



Information bundling and securities litigation

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ABSTRACT

We exploit the exogenous shock of a 2005 U.S. Supreme Court decision on securities class action loss causation requirements to examine two ways that firms bundle information with restatements: “positive bundling” of good news and “noise bundling” of additional bad news. We find that positive bundling offsets price declines and results in less litigation. In contrast, noise bundling magnifies price declines, but nevertheless deters litigation by confounding which bad news caused a decline. Non-bundled restatements are 5.94 times more likely to result in litigation. Bundled restatements have 8.17 times higher dismissal rates and \$21.17 to \$23.45 million lower settlement amounts.

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1. Introduction

Empirical evidence suggests that some disclosure practices deter litigation (Field et al., 2005), while others prompt litigation (Francis et al., 1994; Johnson et al., 2007; Skinner, 1997). Spindler (2007) and Ferrell and Saha (2007) theorize that information bundling might reduce the costs of securities litigation. We examine whether bundling restatement announcements with other information makes it more difficult for plaintiffs to establish loss causation, a crucial element in securities class action lawsuits, thereby reducing firms' litigation costs.¹

This question about the relationship between information bundling and securities litigation arises from two important literatures. First, managers use information bundling in various contexts. For example, managers bundle earnings forecasts and announcements (Billings et al., 2015; Rogers and Van Buskirk, 2013; Wasley and Wu, 2006), withhold bad news and bury the bad news with subsequent corporate events (Kothari et al., 2009), coordinate the timing of good and bad news releases (Lansford, 2006), and bundle positive earnings guidance with earnings announcements before selling shares (Billings and Cedergrén, 2015). Graham et al., (2005) report that one third of chief financial officers admit to trying to package bad news with other disclosures.

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¹ Loss causation is the requirement that plaintiffs in a shareholder class action must show that shareholders suffered damages, and that these damages were the result of the firm's misstatements or omissions.

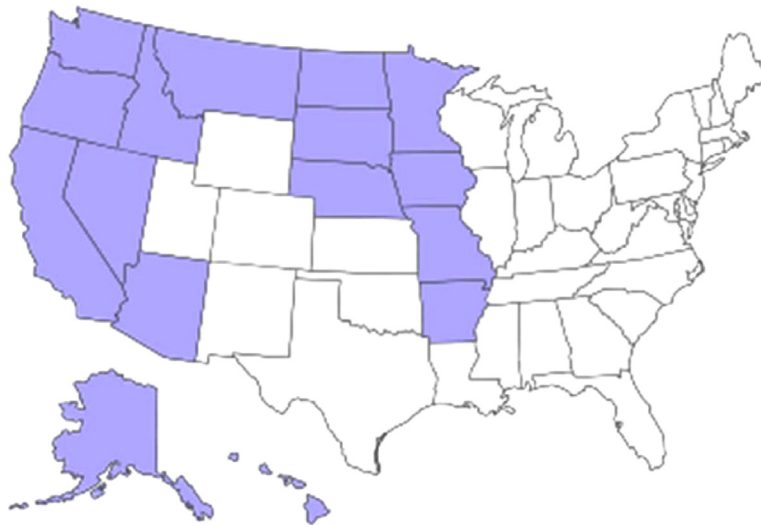


Fig. 1. Eighth and Ninth Circuits of the U.S. Courts of Appeals.

The states in the Eighth and Ninth Circuits of the U.S. Courts of Appeals are shaded on the map. The Eighth Circuit includes Arkansas, Iowa, Minnesota, Missouri, Nebraska, North Dakota, and South Dakota. The Ninth Circuit includes Alaska, Arizona, California, Hawaii, Idaho, Montana, Nevada, Oregon, and Washington. Non-shaded states belong to Circuits other than the Eighth and Ninth Circuits.

