columns 6 to 9 of Table 4 report the same statistics after applying the weights from the matching procedure to each observation. Across all years the difference in propensity scores between the treatment and control subsamples become insignificant; in fact the mean propensity scores become almost identical each year.

6.3.2 Covariate balance

While the preceding subsection (Section 6.3.1) showed that the propensity matching procedure resulted in near-identical average propensity scores between the treatment and control subsamples, there may still be some imbalance in individual covariates.

I report the impact of the matching procedure on covariate balance at Table 5. For each variable I report the treatment and control subsample means respectively, and the difference between the treatment and control subsample means. The columns on the left are based on the sample before matching, and the columns on the right are based on the sample after matching, with weights applied according to the matching procedure. I report the statistics for all variables used in the matching models, omitting industry fixed effects for brevity.

I assess covariate balance using the absolute standardized difference in means between the treatment and control subsamples, pooled over all years.[23] As in Ho et al. (2011), the standardized difference in means is defined as the difference in means scaled by the standard deviation in the treated subsample. I use an absolute standardized difference of 25 percent as a cutoff beyond which the amount of remaining bias is unacceptably large (e.g. Rosenbaum & Rubin, 1985, p. 37).

Panel A of Table 5 reports the covariate balance for the write-downs sample. The largest absolute standardized difference is in log_age_t, 41.05 percent, substantially larger than the cutoff of 25 percent. In other words, the difference in logged age between the treatment and control subsamples is almost half of the standard deviation in the treatment subsample. The other covariates have absolute standardized differences below nine percent. After matching, the absolute standardized difference for log_age_t decreases from 41.05 percent to 8.68 percent, and all other covariates have absolute standardized differences of less than five percent.

Panel B of Table 5 reports the covariate balance for the accrual reversals sample. As in Panel A, the largest absolute standardized difference is in log_age_t, at 39.44 percent, larger than the cutoff of 25 percent. log_mv_{t-1} also has a relatively large absolute standardized

[23] In general, using t-statistics to assess covariate balance is "highly misleading" (Ho et al., 2011, p. 4). See Imai et al. (2008, p. 495–498) for a detailed discussion of the *balance test fallacy* in using hypothesis tests, and t-tests in particular, in balance assessment.

difference of 13.89 percent. After matching, the absolute standardized difference for log_age_t and log_mv_{t-1} decrease to 7.58 percent and 4.38 percent respectively, and all other variables have absolute standardized differences of less than six percent.

Table 5: Covariate balance before and after matching

Panel A: Write-downs sample

	Covariate means before matching				Covariate means after matching			
	Treatment	Control	$T-C$	$\lvert\frac{T-C}{\sigma}\rvert$	Treatment	Control	$T-C$	$\lvert\frac{T-C}{\sigma}\rvert$
$abtm_{t-1}$	0.55	0.57	-0.03	8.60%	0.55	0.55	-0.002	0.59%
int_{t-1}	0.23	0.24	-0.01	5.94%	0.23	0.24	-0.01	3.95%
roa_t	-0.16	-0.15	-0.004	1.04%	-0.16	-0.17	0.01	3.95%
log_age_t	2.45	2.67	-0.23†	41.05%	2.46	2.41	0.05	8.68%
log_at_{t-1}	5.20	5.16	0.04	1.96%	5.19	5.09	0.10	4.46%
log_mv_{t-1}	5.63	5.44	0.19	8.77%	5.61	5.51	0.10	4.45%
Firm-yrs.	1,882	3,499			1,854	3,410		
Firms	513	902			506	891		

Panel B: Accrual reversals sample

	Covariate means before matching				Covariate means after matching			
	Treatment	Control	$T-C$	$\lvert\frac{T-C}{\sigma}\rvert$	Treatment	Control	$T-C$	$\lvert\frac{T-C}{\sigma}\rvert$
acc_{t-1}	-0.01	-0.004	-0.004	3.79%	-0.01	-0.01	0.0002	0.16%
roa_t	-0.17	-0.16	-0.01	2.20%	-0.17	-0.19	0.02	5.52%
$growth_t$	0.27	0.20	0.07	7.46%	0.26	0.25	0.01	0.70%
$ebtm_{t-1}$	0.37	0.36	0.02	1.96%	0.37	0.37	0.01	1.05%
log_age_t	2.47	2.70	-0.22†	39.44%	2.48	2.44	0.04	7.58%
log_mv_{t-1}	5.53	5.23	0.30	13.89%	5.51	5.42	0.10	4.38%
Firm-yrs.	2,383	4,188			2,359	4,147		
Firms	597	1,014			591	1,009		

Panels A and B of this table document the covariate balance before and after the propensity matching procedure described at Section 5.4. For each variable I report treatment and control means, the difference in means, and the absolute standardized difference in means. The standardized difference in means is defined as the difference in means scaled by the standard deviation in the treatment subsample. The columns on the left report the statistics before matching. The columns on the right report the statistics after matching, using means and standard deviations weighted according to the matching procedure. The difference in means is labeled with † if the absolute standardized difference is greater than 25 percent.

7 Write-downs

7.1 Delaying of write-downs

The results from estimating Equation 1 are documented at Table 6. Columns (1) to (3) are based on the industry-matched sample, while columns (4) to (6) incorporate propensity matching. In columns (1), (2), (4), and (5), I estimate Equation 1 within the pre- and post-*Dura* periods respectively after omitting the $post_t$ dummy. In the regressions with propensity matching I use quasibinomial logistic models weighted according to the matching procedure described in Section 5.4.[24]

The significantly negative coefficient estimates for $abtmd_{t-1} \times post_t \times treat_t$ in columns (3) and (6) suggest that treatment firms become more likely to delay write-downs after *Dura* relative to control firms. The estimated effect is also highly significant economically. From the estimates after propensity matching in column (6), for example, the coefficient of -1.074 corresponds to about a 65.8% decline $(1-e^{-1.074})$ in the odds of a write-down, *ceteris paribus*.

The significantly positive coefficient estimates for $abtmd_{t-1} \times treat_t$ in columns (1) and (4)—the pre-*Dura* period—suggest that before *Dura*, write-downs were more timely in the Ninth Circuit relative to control circuits. This is consistent with the circuit split before *Dura*: the *ex post* loss rule was the prevailing legal standard in the control circuits but not the Ninth Circuit before *Dura*, so firms in control circuits would have greater incentive to delay write-downs in order to disrupt the causal link between alleged misconduct and a price decline. From column (4), the coefficient of 0.597 corresponds to about 81.7% higher odds $(e^{0.597} - 1)$ of recording a write-down.

Figure 2 illustrates the main results documented at Table 6 by plotting the difference in the timeliness of write-downs between treatment and control firms—i.e. the coefficient on $abtmd_{t-1} \times treat_t$ when Equation 1 is estimated without $post_t$—for specific time periods. Specifically, Panels A and B correspond to Columns (1), (2), (4), and (5) of Table 6, and illustrate the decline in timeliness of write-downs between the pre- and post-*Dura* periods in treatment firms relative to control firms. Panels C and D show the treatment-control difference within each year, and show a large decline in the timeliness of write-downs for treatment relative to control firms after 2005.

Other significant coefficients in Table 6 suggest changes in certain subsamples around *Dura*. The increase in the coefficient on $abtmd_{t-1}$ after *Dura* suggest that the timeliness of write-downs in control firms increased, and the increase in the coefficient on $treat_t$ after *Dura* suggests that the odds of writing down when book-to-market is low increased in treatment relative to control firms. In untabulated analyses, I find evidence that these are driven substantially by 2008, the final year in the sample, and are therefore likely to be related to the financial crisis. Specifically, write-downs conditional on a high book-to-market increased markedly in 2008 for both treatment and control firms, and write-downs when beginning

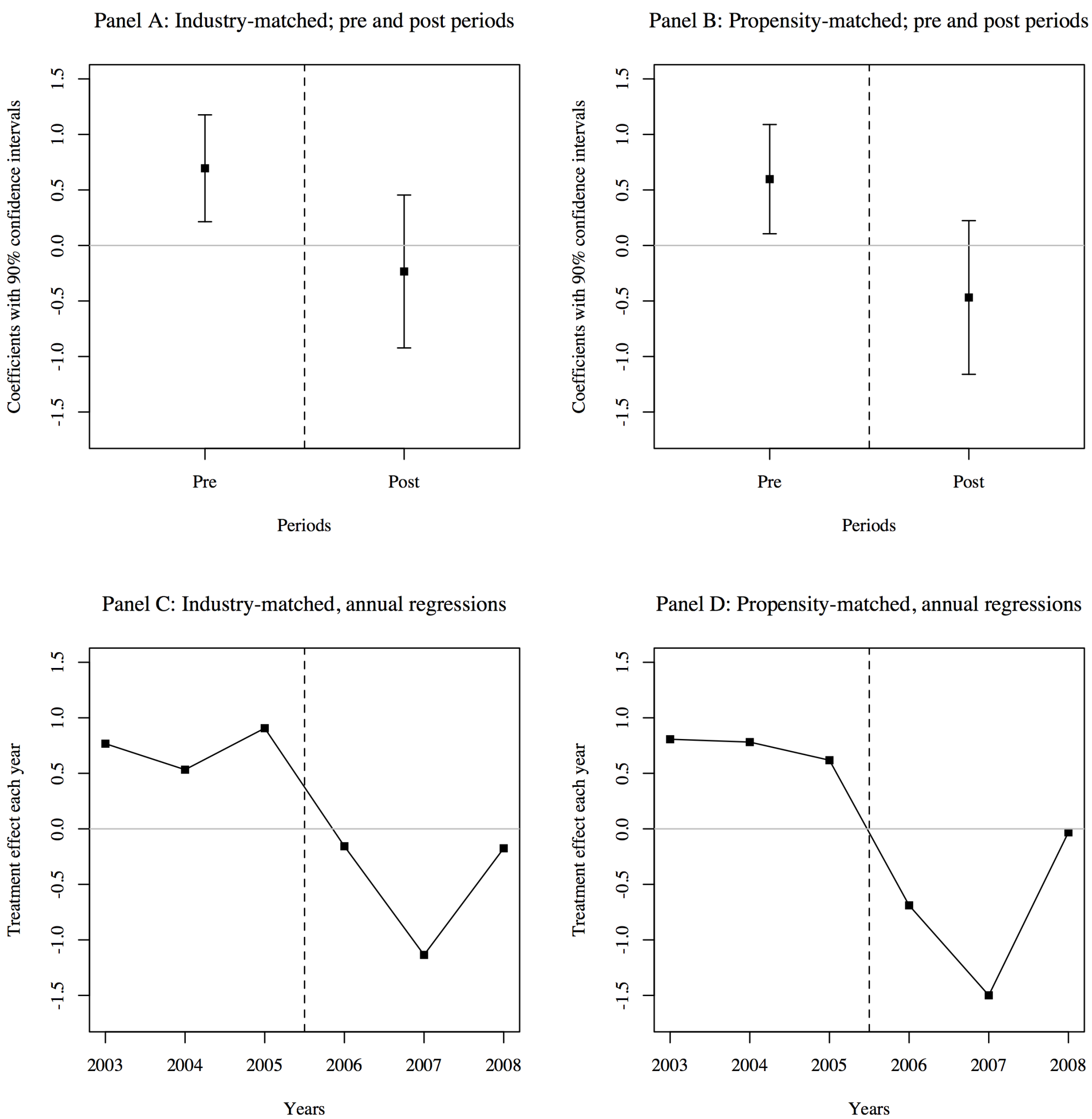
[24] Using a quasibinomial distribution generalizes the logistic model by allowing for the variance of the dependent variable to be greater than what would be expected from binomial data (see McCullagh & Nelder, 1989).