A second literature examines practices that reduce the costs of securities litigation. [Skinner \(1994\)](#) finds that managers voluntarily warn of bad news due to fear of legal liability. Several studies have documented that managers vary how information is disseminated to reduce litigation risk, such as accelerating the release of bad news or giving ‘warnings’ ([Billings and Cedergrén, 2015](#); [Donelson et al., 2012](#); [Field et al., 2005](#)), disclosing bad news in footnotes instead of more prominent portions of disclosures ([Bloomfield, 2002](#); [Files et al., 2009](#); [Hirshleifer and Teoh, 2003](#)), or failing to disclose entirely ([Marinovic and Varas, 2014](#); [Rogers and Van Buskirk, 2009](#)).²

We examine a specific intersection of these two literatures: the use of information bundling to reduce the costs of securities litigation by making it more difficult for shareholder plaintiffs to establish loss causation. At the outset, it is important to note litigation has both direct and indirect costs, including significant declines in the firm’s stock price ([Bhattacharya et al., 2007](#); [Gande and Lewis, 2009](#)), attorney fees, managerial time and focus, reputational costs ([Karpoff and Lott, 1993](#); [Karpoff et al., 2008](#)), and the potential to erode managers’ information advantage over shareholders as information becomes available when a lawsuit proceeds beyond the early stages ([Haslem, 2005](#)). The direct costs of settlement values alone are substantial: billions in aggregate per year ([Zingales, 2007](#)). Furthermore, managers often face increased discipline from takeovers, reduced pay, and potential loss of employment after litigation ([Humphrey-Jenner, 2012](#)).

Loss causation is a particularly important element in securities litigation. According to the courts, loss causation is established in three parts: “(1) identifying a ‘corrective disclosure’ (a release of information that reveals to the market the pertinent truth that was previously concealed or obscured by the company’s fraud); (2) showing that the stock price dropped soon after the corrective disclosure; and (3) eliminating other possible explanations for this price drop, so that the fact finder can infer that it is more probable than not that it was the corrective disclosure—as opposed to other possible depressive factors—that caused at least a ‘substantial’ amount of the price drop” (*FindWhat Investor Group v. Findwhat.com*, 658 F.3d 1282, 1311–12 (11th Cir. 2011)).

Before the 2005 U.S. Supreme Court ruling in *Dura Pharmaceuticals v. Broudo*, courts in the Eighth and Ninth Circuits (see [Figure 1](#)), permitted plaintiffs to establish loss causation merely by alleging that a firm’s stock price was inflated at the time of the alleged misstatements or omissions. Other courts, outside the Eighth and Ninth Circuits, disagreed and ruled that a claim of price inflation was insufficient to establish loss causation. Instead, those courts required that plaintiffs must meet higher standards of pleading and proof by establishing a connection between a “corrective disclosure” and a contemporaneous stock price decline. The *Dura* ruling effectively raised the standard for the Eighth and Ninth Circuits to the same level that other courts previously imposed.

We examine how a firm’s release of information can influence the likelihood and cost of litigation, both by impacting the stock price decline and by confounding the facts about what caused that decline. Specifically, we focus on two distinct types of information bundling. First, we examine “positive bundling,” a disclosure practice that adds good news to bad news. [Thaler \(1985\)](#) labels this the “silver lining” approach. The release of contemporaneous good news can positively

² Other studies show how firms alter the release of bad news such as disclosing after-market hours ([deHaan, et al., 2015](#); [Patell and Wolfson, 1982](#)), and adding complexity to financial statements to obfuscate poor performance ([Li, 2008](#)).

influence perceptions that would have been negative in isolation. Positive bundling makes securities litigation less attractive by reducing the magnitude of, or even eliminating, the stock price decline associated with a restatement.

Second, we examine “noise bundling,” a disclosure practice that adds bad or confounding news to bad news. For example, firms may engage in a ‘big bath’ by announcing other negative news, such as write downs, along with disappointing earnings (Elliott and Shaw, 1988; Haggard et al., 2015; Levitt, 1998). Noise bundling includes not only bad news, but also confounding news that might or might not be objectively bad but nevertheless complicates the task of establishing loss causation. Piling bad or confounding news on top of bad news might seem counterproductive to the extent that it amplifies the magnitude of a stock price decline, and that judges mistakenly view the multiple disclosures more negatively (Donelson and Hopkins, 2016). However, noise bundling may generate countervailing benefits by making it difficult or impossible to unravel how much of the stock price decline is attributable to each piece of bad news and complicate proof of loss causation for plaintiffs.

The literature has mixed evidence on whether disclosure practices induce litigation, or whether litigation risk influences disclosure practices. Our hypothesis is that both types of information bundling reduce the costs of securities litigation by confounding the loss causation requirement. To disentangle the causal effect, we exploit an exogenous shock arising from the 2005 U.S. Supreme Court ruling on loss causation standards to achieve identification in our tests. The *Dura* ruling provides an exogenous source of variation and a random assignment of treatment (Eighth and Ninth Circuits) and control (other Circuits) disclosures. With restatement characteristics, firm characteristics, information endowments, and bundling choices experiencing similar trends across Circuits, the change in loss causation standards provides a relatively clean setting to examine how information bundling affects litigation because the only change is the loss causation requirements for a subset of firms after the *Dura* ruling.

One preliminary challenge associated with testing this hypothesis is that, although information bundling and litigation costs are observable, the degree to which the loss causation requirement is confounded is not observable. Additionally, when a firm issues a restatement, the decision to bundle may not be completely random or exogenous. Restatements that are bundled may be, on average, systematically different from stand-alone restatements. Firms that bundle their restatements may be different in various dimensions, including their endowments of good and bad news. To address this challenge, we employ several empirical strategies including quasi-experimental techniques (difference-in-difference; difference-in-difference-in-difference), falsification tests, and matching analyses.

In our setting, we use 1562 restatements from 2003–2009 as the “corrective disclosures” in part (1) of the loss causation test described above. We analyze the effects of information bundling on the stock price reaction to each restatement, part (2) of the loss causation test. We then consider the extent to which information bundling makes it more difficult to establish part (3) of the test, by introducing other potential causes of the stock price reaction.

The average CAR[−1,1] for positive bundled restatements is 2.27%, compared to −2.74% for non-bundled restatements. Although we cannot observe the specific processes within plaintiffs’ law firms that lead to the filing of cases, it is reasonable to conclude that a positive stock market reaction deters plaintiffs from suing, because it is more difficult to prove that any significant loss occurred to the shareholders. To the extent positive bundling reduces the magnitude of the stock price decline at the time of the restatement, it reduces damages and makes securities litigation relatively less attractive for plaintiffs.³

In contrast, the average cumulative abnormal return (CAR[1,1]) for noise bundled restatements is −3.67%. Even though noise bundling exacerbates the stock decline, it has the ability to confound the loss causation requirement, relative to restatements made in isolation. As an example, the court dismissed plaintiffs’ claims in *Fener v. Belo* on April 2, 2008, because it could not discern whether the stock price of the defendant declined because of the revelation of information about an alleged fraud or the revelation of other bad news about the firm’s business and industry overall.

Next, we examine the effects of both positive and noise bundling on the incidence of litigation. Our hypothesis is that if information bundling makes securities litigation more difficult by confounding loss causation, then plaintiffs’ lawyers will file fewer cases. We use a triple difference (difference-in-difference-in-difference) test procedure that include the following: (1) the response of litigation to information bundling, (2) the response of litigation to the heightened loss causation standards, and (3) the particular response of litigation in the Eighth and Ninth Circuits versus a control set of restatements that were not subject to a shift in loss causation standards. Our main variable of interest is the interaction of these three: litigation occurrence for bundled restatements that were subject to the exogenous shock of heightened loss causation standards in the Eighth and Ninth Circuits.

Triple difference, difference-in-difference, and matching analyses confirm that information bundling significantly reduced litigation occurrence for firms in the Eighth and Ninth Circuits after the *Dura* ruling. Specifically, the occurrence of litigation dropped from 6.7% to 0% for positive bundled restatements, and from 20.3% to 5.6% for noise bundled restatements after the tightening of loss causation standards. The heightened loss causation standards effectively made non-bundled restatements 5.94 times more likely to face litigation than bundled restatements in the Eighth and Ninth Circuits.