Table 6: Odds of writing down and beginning book-to-market

	Dependent variable: wdd_t					
Model:	Logistic (binomial)			Logistic (quasibinomial)		
Matching:	Industry-matched			Propensity-matched		
Period:	Pre	Post	DID	Pre	Post	DID
	(1)	(2)	(3)	(4)	(5)	(6)
$abtmd_{t-1} \times treat_t$	**0.695****	**−0.234**	0.689**	**0.597****	**−0.469**	0.602**
	(0.292)	(0.418)	(0.292)	(0.299)	(0.421)	(0.299)
$abtmd_{t-1}$	0.039	0.664***	−0.047	−0.023	0.786***	−0.063
	(0.199)	(0.241)	(0.191)	(0.205)	(0.245)	(0.196)
$treat_t$	−0.169	0.177*	−0.178*	−0.135	0.333***	−0.141
	(0.108)	(0.107)	(0.105)	(0.104)	(0.104)	(0.104)
$abtmd_{t-1} \times post_t \times treat_t$			**−0.871***			**−1.074****
			(0.500)			(0.504)
$abtmd_{t-1} \times post_t$			0.931***			1.144***
			(0.288)			(0.289)
$post_t \times treat_t$			0.372***			0.470***
			(0.143)			(0.145)
$post_t$			−0.064			−0.172*
			(0.086)			(0.089)
Year FEs	Yes	Yes	No	Yes	Yes	No
Controls & Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,793	2,588	5,381	2,742	2,522	5,264
McFadden R^2	0.075	0.103	0.079	0.087	0.106	0.081
Nagelkerke R^2	0.120	0.165	0.127	0.139	0.169	0.130

This table shows the results from estimating Equation 1. I estimate the model using logistic regressions, and I use a quasibinomial distribution for the propensity-matched regressions. In columns (1), (2), (4), and (5), I estimate the model for specific time periods. The control variables are int_{t-1}, roa_t, log_age_t, log_at_{t-1}, and log_mv_{t-1}, and variable definitions are at Appendix D. Standard errors are shown in parentheses and the p-values are labeled as follows: *p<0.1; **p<0.05; ***p<0.01.

Figure 2: Write-downs and book-to-market, treatment-control difference



This figure illustrates the results documented at Table 6 by plotting the coefficient of $abtmd_{t-1} \times treat_t$ when Equation 1 is estimated within specific time periods without $post_t$. A coefficient of β implies that the increase in the odds of writing down when a firm's starting book-to-market becomes greater than one is e^{β} times as much in a treatment firm relative to a control firm, on average. Panels A and B correspond to the results at Columns (1), (2), (4), and (5) of Table 6, and examine the coefficients within the Pre and Post periods respectively. For Panels C and D I estimate Equation 1 each year without $post_t$ and without year fixed effects.

book-to-market is low increased more in 2008 in treatment relative to control firms. In untabulated sensitivity analyses that replicate the tests after omitting 2005 and 2008, I find that my main inferences are unchanged. In this alternative specification, the coefficients on $abtmd_{t-1}$, $treat_t$, $abtmd_{t-1} \times post_t$, and $post_t \times treat_t$ that are statistically significant in Table 6 become smaller in magnitude and statistical significance. Please see Section 7.3 for details on this and other sensitivity and additional analyses.

7.2 Level of write-downs when they occur

Next, I examine the impact of *Dura* on the level of write-downs that are recorded by estimating Equation 2 for firm-years with write-downs (i.e. where $wdd_t = 1$). The results are documented at Table 7. As in Table 6, columns (1) to (3) are based on the industry-matched sample, while columns (4) to (6) incorporate propensity matching. In columns (1), (2), (4), and (5), I estimate Equation 1 within the pre- and post-*Dura* periods respectively after omitting the $post_t$ dummy. For the tests with propensity matching, I carry out the matching methodology using only the observations with write-downs.

The significantly negative coefficient estimates for $abtm_{t-1} \times post_t \times treat_t$ in columns (3) and (6) suggest that write-downs by treatment firms became larger post-*Dura* relative to control firms, for a given increase in beginning book-to-market. The effect is highly significant economically: the coefficient of -0.208 in column (6) corresponds to a 6.7 percentage point increase in the level of write-downs as a percentage of beginning market value, per standard deviation of beginning book to market.[25]

The significantly positive coefficient estimates for $abtm_{t-1} \times treat_t$ in columns (1) and (4)—the pre-*Dura* period—suggest that before *Dura*, write-downs were smaller relative to beginning book-to-market, in the Ninth Circuit relative to control circuits. This is consistent with the circuit split before *Dura*: because firms in control circuits had greater incentive to delay write-downs, the write-downs are larger when they are eventually recorded: "when firms have bad news to report that can be delayed no longer, they may as well report as much bad news as possible." (Spindler, 2007, p. 684-685)

7.3 Additional tests and sensitivity analyses

I next carry out a several additional tests and sensitivity analyses that I leave untabulated for brevity. First, I replicate my estimation of Equation 1 using different book-to-market

[25]The standard deviation of $abtm_{t-1}$ in the sample of firm-years recording a write-down is 0.323. Compustat records write-downs (i.e. *wdp* and *gdwlip*) as negative values, so the economic significance of 6.7 percentage points is estimated as follows: $-0.208 \times 0.323 \times (-1) = 0.0672$.

Table 7: Level of write-downs when they occur

	Dependent variable: wd_t					
Model:	Ordinary least squares			Weighted least squares		
Matching:	Industry-matched			Propensity-matched		
Period:	Pre	Post	DID	Pre	Post	DID
	(1)	(2)	(3)	(4)	(5)	(6)
$abtm_{t-1} \times treat_t$	**0.114*****	**−0.032**	0.113***	**0.138*****	**−0.060**	0.139***
	(0.035)	(0.039)	(0.034)	(0.035)	(0.037)	(0.033)
$abtm_{t-1}$	−0.003	−0.096***	−0.051*	−0.020	−0.117***	−0.087***
	(0.031)	(0.036)	(0.027)	(0.032)	(0.034)	(0.026)
$treat_t$	−0.067**	0.009	−0.068***	−0.088***	0.037	−0.089***
	(0.027)	(0.027)	(0.025)	(0.026)	(0.026)	(0.025)
$abtm_{t-1} \times post_t \times treat_t$			**−0.144*****			**−0.208*****
			(0.054)			(0.053)
$abtm_{t-1} \times post_t$			0.009			0.039
			(0.032)			(0.032)
$post_t \times treat_t$			0.072*			0.126***
			(0.038)			(0.038)
$post_t$			−0.019			−0.048**
			(0.023)			(0.022)
Year FEs	Yes	Yes	No	Yes	Yes	No
Controls & Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	705	716	1,421	649	653	1,302
Adjusted R^2	0.417	0.494	0.433	0.427	0.546	0.457

This table shows the results from estimating Equation 2 for observations where $wdd_t = 1$. I estimate the model using least squares, and I use weighted least squares for the propensity-matched regressions. I apply the propensity matching within this smaller sample where $wdd_t = 1$. In columns (1), (2), (4), and (5), I estimate the model for specific time periods. The control variables are int_{t-1}, roa_t, log_age_t, log_at_{t-1}, and log_mv_{t-1}, and variable definitions are at Appendix D. Standard errors are shown in parentheses and the p-values are labeled as follows: *p<0.1; **p<0.05; ***p<0.01.

threshold values when defining $abtmd_{t-1}$. Under both the industry-matching and propensity-matching methodologies, when the threshold is slightly less than one (0.9 or 0.95), the main triple difference coefficient becomes smaller in magnitude and insignificant at conventional significance levels. This may be due to the introduction of excessive noise as more firms are erroneously assumed to require write-downs under GAAP. In contrast, when the threshold is slightly more than one (1.05 and 1.1), the coefficient becomes larger in economic and statistical significance. This is consistent with GAAP rules allowing the carrying value of certain assets classes to exceed their fair values, causing incentives to avoid GAAP-required write-downs to apply only at higher thresholds for certain firms.[26]

Second, I replicate my tests using an alternative sample period: 2003 to 2004 as the pre-*Dura* period, and 2006 to 2007 as the post-*Dura* period. The inferences under this alternative specification are similar to that of the main tests and are generally more significant economically. For example, the coefficients on $abtmd_{t-1} \times post_t \times treat_t$ are -1.301 and -1.602 under the two matching methods respectively, larger than -0.871 and -1.074 in the main tests (see Table 6). Similarly, the coefficients on $abtm_{t-1} \times post_t \times treat_t$ are larger under the alternative specification than in the main tests (Table 7).

Third, I replicate the main tests of write-down timeliness (see Table 6) using an alternative definition of write-downs that includes other special items. Specifically, for this sensitivity analysis I define wdd_t as one if the firm recorded a write-down at t (as in my main tests), or if the firm recorded negative special items (Compustat: $spi < 0$) at t. I drop firm-years with missing special items in Compustat, resulting in a slight decline in the sample size. In this sensitivity analysis, the coefficients on the triple interaction $abtmd_{t-1} \times post_t \times treat_t$ remain significantly negative in both the industry-matched and propensity-matched samples, suggesting that treatment firms became more likely to delay write-downs or charges to special items after *Dura* relative to control firms, consistent with Hypothesis 1.

Finally, I examine the impact of *Dura* on ending asset book-to-market ratios ($abtm_t$) as a proxy for the degree of asset overvaluation. During the pre-*Dura* period, the mean ending book-to-market ratio is significantly lower for firms under the jurisdiction of the Ninth Circuit than for control firms. Ninth Circuit firms had an average book-to-market of 51.1 percent, while control firms had an average book-to-market of 54.5 percent; the difference in means is statistically significant with a t-statistic of -3.15. This is consistent with firms in the Ninth Circuit recording more timely asset write-downs than control firms before *Dura*, as documented at Table 6. In the post-*Dura* period, while the average book-

[26] An example cited in Lawrence et al. (2013) is the case of property, plant, and equipment and intangible assets with finite lives: "The carrying values of PP&E and finite-lived intangibles can therefore exceed their fair values when the carrying value is determined to be recoverable." (p. 15)

to-market ratio increased in both treatment and control firms, there is no evidence for a statistically significant difference in mean asset book-to-market ratio between treatment and control firms at conventional significance levels ($t = -0.19$).

8 Accrual reversals

8.1 Income-decreasing accrual error reversals

The results from estimating Equations 3 and 4 are documented at Tables 8 and 9 respectively. Columns (1) to (3) are based on the industry-matched sample, while columns (4) to (6) incorporate propensity matching. In columns (1), (2), (4), and (5), I estimate Equations 3 and 4 within the pre- and post-*Dura* periods respectively after omitting the $post_t$ dummy. As in the write-downs analyses (see Section 7.1), in the regressions with propensity matching I use quasibinomial logistic models weighted according to the matching procedure described in Section 5.4.

The significantly negative coefficient estimates for $I(q\_acc_{t-1} \geq 4) \times post_t \times treat_t$ and $I(q\_acc_{t-1} = 5) \times post_t \times treat_t$ in columns (3) and (6) of Tables 8 and 9 suggest that treatment firms become more likely to delay income-decreasing accrual error reversals after *Dura* relative to control firms. The estimated effects are also highly significant economically. The coefficient of -1.251 at column (6) of Table 8 corresponds to a 71.4 percent decline $(1-e^{-1.251})$ in the odds of a firm recording highly negative accrual errors, while the coefficient of -1.131 at column (6) of Table 8 corresponds to a 67.7 percent decline $(1 - e^{-1.131})$ in the odds.

In the regressions using the highest two starting accrual error quintiles as a proxy for highly positive accruals (Table 8), the coefficient on $I(q\_acc_{t-1} \geq 4) \times treat_t$ is significantly positive in the pre-*Dura* period under both industry matching (column 1) and propensity matching (column 4). When only the highest accrual error quintile is used (Table 8), the corresponding coefficients are also positive, but are only significant statistically under propensity matching. Under all specifications (columns 2 and 5 of Tables 8 and 9), the coefficients on $I(q\_acc_{t-1} \geq 4) \times treat_t$ and $I(q\_acc_{t-1} = 5) \times treat_t$ are significantly negative in the post-*Dura* periods.

The significantly positive coefficients on $I(q\_acc_{t-1} \geq 4) \times treat_t$ and $I(q\_acc_{t-1} = 5) \times treat_t$ in the pre-*Dura* period under propensity matching are consistent with firms in the Ninth Circuit having less incentive to delay income-decreasing accrual error reversals before *Dura*, and thus recording more timely downward reversals than matched control firms. Unlike the write-downs tests, the significantly negative coefficients in the post-*Dura* period suggests that the difference between treatment and control firms is statistically significant in the

Table 8: Downward accrual error reversals, highest two starting accrual quintiles

	Dependent variable: $I(q\_acc_t = 1)$					
Model:	Logistic (binomial)			Logistic (quasibinomial)		
Matching:	Industry-matched			Propensity-matched		
Period:	Pre	Post	DID	Pre	Post	DID
	(1)	(2)	(3)	(4)	(5)	(6)
$I(q\_acc_{t-1} \geq 4)$ $\times treat_t$	$\mathbf{0.392}^{**}$	$\mathbf{-0.678}^{***}$	0.392^{**}	$\mathbf{0.589}^{***}$	$\mathbf{-0.687}^{***}$	0.582^{***}
	(0.189)	(0.205)	(0.189)	(0.192)	(0.206)	(0.191)
$I(q\_acc_{t-1} \geq 4)$	0.044	0.571^{***}	0.040	−0.103	0.600^{***}	−0.126
	(0.113)	(0.127)	(0.113)	(0.117)	(0.128)	(0.116)
$treat_t$	-0.279^{**}	0.384^{***}	-0.291^{**}	-0.293^{**}	0.455^{***}	-0.278^{**}
	(0.127)	(0.136)	(0.126)	(0.123)	(0.134)	(0.122)
$I(q\_acc_{t-1} \geq 4)$ $\times post_t \times treat_t$			$\mathbf{-1.067}^{***}$			$\mathbf{-1.251}^{***}$
			(0.279)			(0.280)
$I(q\_acc_{t-1} \geq 4)$ $\times post_t$			0.537^{***}			0.742^{***}
			(0.169)			(0.171)
$post_t \times treat_t$			0.681^{***}			0.717^{***}
			(0.181)			(0.180)
$post_t$			-0.281^{**}			-0.346^{***}
			(0.115)			(0.114)
Year FEs	Yes	Yes	No	Yes	Yes	No
Controls & Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,544	3,027	6,571	3,514	2,992	6,506
McFadden R^2	0.109	0.123	0.114	0.115	0.124	0.115
Nagelkerke R^2	0.163	0.184	0.171	0.172	0.184	0.173

This table shows the results from estimating Equation 3. I estimate the model using logistic regressions, and use a quasibinomial distribution for the propensity-matched regressions. In columns (1), (2), (4), and (5), I estimate the model for specific time periods. The control variables are roa_t, $growth_t$, $ebtm_{t-1}$, log_age_t, and log_mv_{t-1}, and variable definitions are at Appendix D. Standard errors are shown in parentheses and the p-values are labeled as follows: *p<0.1; **p<0.05; ***p<0.01.

Table 9: Downward accrual error reversals, highest starting accrual quintile

	Dependent variable: $I(q\_acc_t = 1)$					
Model:	Logistic (binomial)			Logistic (quasibinomial)		
Matching:	Industry-matched			Propensity-matched		
Period:	Pre	Post	DID	Pre	Post	DID
	(1)	(2)	(3)	(4)	(5)	(6)
$I(q\_acc_{t-1} = 5)$ $\times treat_t$	**0.278**	$-$**0.482****	0.287	**0.442****	$-$**0.696*****	0.439**
	(0.217)	(0.230)	(0.216)	(0.223)	(0.232)	(0.221)
$I(q\_acc_{t-1} = 5)$	0.309**	0.827***	0.309**	0.229*	1.098***	0.209
	(0.129)	(0.140)	(0.128)	(0.137)	(0.143)	(0.135)
$treat_t$	$-$0.177	0.211*	$-$0.194*	$-$0.160	0.327***	$-$0.149
	(0.112)	(0.121)	(0.110)	(0.108)	(0.118)	(0.108)
$I(q\_acc_{t-1} = 5)$ $\times post_t \times treat_t$			$-$**0.766****			$-$**1.131*****
			(0.316)			(0.320)
$I(q\_acc_{t-1} = 5)$ $\times post_t$			0.525***			0.925***
			(0.188)			(0.194)
$post_t \times treat_t$			0.417***			0.467***
			(0.160)			(0.160)
$post_t$			$-$0.187*			$-$0.266***
			(0.100)			(0.100)
Year FEs	Yes	Yes	No	Yes	Yes	No
Controls & Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,544	3,027	6,571	3,514	2,992	6,506
McFadden R^2	0.111	0.129	0.118	0.116	0.137	0.122
Nagelkerke R^2	0.166	0.191	0.176	0.175	0.202	0.183

This table shows the results from estimating Equation 4. I estimate the model using logistic regressions, and use a quasibinomial distribution for the propensity-matched regressions. In columns (1), (2), (4), and (5), I estimate the model for specific time periods. The control variables are roa_t, $growth_t$, $ebtm_{t-1}$, log_age_t, and log_mv_{t-1}, and variable definitions are at Appendix D. Standard errors are shown in parentheses and the p-values are labeled as follows: *p<0.1; **p<0.05; ***p<0.01.

opposite direction after *Dura*. This may be due to an overreaction to *Dura* by Ninth Circuit firms.

Figures 3 and 4 illustrate the results documented at Tables 8 and 9 respectively by plotting the difference in the timeliness of income-decreasing accrual reversals between treatment and control firms—i.e. the coefficients on $I(q\_acc_{t-1} \geq 4) \times treat_t$ and $I(q\_acc_{t-1} = 5) \times treat_t$ when Equations 3 and 4 are estimated without $post_t$—for specific time periods. Specifically, Panels A and B of both figures correspond to Columns (1), (2), (4), and (5) of Tables 8 and 9 respectively, and illustrate the decline in timeliness of income-decreasing accrual reversals between the pre- and post-*Dura* periods in treatment firms relative to control firms. Panels C and D show the treatment-control difference within each year, and show a large decline in the timeliness of write-downs for treatment relative to control firms after 2005.

Figures 5 and 6 provide additional details on the economic significance of *Dura* on accrual reversals. These figures show the percentage of firms with highly positive accruals in the previous year that are in each quintile of accruals in the current year. For example, in Panel A of Figure 5, the top left data point indicates that in the pre-*Dura* period, about 24% of treatment firms with highly positive accruals last year are in the lowest quintile of accruals this year. Figure 5 is based on firm-years with previous-year accruals in the highest two quintiles, while Figure 6 is based on the highest quintile. Panel A of both figures show that in the pre-*Dura* period, treatment firms with highly positive accrual errors in the previous year were more likely than control firms to reverse accruals downwards to the lowest quintile of accrual errors. However, panel B of both figures show that after *Dura*, they became less likely than control firms to do so.