For cases outside the Eighth and Ninth Circuits, difference-in-difference analysis shows no significant difference in litigation occurrence for bundled and non-bundled restatements. In other words, the control group was unaffected by the *Dura*

³ We also contacted lawyers from the three leading law firms, based on league tables from the relevant period, and asked for a description of “how (if at all) having multiple simultaneous disclosures makes litigation more difficult and/or costly.” This unreported survey confirmed the above statistical tests regarding bundling.

ruling, whereas information bundling significantly reduced litigation occurrence for the treatment group. After controlling for industry, restatement magnitude, length of restatement period, firm size, SEC investigation-related restatements, share turnover, and prior 12-month return, our matching estimator indicates that positive and noise bundling were associated with lower litigation occurrence for firms outside the Eighth and Ninth Circuits during both time periods. These findings support the evidence that information bundling discourages shareholder litigation by confounding the loss causation requirement under strict pleading standards. To explain our results, an omitted variable would need to be positively correlated with information bundling, litigation propensity, the timing of the *Dura* ruling, and differentially affect firms in the Eighth and Ninth Circuits (Guay et al., 2016). Although our research setting puts structure and boundaries on the characteristics that would make correlated omitted variables problematic, it is possible that the *Dura* ruling impacted managers' disclosure choices, as well as real choices that could correlate with information bundling.

Finally, we quantify the effects of information bundling on securities litigation outcomes, something that has not previously been done in the literature. Information bundling potentially increases dismissals and reduces settlement values by making it more difficult for plaintiffs to disentangle how much of the stock price decline was caused by a restatement versus other confounding factors. We hypothesize that for shareholder lawsuits that are filed, information bundling leads to higher rates of dismissal in early stages. We further hypothesize that for shareholder lawsuits that survive early-stage motions to dismiss, information bundling leads to lower settlement values on average.

In our subsample of 144 litigated cases, we find that information bundling increased the dismissal rate and reduced settlement values. Difference-in-difference tests indicate that dismissals increased significantly from 44.4% to 53.8% for information bundled restatements for firms located in the Eighth and Ninth Circuits after the *Dura* ruling. In addition, our matching estimator indicates that information bundling effectively lowered settlement amounts by 21.17 to 23.36 million dollars for firms located in the Eighth and Ninth Circuits after the *Dura* ruling. Triple differences (differences-in-differences-in-difference) testing yields similar findings.

Our study supports the theory that information bundling reduces the costs of securities litigation for firms that issue a corrective disclosure. We contribute to the two literatures discussed above – on the timing and packaging of disclosures and on practices that reduce the likelihood and cost of securities litigation – by showing that bundling restatement announcements with other information reduces litigation costs. We are the first to quantify the effects of information bundling on securities litigation, and to document the surprising result that managers of firms disclosing restatements benefit not only from the positive stock price reaction to simultaneous good news, but also from the noise that arises from additional bad or neutral news.

The remainder of the paper is organized as follows. Section 2 describes our sample and provides descriptive statistics. Section 3 presents the empirical findings on litigation occurrence by bundling, and litigation outcome by bundling. Section 4 concludes.

2. Sample and data description

We begin our sample construction by gathering all restatements from Audit Analytics that occurred from January 1, 2003 to December 31, 2009 for U.S. firms, and exclude restatements that results in a positive impact on prior earnings.⁴ Accordingly, studying the link between information bundling and litigation settlement requires the passage of several years after an exogenous change, so that there are sufficient data both before and after the change. We construct our sample so that a sufficient amount of time has passed to enable the vast majority of cases to reach settlement.

In our study, we are deliberately agnostic about whether any particular restatement reveals actual fraud or misconduct. Instead, we are interested in the larger set of restatements that could be *perceived* by plaintiffs and managers as being sufficiently actionable to generate litigation alleging that the firm violated securities laws. One the main concerns behind the *Dura* ruling was that under the “price inflation” standard, plaintiffs’ lawyers could bring a lawsuit too easily, even when they did not have much, or any, evidence of loss causation. Although there are many types of misstatements and subsequent corrections that can trigger litigation, we focus on the corrective disclosures in the form of restatements of financial reports, due to the possible negative implications of those restatements.

We obtain stock price data from the Center of Research in Security Prices (CRSP) and limit our sample to firms with common stock (share code 10 or 11) data available for the year prior to the restatement announcement. We obtain firm level accounting data from Compustat. Earnings and managements’ guidance data are obtained from the Institutional Brokers Estimate Survey (I/B/E/S), and we require firms to have earnings data available within one year of the restatement filing. Repurchase announcements are obtained from SDC Platinum.