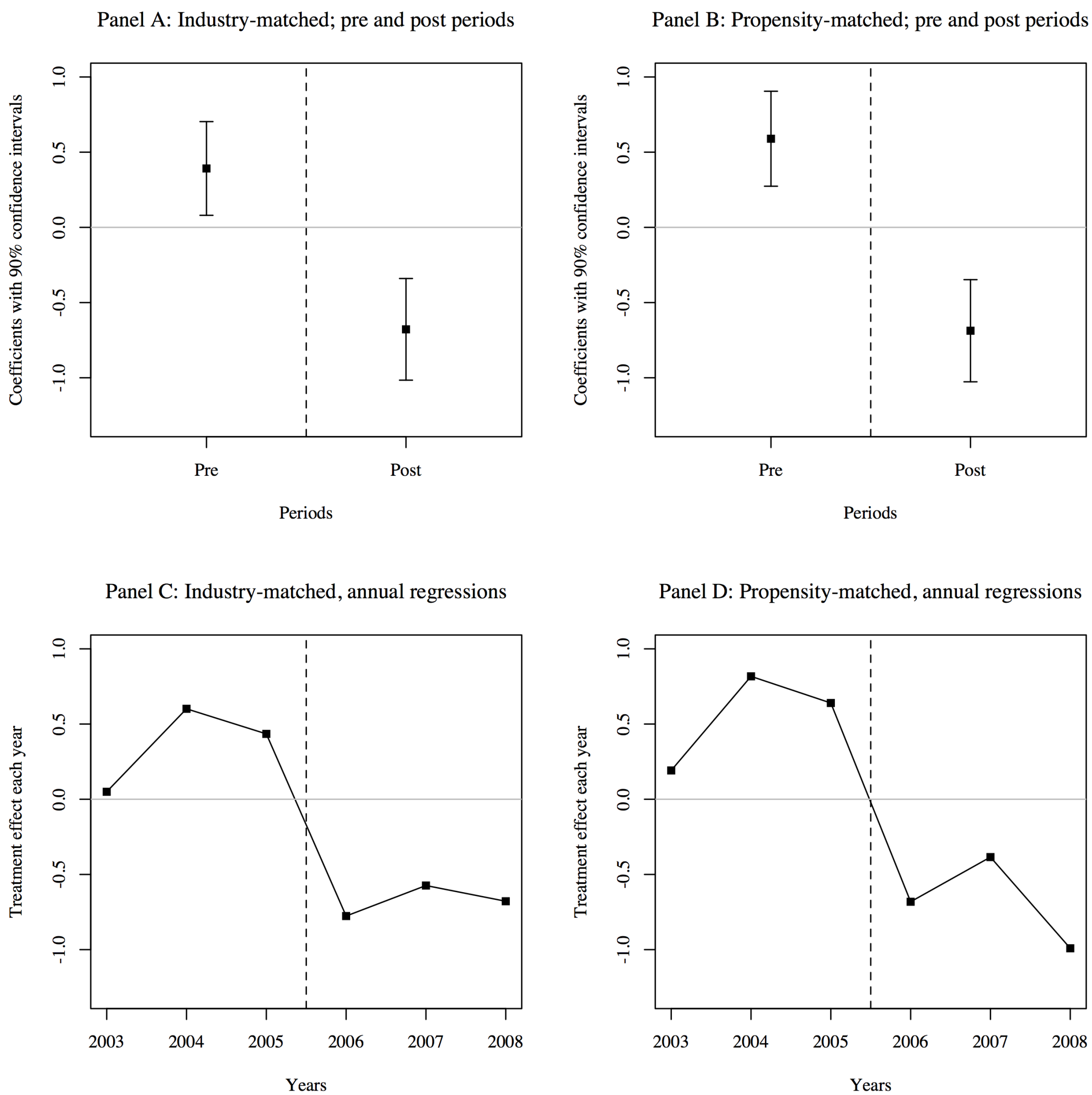
8.2 All accrual error reversals

Finally, the results from estimating Equation 5 are documented at Table 10. From columns (3) and (6), the significantly positive coefficients on $q\_acc_{t-1} \times post_t \times treat_t$ suggest that accrual error reversals declined after *Dura* in treatment firms, relative to control firms.

Since both q_acc_{t-1} and q_acc_t are quantile bins, a coefficient estimate of $\hat{\beta}$ indicates that an increase in the explanatory variable from the lowest to the highest quantile is associated with a $\hat{\beta} \times 100$ percentage point increase in the outcome variable along its distribution, on average. From columns (4) and (5), in the pre-*Dura* period, an increase in starting accruals from the lowest to the highest quintile decreased ending accruals by 7.5 percentage points more in treatment than control firms, but in the post-*Dura* period it decreased it by 10.1 percentage points *less*.

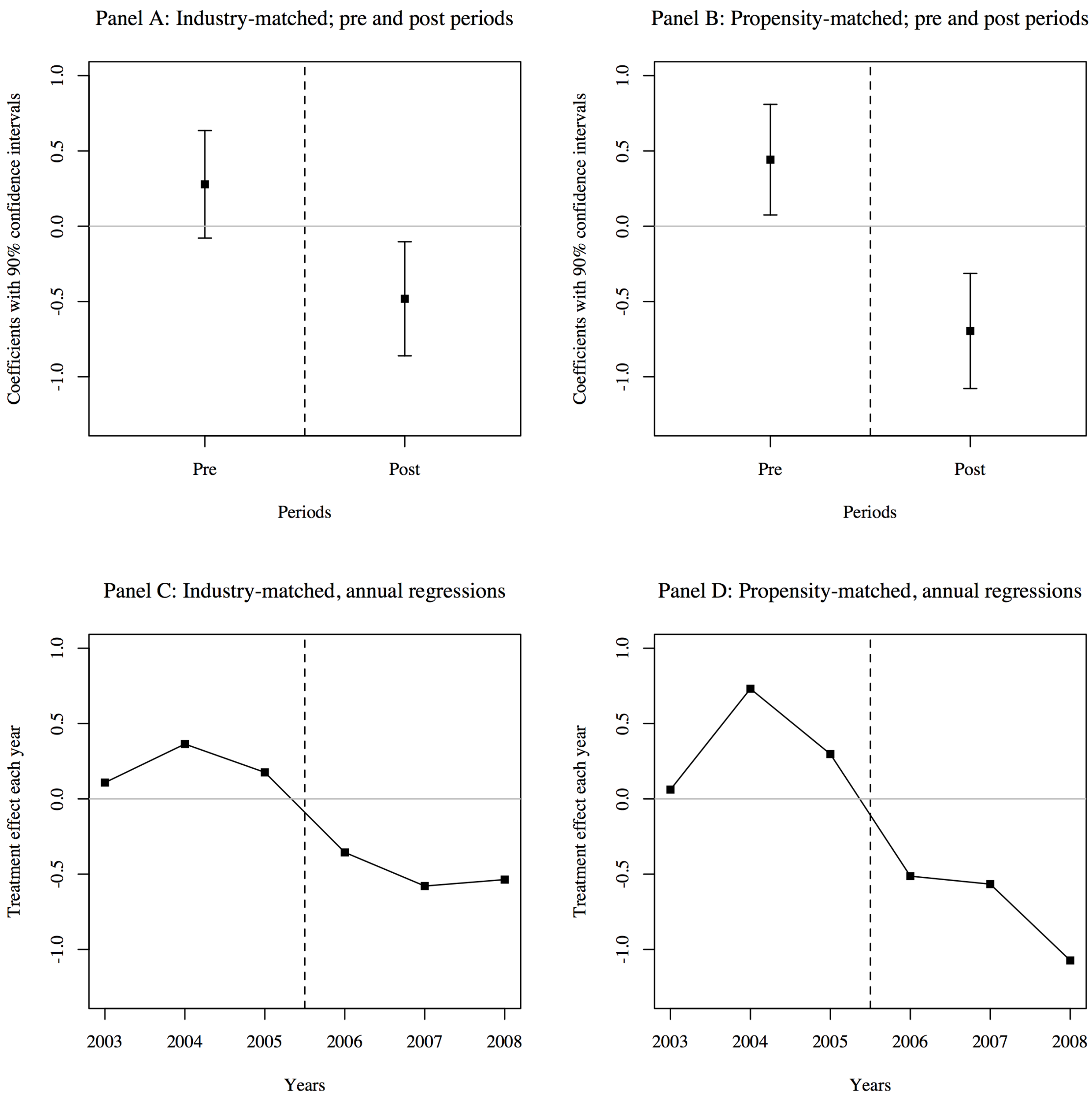
In untabulated analyses I find that the estimated coefficient on $q\_acc_{t-1} \times post_t \times treat_t$ becomes insignificant and close to zero when I omit firms in the lowest quintile of accrual

Figure 3: Downward accrual error reversals, treatment-control difference, highest two starting accrual quintiles



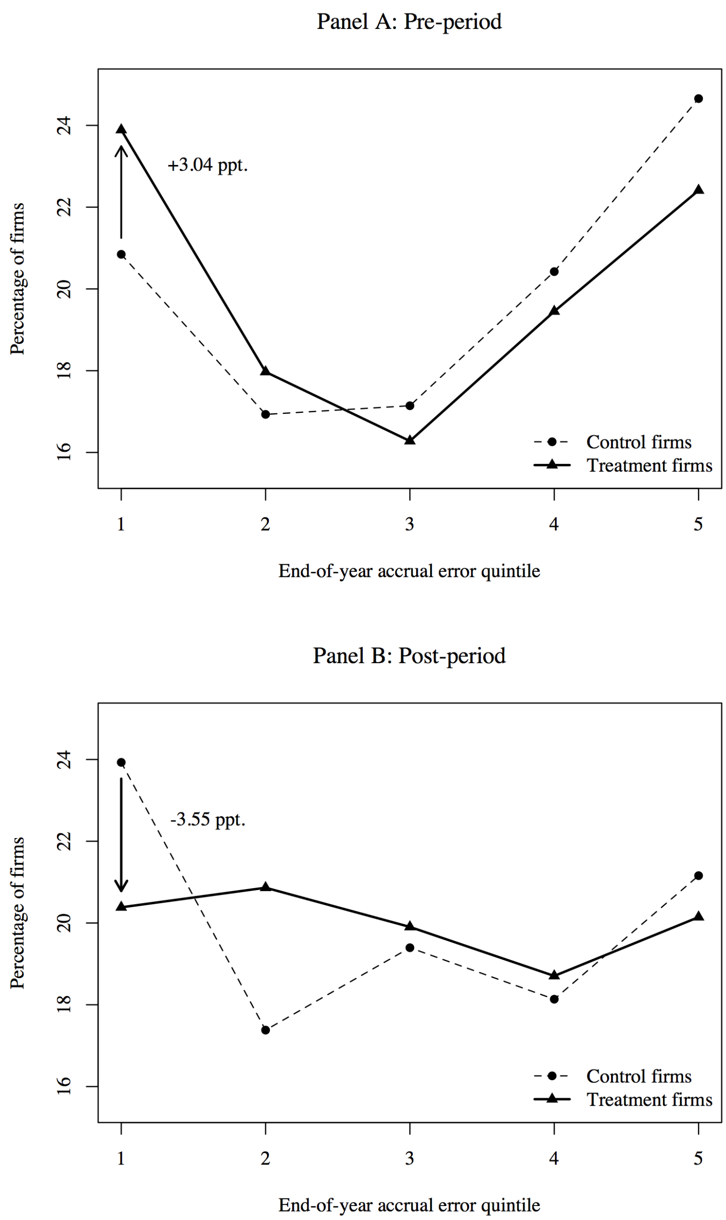
This figure illustrates the results documented at Table 8 by plotting the coefficient of $I(q\_acc_{t-1} \geq 4) \times treat_t$ when Equation 3 is estimated within specific time periods without $post_t$. A coefficient of β implies that the increase in the odds of a highly negative accrual error when a previous-year accrual error becomes highly positive is e^{β} times as much in a treatment firm relative to a control firm, on average. Panels A and B correspond to the results at Columns (1), (2), (4), and (5) of Table 8, and examine the coefficients within the Pre-*Dura* and Post-*Dura* periods respectively. For Panels C and D I estimate Equation 3 each year without $post_t$ and without year fixed effects.

Figure 4: Downward accrual error reversals, treatment-control difference, highest starting accrual quintile



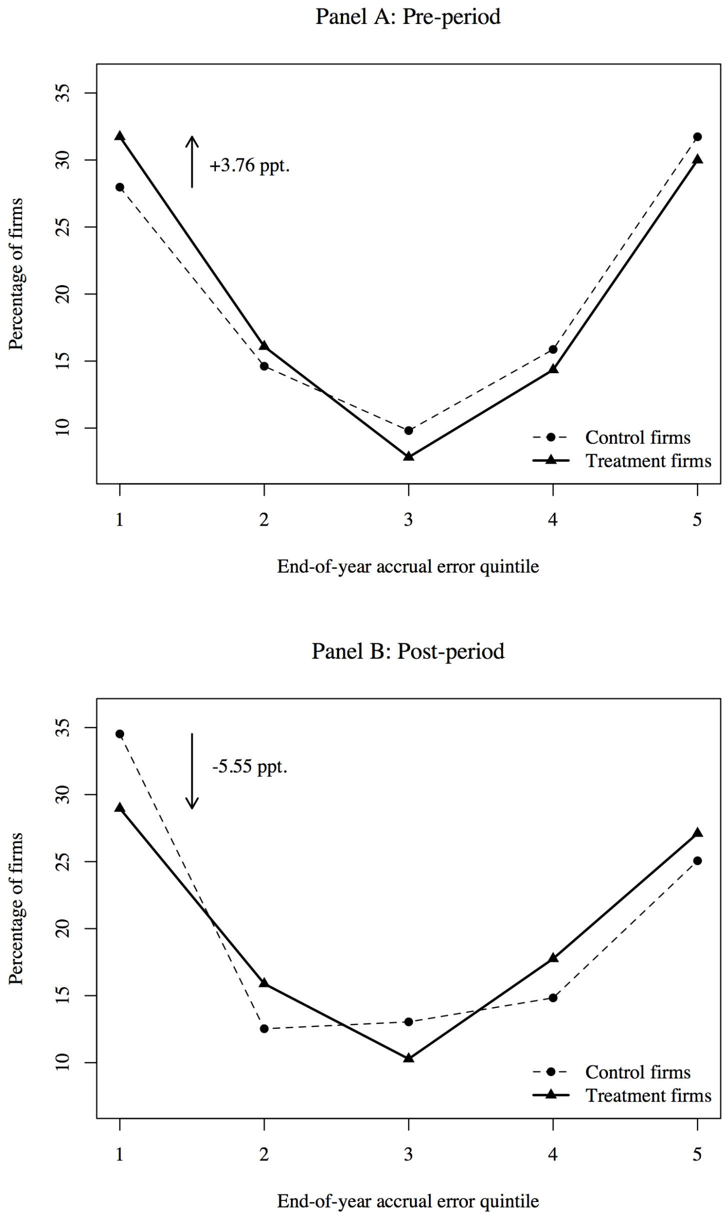
This figure illustrates the results documented at Table 9 by plotting the coefficient of $I(q\_acc_{t-1} = 5) \times treat_t$ when Equation 4 is estimated within specific time periods without $post_t$. A coefficient of β implies that the increase in the odds of a highly negative accrual error when a previous-year accrual error becomes highly positive is e^{β} times as much in a treatment firm relative to a control firm, on average. Panels A and B correspond to the results at Columns (1), (2), (4), and (5) of Table 9, and examine the coefficients within the Pre-*Dura* and Post-*Dura* periods respectively. For Panels C and D I estimate Equation 4 each year without $post_t$ and without year fixed effects.

Figure 5: Accrual reversals by ending quintile, highest two starting accrual quintiles



These figures plot the percentage of firms with previous-year accrual errors in the highest two quintiles that are in each quintile of accrual errors in the current year.

Figure 6: Accrual reversals by ending quintile, highest starting accrual quintile



These figures plot the percentage of firms with previous-year accrual errors in the highest quintiles that are in each quintile of accrual errors in the current year.

Table 10: All accrual error reversals

	Dependent variable: q_acc_t					
Model:	Ordinary least squares			Weighted least squares		
Matching:	Industry-matched			Propensity-matched		
Period:	Pre	Post	DID	Pre	Post	DID
	(1)	(2)	(3)	(4)	(5)	(6)
$q\_acc_{t-1} \times treat_t$	**−0.038**	**0.103*****	−0.038	**−0.075****	**0.101*****	−0.075**
	(0.035)	(0.037)	(0.034)	(0.035)	(0.037)	(0.034)
q_acc_{t-1}	0.026	−0.083***	0.027	0.061***	−0.091***	0.062***
	(0.020)	(0.022)	(0.020)	(0.021)	(0.022)	(0.020)
$treat_t$	0.107	−0.371***	0.115	0.193*	−0.341***	0.187*
	(0.114)	(0.123)	(0.114)	(0.113)	(0.120)	(0.112)
$q\_acc_{t-1} \times post_t \times treat_t$			**0.141*****			**0.178*****
			(0.050)			(0.050)
$q\_acc_{t-1} \times post_t$			−0.111***			−0.157***
			(0.030)			(0.030)
$post_t \times treat_t$			−0.497***			−0.528***
			(0.165)			(0.165)
$post_t$			0.404***			0.468***
			(0.102)			(0.101)
Year FEs	Yes	Yes	No	Yes	Yes	No
Controls & Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,544	3,027	6,571	3,514	2,992	6,506
Adjusted R^2	0.047	0.055	0.051	0.058	0.065	0.057

This table shows the results from estimating Equation 5. I estimate the model using least squares, and use weighted least squares for the propensity-matched regressions. In columns (1), (2), (4), and (5), I estimate the model for specific time periods. The control variables are roa_t, $growth_t$, $ebtm_{t-1}$, log_age_t, and log_mv_{t-1}, and variable definitions are at Appendix D. Standard errors are shown in parentheses and the p-values are labeled as follows: *p<0.1; **p<0.05; ***p<0.01.

errors at t. This indicates that the change in overall accruals error reversals after *Dura* is driven almost entirely by income-decreasing accruals.

8.3 Additional tests and sensitivity analyses

I next carry out several additional tests and sensitivity analyses that I leave untabulated for brevity.

As in the write-downs tests (see Section 7.3), I replicate the tests of accrual reversals using an alternative sample period in which the pre-*Dura* period is defined as 2003 and 2004, and the post-*Dura* period is defined as 2006 and 2007. Under this specification, the coefficients on $I(q\_acc_{t-1} \geq 4) \times post_t \times treat_t$, $I(q\_acc_{t-1} = 5) \times post_t \times treat_t$, and $q\_acc_{t-1} \times post_t \times treat_t$ are statistically significant and of comparable economic significance to the corresponding coefficients in the main analyses (Tables 8, 9, and 10), suggesting that the change in sample period does not have a substantial effect on my main findings.

Next, I replicate my tests of accrual reversals using an alternative definition of accrual errors based on the modified Jones model (Dechow et al., 1995). Beginning with the final sample of 6,571 firm-years used in my accrual reversals tests, I further require the availability of modified Jones accrual errors, and use these in the propensity-matching procedure and in computing the accrual error quintiles (q_acc_{t-1} and q_acc_t) used in the analyses.[27] I find that the accrual errors used in my main tests and accrual errors based on the modified Jones model are highly correlated, with Pearson correlations of 81 percent for current-year accruals and 80 percent for previous-year accruals. I find that my primary inferences are unchanged under this alternative specification. Specifically, all the coefficients of interest—i.e. the coefficients on $I(q\_acc_{t-1} \geq 4 \times post_t \times treat_t$, $I(q\_acc_{t-1} = 5) \times post_t \times treat_t$, and $q\_acc_{t-1} \times post_t \times treat_t$—remain statistically significant at conventional significance levels and in the hypothesized directions under both the industry-matching and the propensity-matching methods.

[27] As in the methodology in my main tests (see Appendix E), I estimate the modified Jones model with a scaled intercept within industry-years, scaling each term by average assets. I require the industry-years to comprise at least ten observations and use only firm-years with average assets of at least $5 million, in order to reduce the impact of outliers and small sample sizes when estimating the model's coefficients. I also winsorize each term at the top and bottom percentile each industry-year. I then merge the estimated model coefficients for each industry-year back into the data, and compute the accrual estimation error as the component of accruals not explained by the model.