Data on securities class action lawsuits filed from 2003–2010 against publicly listed firms are from the Stanford Law School’s Securities Class Action Clearinghouse (SCAC), which contains defendant names, filing dates and venues, stock ex-

⁴ Although Audit Analytics has been criticized recently by Karpoff et al. (2017) for restatement omissions, those criticisms are not relevant to our use of the database. Audit Analytics includes restatements that correct misstated financial statements, whether due to unintentional errors or irregularities, but excludes other financial statement changes. For example, some companies use the term “restatement” when reporting events such as adopting new accounting standards. Audit Analytics eliminates these restatements, as they are not corrections of financial statements filed with the Securities and Exchange Commission that were subsequently found to involve inaccurate accounting or disclosures (Scholz, 2014). Accordingly, Audit Analytics includes precisely the kinds of misstatements we are interested in studying, and excludes precisely the kinds we are not.

changes, and ticker symbols. Dismissal and settlement data from 2003–2015 were provided by Cornerstone Research. To ensure that all of the lawsuits in our sample are related to a restatement, we read through the complaints, and omit those cases that are not relevant to our study. Following prior literature, we use the firm's headquarters to classify each firm into jurisdiction regions (Ferrell and Saha, 2007; Spindler, 2007). States belonging to the Eighth and Ninth Circuits are shaded in Figure 1. Our final sample consists of 1562 earnings restatements for 1189 unique firms.

We split our sample before and after the announcement of the decision in *Dura*. Restatements that occurred from January 1, 2003 to April 18, 2005 are categorized as *Pre-Dura*; restatements that occurred from April 19, 2005 to December 31, 2009 are categorized as *Post-Dura*. We use a start date of 2003 to avoid the wave of 2001–02 accounting scandals and to include restatements after the CEO and CFO financial statement certification mandate, which began in 2002. We use an end date of 2009 so that there has been sufficient time for the vast majority of filed cases either to be dismissed or reach settlement.⁵

In addition to matching each restatement with events located in I/B/E/S, CRSP, and SDC, we manually read all filings on the SEC EDGAR website that take place on the same date as the restatement to ensure that we capture all types of information that is simultaneously disclosed. For an ex-ante approach, we use prior findings in the literature to motivate our bundling categorization, and construct two types of bundling categories, positive bundling and noise bundling. Positive bundling involves managers simultaneously disclosing a restatement along with other information that is likely to be perceived as positive, and might counterbalance, or mute, the stock price decline that would occur based on the restatement-related news alone. In contrast, noise bundling involves managers simultaneously disclosing a restatement along with other information that is likely to be perceived as negative, thus creating noise that may prevent critics from using the stock market reaction as an objective piece of evidence due to the confounding event.

We construct our variable, *Positive Bundled*, as a binary variable equal to one if the restatement was made on the same day as a positive earnings announcement (EPS equal or greater than the mean analyst forecast, or greater than the same fiscal quarter of the previous year if I/B/E/S data is not available) (Ball and Brown, 1968), a dividend increase announcement (Aharony and Swary, 1980), new positive earnings guidance (guidance equal to or greater than the mean analyst forecast) (Waymire, 1984), an announcement of a tender offer (Chatterjee et al., 1995), an announcement of a merger in which the firms is a target (Andrade et al., 2001), an announcement of a special dividend (DeAngelo et al., 2000), positive product development news (Chaney et al., 1991), or a share repurchase announcement (Ikenberry et al., 1995; Grullon and Michaely, 2004).⁶

We construct *Noise Bundled*, as a binary variable equal to one if the restatement was made on the same day as a disappointing earnings announcement (actual EPS was less than the mean analyst forecast, or less than the same fiscal quarter of the previous year if I/B/E/S data is not available) (Ball and Brown, 1968), a dividend reduction announcement (Healy and Palepu, 1988), the dividend ex-date (Campbell and Beranek, 1955), new disappointing earnings guidance (guidance less than the mean analyst forecast) (Waymire, 1984), an announcement of a change in management (Mian, 2001; Gupta and Fields, 2009), announcements related to other lawsuits (Bhagat et al., 1998), registration of SEO (Masulis and Korwar, 1986), an announcement related to new credit arrangements or debt arrangements (Lummer and McConnell, 1989), delisting from exchanges (Sanger and Peterson, 1990), or an announcement of an acquisition (Malmendier and Tate, 2008). For restatements that are bundled with other information but do not have a clear benchmark, such as those bundled with a quarterly or annual filing (10-Q or 10-K), we categorize the restatement as noise bundled.