9 Falsification tests

9.1 Alternative case on a different legal issue

I verify that my findings are specific to the *ex post* loss rule by replicating my analyses using an alternative Supreme Court decision that also affected barriers to litigation in the Ninth Circuit, but that concerned a different legal issue. A finding that barriers to litigation are positively associated with delays to downward corrections in this setting would suggest an alternative interpretation of my results: that the increase in delaying after *Dura* may be attributable to the increase in barriers to litigation, independent of the adoption of the *ex post* loss rule.

I carry out this falsification test using *Tellabs, Inc. v. Makor Issues & Rights, Ltd.*, 551 U.S. 308 (2007). *Tellabs* is an ideal setting because it affected barriers to litigation in the Ninth Circuit, but concerned the approach for pleading scienter, a separate issue from the *ex post* loss rule and loss causation.[28] Choi & Pritchard (2012) found that *Tellabs* had the "greatest impact" on the Ninth Circuit: "scienter-based dismissal decreased in the Ninth Circuit after the *Tellabs* decision relative to other circuits" (p. 877). In addition, using *Tellabs* allows me to carry out the falsification tests within the post-SFAS 142 and post-Sarbanes Oxley Act regulatory environments.

I replicate my main analyses (Tables 6, 7, 8, 9, and 10) starting the post-period in 2008, but with otherwise identical methodology. I document the results at Table 11, tabulating only the main coefficients for brevity. Because *Tellabs* *decreased* barriers to litigation in the Ninth Circuit, finding a decline in delaying of write-downs and downward accrual reversals in the Ninth Circuit relative to control circuits after *Tellabs* would suggest that the alternative interpretation is important in explaining my findings. Columns 3 and 6 of Table 11 document the estimated coefficients on the triple interactions. All are smaller in magnitude to my main results, and are statistically insignificant at conventional significance levels, or, in one case, significant but in the wrong direction to the result that would support the alternative interpretation.

9.2 Firms not in high-litigation industries

The motivation of this paper is that the *ex post* loss rule provides managers with the ability to reduce or avoid losses from securities litigation by delaying corrective disclosures. The rule

[28] Scienter and loss causation are two of the six different elements in a securities case, as spelled out by the Supreme Court in *Dura*: "(1) a material misrepresentation or omission; (2) scienter (a wrongful state of mind); (3) a connection with the purchase or sale of a security; (4) reliance [...]; (5) economic loss [...]; and (6) loss causation" (p. 580, internal quotation marks omitted).

Table 11: Main tests with an alternative Supreme Court decision

Matching:	Industry-matched			Propensity-matched		
Period:	Pre	Post	DID	Pre	Post	DID
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Odds of writing down (logistic; dependent variable: wdd_t)						
$abtmd_{t-1} \times treat_t$	**0.010**	**−0.184**	0.013	**−0.314**	**0.032**	−0.294
	(0.485)	(0.273)	(0.482)	(0.502)	(0.272)	(0.494)
$abtmd_{t-1} \times post_t \times treat_t$			**−0.166**			**0.205**
			(0.550)			(0.560)
Observations	2,685	2,340	5,025	2,640	2,305	4,945
McFadden R^2	0.065	0.112	0.071	0.070	0.103	0.064
Panel B: Level of write-downs (least squares; dependent variable: wd_t)						
$abtm_{t-1} \times treat_t$	**−0.052**	**0.021**	−0.049	**−0.053**	**−0.005**	−0.076
	(0.032)	(0.041)	(0.049)	(0.033)	(0.041)	(0.051)
$abtm_{t-1} \times post_t \times treat_t$			**0.067**			**0.073**
			(0.059)			(0.062)
Observations	603	657	1,260	538	620	1,158
Adjusted R^2	0.471	0.480	0.473	0.564	0.517	0.516
Panel A: Accrual reversals, top two quintiles (logistic; dependent variable: $I(q\_acc_t = 1)$)						
$I(q\_acc_{t-1} \geq 4) \times treat_t$	**−0.276**	**−0.439****	−0.255	**−0.128**	**−0.654*****	−0.094
	(0.199)	(0.212)	(0.196)	(0.201)	(0.218)	(0.199)
$I(q\_acc_{t-1} \geq 4) \times post_t \times treat_t$			**−0.220**			**−0.647****
			(0.291)			(0.294)
Observations	3,179	2,713	5,892	3,148	2,666	5,814
McFadden R^2	0.126	0.093	0.107	0.127	0.108	0.112
Panel B: Accrual reversals, top quintile (logistic; dependent variable: $I(q\_acc_t = 1)$)						
$I(q\_acc_{t-1} = 5) \times treat_t$	**−0.218**	**−0.288**	−0.213	**−0.241**	**−0.561****	−0.240
	(0.226)	(0.242)	(0.221)	(0.229)	(0.248)	(0.226)
$I(q\_acc_{t-1} = 5) \times post_t \times treat_t$			**−0.125**			**−0.391**
			(0.330)			(0.335)
Observations	3,179	2,713	5,892	3,148	2,666	5,814
McFadden R^2	0.132	0.097	0.112	0.134	0.115	0.120
Panel C: Accrual reversals, all reversals (least squares; dependent variable: q_acc_t)						
$q\_acc_{t-1} \times treat_t$	**0.048**	**0.046**	0.047	**0.021**	**0.062**	0.043
	(0.036)	(0.039)	(0.036)	(0.035)	(0.040)	(0.036)
$q\_acc_{t-1} \times post_t \times treat_t$			**0.004**			**−0.011**
			(0.053)			(0.054)
Observations	3,179	2,713	5,892	3,148	2,666	5,814
Adjusted R^2	0.053	0.039	0.042	0.061	0.049	0.043

This table shows the results from replicating the main tests of this paper using *Tellabs* instead of *Dura* but with otherwise identical methodology. Specifically, the results documented at Tables 6, 7, 8, 9, and 10 are replicated in Panels A to E respectively. I tabulate only the key coefficients for brevity. Variable definitions are at Appendix D. Standard errors are shown in parentheses and the p-values are labeled as follows: *p<0.1; **p<0.05; ***p<0.01.

should therefore only affect companies with sufficient securities litigation risk: a hypothetical firm that has a negligible probability of facing litigation would have no incentive to take a costly action to avoid litigation.[29] Finding a similar or stronger result for firms in industries with lower litigation risk would therefore call my interpretation of my main results into question.

I carry out this falsification test by replicating my main analyses (Tables 6, 7, 8, 9, and 10) using non-financial firms that are not in the set of high-litigation industries that are the focus of this study, but with otherwise identical methodologies.[30] I leave the results untabulated for brevity. Under both industry matching and propensity matching, the coefficients on the triple interaction terms are statistically insignificant at conventional significance levels or in the opposite direction from the corresponding coefficients in my main tests. These results suggest that there is no evidence that the *ex post* loss rule affects firms not in highly-litigated industries.

10 Concluding remarks

> *"While* Dura*'s transformative effect has been undeniable, it was only the beginning of the evolution of the significance of loss causation to the application of securities laws."*
>
> (Sullivan et al., 2010)

Under the *ex post* loss rule, plaintiffs in securities lawsuits are required to show that the firm's alleged misconduct caused a corrective disclosure that resulted in a stock price decline. Firms at high risk of litigation would therefore be incentivized to avoid or delay bad news in the hopes of a turnaround or the occurrence of other news that can be bundled with the bad news (see Spindler, 2007 and Bliss et al., 2018). My overall research question is whether this applies to the delaying of income-decreasing accounting choices, which would demonstrate a fundamental link between corrective disclosures in the context of securities litigation, and downwards corrections of financials as required by GAAP.

My research design exploits the fact that the Supreme Court's decision in *Dura* caused the

[29] Delaying corrective disclosures is potentially costly; for example because they increase potential damages should the firm be sued eventually (see Section 3.3).

[30] The high-litigation industries are the biotechnology (SIC codes 2833–2836 and 8731–8734); computer manufacturing and software (SIC 3570–3577 and 7370–7374); electronics manufacturing (SIC 3600–3674); and telecommunications and electric services (SIC 4810–4813, 4911, and 4931) industries. Please see Appendix C for further discussion, including details on industry representation in securities lawsuit filings prior to *Dura*, and the probability of securities litigation by industry. Financial firms are defined as firms with SIC codes beginning with six.

ex post loss rule to be adopted in the Ninth Circuit, when it was already the prevailing legal standard in the majority of the circuits. I find that after *Dura*, firms under the jurisdiction of the Ninth Circuit and in high-litigation industries became more likely to delay GAAP-required write-downs and income-decreasing reversals of working capital accrual estimation errors at the firm level, relative to matched control firms. This suggests that the current loss causation standard in securities litigation affects income-decreasing accounting choices.

In addition, I find that when write-downs do occur, they are larger after *Dura* relative to the firm's beginning book-to-market ratio, in treatment firms relative to control firms. This is consistent with firms being able to delay write-downs only up to a point, or firms reporting "as much bad news as possible" (Spindler, 2007, p. 684–685) by recording larger write-downs or write-downs to more assets in the same period, once they are unable to delay a write-down. I also find that the impact of *Dura* on income-decreasing working capital accrual error reversals is large enough to drive a significant change in *overall* working capital accrual error reversals after *Dura*. Finally, I carry out two falsification tests: the first uses an alternative Supreme Court decision that also affected litigation in the Ninth Circuit but that did not concern the *ex post* loss rule, and the second uses firms not in high-litigation industries.

This study contributes to the literature on the relationship between the legal environment and accounting, and in particular to the growing literature on the impact of court rulings on financial reporting. I believe that this study is informative to policy makers and legal and accounting practitioners because downward corrections to a company's financials are fundamental to GAAP and are useful for example in the detection of fraud (e.g. Dechow et al., 2012). If, as my findings suggest, the *ex post* loss rule increases incentives to delay income-decreasing accounting choices in highly-litigated industries, then this fundamental feature of the legal environment causes distortions in firms' financial disclosures.

In the years since *Dura*, the *ex post* loss rule and, more generally, loss causation have continued to be areas of active development. While the rule is now the prevailing legal standard across all circuit courts, differences continue to develop from time to time between the circuits in their interpretations of the Supreme Court's decision, for example in the standard by which plaintiffs are required to plead a causal relationship between misconduct and economic loss (see Sullivan et al., 2010). The methods by which plaintiffs show the economic losses—typically based on event studies—have also been called into question in the literature (for example, Torchio, 2009, Kaufman & Wunderlich, 2010, and Baker, 2016), and several Supreme Court decisions subsequent to *Dura* have been related to loss causation to varying degrees and have incrementally refined the doctrine (see, for example, Cox, 2013 and Perry & Conover, 2016).

Amid this rapid development, loss causation is an issue that clearly links securities litigation and financial reporting: financial disclosures can affect prices, and can therefore play key roles in identifying either *ex ante* or *ex post* losses. The goal of this paper is to shed light on a specific aspect of this link—the relationship between the prevailing *ex post* loss rule and the delaying of income-decreasing accounting corrections. More broadly, however, the continually-developing legal environment and the relationship between litigation and the incentives of managers, shareholders, and other stakeholders make the intersection of law and accounting a rich field for further research.

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Appendix A Summary of *Broudo* and *Dura*

Broudo v. Dura Pharmaceuticals, 339 F.3d 933 (9th Cir. 2003), henceforth *Broudo*, was a securities case heard in the Ninth Circuit that concerned statements made by Dura Pharmaceuticals, Inc. during the class period April 15, 1997 to February 24, 1998. The Ninth Circuit ruled in favor of the plaintiff investors, holding that loss causation is established if plaintiff show "that the price on the date of purchase was inflated because of the misrepresentation" (p. 934). The defendants appealed, and in *Dura Pharmaceuticals v. Broudo*, 544 U.S. 336 (2005), henceforth *Dura*, the Supreme Court overturned the Ninth Circuit, holding that price inflation is insufficient to establish loss causation.

A.1 *Broudo*

Dura Pharmaceuticals, Inc. issued several press releases during the class period April 15, 1997 to February 24, 1998 disclosing progress on the clinical trials and FDA application of its *Albuterol Spiros* asthma medication delivery device, and indicating favorable sales growth of its *Ceclor CD* antibiotic. However, Dura's stock price fell 47 percent between February 24 and 25, 1998 when it lowered its revenue and EPS guidance due in part to poor sales of Ceclor CD. Dura also disclosed several months later that its Albuterol Spiros device failed to receive FDA approval.

Investors sued, alleging that the firm and the individual defendants knew that the statements issued during the class period were untrue. The district court dismissed the lawsuits in 2000 and 2001 on the basis of loss causation, because the price decline in February 1998 was unrelated to the Albuterol Spiros device; and on the basis of scienter, because it found insufficient evidence that the statements about Ceclor CD during the class period were false or made in the knowledge that they were false.[31] The plaintiffs appealed to the Ninth Circuit.

In *Broudo*, the Ninth Circuit reversed the district court's ruling. The Ninth Circuit generally agreed with the district court's conclusions on scienter, but disagreed with its conclusions concerning loss causation. The court said that

> "for a cause of action to accrue, it is not necessary that a disclosure and subsequent drop in the market price of the stock have actually occurred, because *the injury occurs at the time of the transaction*." (p. 935, emphasis mine)

Accordingly, the Ninth Circuit ruled that the plaintiffs succssfully pled loss causation, because they did plead that Dura Pharmaceutical's stock price was inflated due to the mis-

[31] See *In re Dura Pharmaceuticals, Inc. Securities Litigation,*, 2000 U.S. Dist. LEXIS 15258 (S.D. Cal. 2000) and *In re Dura Pharmaceuticals, Inc. Securities Litigation,*, 2001 U.S. Dist. LEXIS 25907 (S.D. Cal. 2001).

representations concerning the Albuterol Spiros device. The defendants appealed to the Supreme Court.

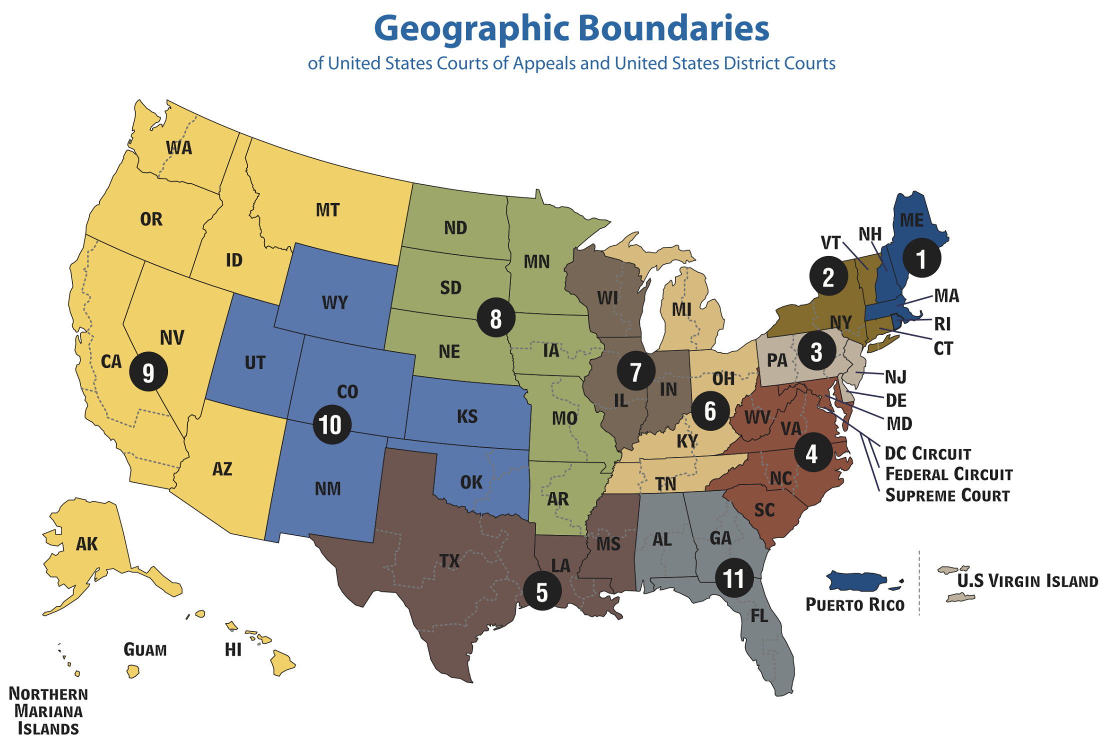
A.2 *Dura*

In *Dura*, the Supreme Court identified a split in the circuit courts' interpretations of loss causation, finding that the Ninth Circuit's interpretation was unique: "other Courts of Appeals [...] have rejected the inflated purchase price approach to showing loss causation." (p. 338)

The Supreme Court reversed the Ninth Circuit, holding that it is insufficient for investors to allege that the firm's stock price was inflated at the time of purchase. Among other reasons, the Supreme Court said that an objective of securities law is to protect investors against economic losses caused by misrepresentations. Because an inflated stock price is not a relevant economic loss, the Ninth Circuit's interpretation "would allow recovery where a misrepresentation [...] does not proximately cause any economic loss." (p. 338)

The Supreme Court also expressed agreement with the majority view that a price decline is required to establish loss causation, noting the plaintiffs' "failure to claim that Dura's share price fell significantly after the truth became known" (p. 347).

Appendix B Circuit court jurisdictions



This map, reproduced in full from United States Courts (n.d.), shows the jurisdiction of each U.S. Court of Appeal. The Ninth Circuit, which is the treatment jurisdiction in this paper, comprises firms located in Alaska, Arizona, California, Guam, Hawaii, Idaho, Montana, Nevada, the Northern Mariana Islands, Oregon, or Washington.

Appendix C High-litigation industries

In my main analyses I focus on firms in highly-litigated industries as of the time of *Dura*. I define high-litigation industries using a combination of (1) industry code ranges from the prior literature, and (2) an examination of securities lawsuit filings prior to *Dura*.

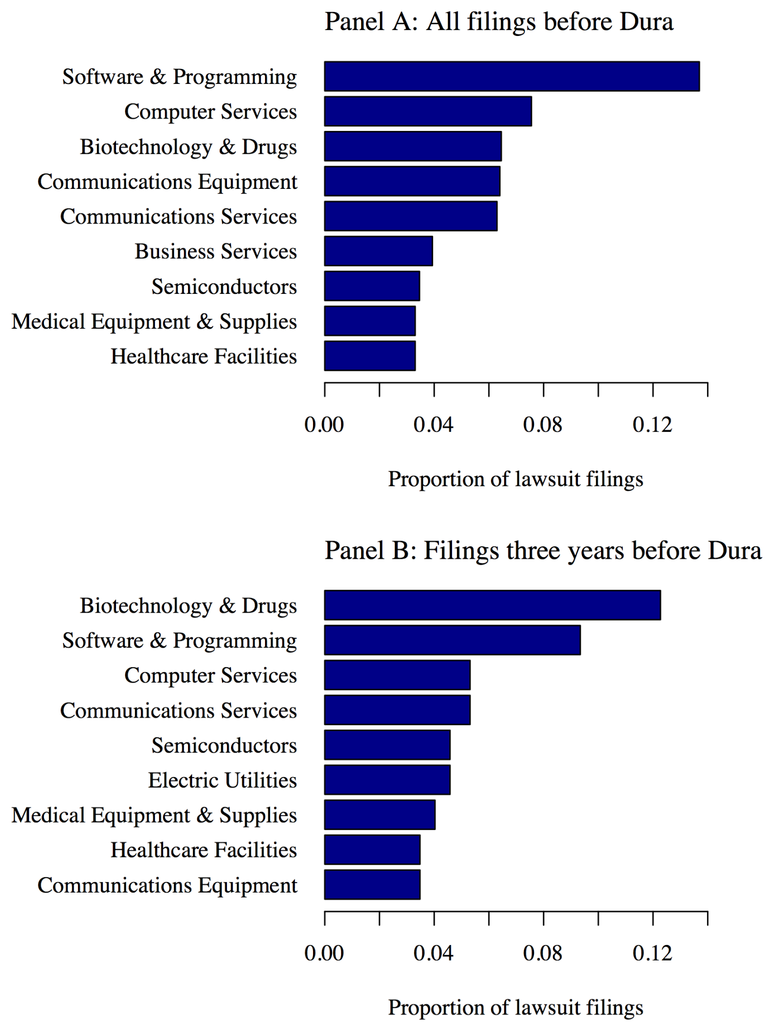
Figure C.1 documents the share of securities lawsuits filed against firms in non-financial industries in two time intervals before *Dura*, focusing on industries with the highest shares—at least three percent—within each period. Panel A is based on lawsuits filed from the start of the SCAC's coverage (1996) to the day before the *Dura* decision ($N = 1,906$), while Panel B is based on lawsuit filings during the three years prior to the decision ($N = 546$). The data and industry classifications are retrieved from Stanford Law School's Securities Class Action Clearinghouse (SCAC) database.