Firms can disclose multiple pieces of additional news with the restatement, such as issuing a restatement along with an earnings announcement and earnings guidance. Similar to Anilowski et al. (2007), 70.1% of the earnings guidance issued by management in our sample take place along with an earnings announcement. If the restatement is made in isolation through a Form 8-K, an amended quarterly or annual filing (10-K/A, or 10-Q/A), or a press release, we set *Positive Bundled* and *Noise Bundled* equal to zero.

Of the 1562 restatements in our sample, 274 are classified as *Positive Bundled*: 212 are bundled with a positive earnings announcement, 7 are bundled with a dividend increase announcement, 69 are bundled with positive earnings guidance, 1 is bundled with a special dividend announcement, 8 are bundled with merger announcements, 3 are bundled with tender offers, 5 are bundled with positive product development news, and 4 are bundled with share repurchase announcements.⁷

There are 616 restatements categorized as *Noise Bundled*: 372 restatements are bundled with a disappointing earnings announcement, 121 are bundled with disappointing earnings guidance, 203 are bundled with 10-K or 10-Q filings, 14 are bundled with an announcement regarding a change in management, 9 are bundled with credit or convertible debt arrangements, 8 are bundled with other lawsuit announcements, 6 are bundled with acquisition announcements, 2 are bundled with the announcements of dividend decreases, 12 are bundled on dividend ex-dates, 14 are bundled with SEO registration forms, and 5 are bundled with delisting warnings/announcements.

We control for several important restatement characteristics in our analysis. *Restatement Period* is the number of quarters that the restatement period spans. *Restatement Magnitude* is measured as the cumulative impact the restatement has on net income during the restatement period, scaled by total assets. *Reasons for Restatement* is the number of individual reasons stated for restating. *Restatement History* is the number of times the firm has issued a restatement in the past five years.

⁵ A substantial portion of lawsuits filed in 2010 were not yet settled or resolved as of the date of our sample selection.

⁶ We do not count announcement as bundled if the firm suspends or reaffirms guidance in light of the restatement.

⁷ We do not report the number of bundling events as mutually exclusive. For example, a firm can report positive earnings and announce a share repurchase program on the same day.

Table 1

Summary statistics.

This table reports the summary statistics for the firms who issued earnings restatements from 2003–2009. Restatement characteristics include restatement period, restatement history, restatement magnitude, reasons for restatement, regulation restatement, $CAR[-1,1]$ at restatement, an indicator for fraud, an indicator for an SEC investigation (SEC), an indicator for bundling, and an indicator for subsequent litigation after the restatement. Firm characteristics include size, prior 12-month return (BHR), stock return volatility, stock return skew, share turnover, market-to-book, and an indicator if the firm is headquartered in the Eighth and Ninth Circuits. Positive Bundled equals one if the restatement was bundled with additional news that is likely to be perceived as positive, and zero if the restatement was made in isolation. Noise Bundled equals one if the restatement was bundled with additional news that is likely to be perceived as negative or neutral, and zero if the restatement was made in isolation. All continuous variables are winsorized at the 1% and 99% level. Please see the [Appendix A](#) for variable definitions.