The industries with the highest representation correspond closely to categorizations of high-litigation industries used in the the prior literature. While there is some variation between studies, many classify the biotechnology (SIC codes 2833–2836 and 8731–8734); computer manufacturing and software (SIC 3570–3577 and 7370–7374); and electronics manufacturing industries (SIC 3600–3674) as high-litigation industries, all of which overlap closely with the industries in Figure C.1 (see, for example, Francis et al., 1994; Kasznik & Lev, 1995; Field et al., 2005; Krishnan et al., 2011; Kim & Skinner, 2012; and Donelson & Hopkins, 2016). I use these industries in my definition of high-litigation industries, and include telecommunications and electric services (SIC 4810–4813, 4911, and 4931) because, as shown in Figure C.1, they are highly represented in the sample of securities lawsuits prior to *Dura*.

Table C.1 examines whether firms in the selected high-litigation industry groups have significantly higher risk of facing securities lawsuits than firms not in the high-litigation industries, in two time intervals. I examine the proportion of firms in each industry group that were sued within a three-year period beginning with the focal year. In Panel A I examine focal years between 1996 and 2003 because the SCAC's coverage begins in 1996, and because 2005 is the final year of the pre-*Dura* period in my main analyses. In Panel B I restrict the data to the final three years of the period, 2001–2003, to examine a period with greater proximity to *Dura*. As in my main tests, I require the firms to be headquartered (Compustat: *loc*) in the United States and located in a state (*state*) under the treatment or control jurisdictions, and I omit firms with beginning total assets (*at*) less than \$1 million. I also omit financial firms with SIC codes beginning with six.

Table C.1 documents that the firms in each of the selected high-litigation industries are significantly more likely to be sued than firms in non-high-litigation industries, in both of the time intervals examined. For example, from Panel B (covering 2001 to 2003), biotechnology

Figure C.1: Industry representation in lawsuit filings pre-*Dura*



Panels A and B illustrate the share of securities lawsuits filed against firms in non-financial industries in two periods before *Dura* respectively, focusing on industries with at least a three percent share within each period. The lawsuits data is obtained from Stanford Law School's Securities Class Action Clearinghouse (SCAC) database, and the industry names are as defined by the SCAC. I omit industries with the words "investment", "bank", "financial", "insurance", or "fund" in their names. Panel A is based on lawsuits filed from the start of the SCAC's coverage to the day before the *Dura* decision ($N = 1,906$), and Panel B is based on lawsuit filings during the three years prior to the decision ($N = 546$). The SCAC's coverage began in 1996.

firms have a 7.33 percent probability of being sued within a 3-year period, compared to 3.11 percent for firms not in high-litigation industries.[32]

Table C.1: Probability of litigation by industry

Panel A: Firm-years between 1996 and 2003

	Biotech	Computers	Electronics	Telecoms	Other	Overall
No. of firm-years	3,386	6,924	3,158	2,351	32,404	48,223
Proportion sued	4.84%	8.88%	6.43%	4.47%	2.64%	4.03%
χ^2 vs. Other	53.48	616.06	142.43	27.02	–	–
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	–	–

Panel B: Firm-years between 2001 and 2003

	Biotech	Computers	Electronics	Telecoms	Other	Overall
No. of firm-years	1,268	2,360	1,071	823	10,627	16,149
Proportion sued	7.33%	8.86%	7.56%	6.20%	3.11%	4.73%
χ^2 vs. Other	59.08	160.54	57.03	22.70	–	–
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	–	–

This table compares the probability of litigation in Compustat firm-years in high-litigation industry groups with that of other firm-years. I document the proportions of firm-years in each industry group that were sued within the three years beginning with the focal year. In Panel A I examine Compustat firm-years between 1996 and 2003, and in Panel B I examine Compustat firm-years between 2001 and 2003. I require the firms to be headquartered (Compustat: *loc*) in the United States and located in a state (*state*) under the treatment or control jurisdictions. Treatment firms are located in the jurisdiction of the Ninth Circuit, while control firms are located in the jurisdictions of circuits other than the Eighth or Ninth Circuits. I omit firms with beginning total assets (*at*) less than $1 million, and financial firms with SIC codes beginning with six. The four high-litigation industry groups are: biotechnology (SIC codes 2833–2836 and 8731–8734); computer manufacturing and software (SIC 3570–3577 and 7370–7374); electronics manufacturing (SIC 3600–3674); and telecommunications and electric services (SIC 4810–4813, 4911, and 4931). I obtain lawsuit filings data from Stanford Law School's Class Action Clearinghouse database. The p-values of the chi-squared tests are based on Monte Carlo simulations (Hope, 1968) with 10,000 replicates.

[32]Several studies classify retail firms as high-litigation (e.g. Francis et al., 1994; Kim & Skinner, 2012) while others do not (e.g. Kasznik & Lev, 1995; Cohen & Zarowin, 2010). I do not include these firms: in untabulated extensions of Panels A and B of Table C.1, I do not find statistically significant evidence that retail firms (SIC 5200–5961) have greater probabilities of securities litigation than other firms not in highly-litigated industries, during both pre-*Dura* periods (p-values 12.1 percent and 26.5 percent respectively).

Appendix D Variable definitions

Table D.1: Variable definitions

Variable	Definition
$abtm_{t-1}$	Total assets (at) at the end of $t-1$ divided by the market value of equity plus total assets minus common equity ($csho \times prcc\_f + at - ceq$) at the end of $t-1$.
$abtmd_{t-1}$	An indicator variable equal to one if $abtm_{t-1} > 1$, and zero otherwise.
acc_t	The component of working capital accruals at t that is attributable to estimation error. See Appendix E for details on the construction of this variable.
$ebtm_{t-1}$	Common equity (ceq) at the end of $t-1$ divided by the market value of equity ($csho \times prcc\_f$) at the end of $t-1$.
$growth_t$	Change in total revenue ($revt$) during t scaled by total revenue during $t-1$.
int_{t-1}	Total intangible assets ($intan$) at the end of $t-1$ scaled by total assets (at) at the end of $t-1$.
log_age_t	The natural logarithm of firm age, defined as t minus the first fiscal year at which the firm appears in Compustat plus one.
log_at_{t-1}	The natural logarithm of total assets (at) at end of $t-1$.
log_mv_{t-1}	The natural logarithm of the market value of equity ($csho \times prcc\_f$) at the end of $t-1$.
$post_t$	An indicator variable equal to one if t is after 2005, and zero otherwise.
q_acc_t	The quintile of acc_t relative to its distribution each year.
roa_t	Income before extraordinary items (ib) during t scaled by average assets (at) during t.
$treat_t$	An indicator variable equal to one if the firm is located in a state ($state$) under the jurisdiction of the Ninth Circuit at t, and zero otherwise. See Appendix B for a map of states by legal jurisdiction.
wd_t	The level of write-downs during t ($wdp + gdwlip$) scaled by the market value of equity ($csho \times prcc\_f$) at the end of $t-1$. Larger write-downs are coded as more negative values.
wdd_t	An indicator variable equal to one if the firm took a write-down at t ($wdp < 0$ or $gdwlip < 0$), and zero otherwise.

Unless otherwise stated, variables in parentheses refer to Compustat Fundamentals Annual variable names.

Appendix E Accrual estimation errors

In my accrual reversals tests I use acc_t, a proxy for the estimation error component of working capital accruals. I construct this variable using a method similar to Allen et al. (2013), who use the Dechow & Dichev (2002) model as modified by Bushman et al. (2012) to decompose accruals into "good accruals"—the component explained by growth and temporary fluctuation in working capital requirements—and accrual estimation errors.

I model the non-error component of working capital accruals at t as a function of operating cash flows at $t-1$, t, and $t+1$, employee growth at t, and sales growth at t, and include a scaled intercept:

$$\begin{aligned}\frac{wca_t}{\bar{a}_t} =&\beta_0 \frac{1}{\bar{a}_t} + \beta_1 \frac{cfo_{t-1}}{\bar{a}_t} + \beta_2 \frac{cfo_t}{\bar{a}_t} + \beta_3 \frac{cfo_{t+1}}{\bar{a}_t} \\ &+ \beta_4 \frac{\Delta emp_t}{emp_{t-1}} + \beta_5 \frac{\Delta rev_t}{rev_{t-1}} + \epsilon\end{aligned} \tag{6}$$

where wca_t is working capital accruals (Compustat: $\Delta(act - che - lct + dlc + txp)$) during t; cfo_t is operating cash flow ($oancf$) during t; emp_t is the number of employees (emp) at t; rev_t is total revenue ($revt$) during t; and $\bar{a}_t$ is average of the starting and ending total assets (at) at t.

I estimate the coefficients of the model every industry-year, where industries are defined according to two-digit SIC codes. To reduce the impact of outliers and small sample sizes on the coefficient estimates, when estimating the coefficients I use only firm-years with average assets greater than \$5 million, require at least ten observations each industry-year, and winsorize each term at the top and bottom percentiles each industry-year.

I then merge the estimated coefficients $\hat{\beta}_i$ for each industry-year back into the data, and compute the accrual estimation error as the component of working capital accruals not explained by the model:

$$\begin{aligned}acc_t = \frac{wca_t}{\bar{a}_t} -&(\hat{\beta}_0 \frac{1}{\bar{a}_t} + \hat{\beta}_1 \frac{cfo_{t-1}}{\bar{a}_t} + \hat{\beta}_2 \frac{cfo_t}{\bar{a}_t} + \hat{\beta}_3 \frac{cfo_{t+1}}{\bar{a}_t} \\ &+ \hat{\beta}_4 \frac{\Delta emp_t}{emp_{t-1}} + \hat{\beta}_5 \frac{\Delta rev_t}{rev_{t-1}})\end{aligned} \tag{7}$$