Variable	N	Mean	Median	Standard Deviation
Restatement Period	1562	9.946	8.100	8.958
Restatement History	1562	0.476	0.000	0.789
Restatement Magnitude	1562	−0.022	−0.003	0.061
Reasons for Restatement	1562	3.649	3.000	1.745
Regulation Restatement	1562	0.153	0.000	0.360
$CAR[-1,1]$ at Restatement	1562	−0.022	−0.010	0.088
Bundled (All)	1562	0.573	1.000	0.494
Noise Bundled	1562	0.394	0.000	0.495
Positive Bundled	1562	0.175	0.000	0.473
Size	1562	6.390	6.324	1.833
Fraud	1562	0.024	0.000	0.152
SEC	1562	0.090	0.000	0.286
BHR	1562	0.011	−0.081	0.525
Volatility	1562	0.031	0.026	0.018
Skew	1562	0.160	0.291	1.507
Share Turnover	1562	0.757	0.844	0.246
Market-to-Book	1547	2.665	1.957	4.243
Firms in 8th & 9th Circuits	1562	0.328	0.000	0.469
Litigation	1545	0.093	0.000	0.292

Due to changes in accounting regulation regarding leases and option dating (Scholz, 2014), we create an indicator variable, *Regulation Restatement*, that equals one if the primary reasons for restatement is lease related in 2005 and option dating in 2006. In addition to restatement characteristics, we control for several firm characteristics in our analysis such as firm size, market-to-book, average share turnover, and prior stock return performance. Please see [Appendix A](#) for a full description of our variables.

[Table 1](#) reports summary statistics. The average restatement period spans 9.9 quarters and has 3.6 reasons for restating. The mean restatement magnitude is −2.2% of total assets, while the median is just −0.3%, indicating that some restatements are relatively large while others have little impact on assets. Only 2.4% of the restatements are fraud related (*Fraud*), 9% of the restatements in the sample are related to an SEC investigation (*SEC*), and 15.3% of the restatements are related to regulation changes (*Regulation Restatements*). These results are consistent with [Hennes et al. \(2008\)](#), which document that many restatements are a result of minor accounting issues rather than fraud and misconduct. However, the average cumulative abnormal return surrounding a restatement ($CAR[-1,1]$) is −2.2%, indicating that a restatement announcement generally has a negative impact on a firm's share price.

3. Empirical results

3.1. Influence of information bundling on stock market reactions to restatements

[Table 2](#) examines restatement characteristics by bundling category to determine any significant differences between bundled and non-bundled restatements. For positive bundled restatements, the initial market reaction ($CAR[-1,1]$) is 2.2%, which is significantly different (p -value = 0.000) from the −2.7% market reaction of non-bundled restatement. This finding provides preliminary support for our hypothesis that the additional information used in positive bundling may mute the stock price decline, or even generate a positive offsetting effect. However, the restatement magnitude of the positive bundled restatements is of lesser magnitude than non-bundled restatements (p -value = 0.002), which may explain the differential in the cumulative abnormal returns. We find no significant difference in the length of the restatement period, the number of reasons for the restatement, or proportion of regulation-induced restatements.

For noise bundled restatements, the initial market reaction ($CAR[-1,1]$) is −3.7%, a significantly sharper decline (p -value = 0.079) than the −2.74% market reaction of the non-bundled restatements. Despite the market reaction being larger

Table 2

Restatement characteristics by bundling category.

Table 2 reports restatement characteristics by bundling category for restatements from 2003–2009. Positive Bundled equals one if the restatement was bundled with additional news that is likely to be perceived as positive, and zero if the restatement was made in isolation. Noise Bundled equals one if the restatement was bundled with additional news that is likely to be perceived as negative or neutral, and zero if the restatement was made in isolation. Please see [Appendix A](#) for full variable description. The test for a difference in the mean across the two time periods is conducted by calculating a two-tail t-statistic with standard errors clustered by industry (48-classification). *, **, *** indicates significance at the 1%, 5%, and 10% level, respectively.

	N	Bundled	Non-Bundled	T-Statistic (p-value)
Positive Bundled:				
CAR[−1,1] at Restatement	274	0.023	−0.027	8.53*** (0.000)
Restatement Magnitude	274	−0.015	−0.027	3.23** (0.002)
Restatement Period	274	10.896	10.273	0.80 (0.430)
Reasons for Restatement	274	3.817	3.673	1.32 (0.193)
Regulation Restatement	274	0.204	0.159	1.19 (0.239)
Noise Bundled:				
CAR[−1,1] at Restatement	616	−0.037	−0.027	−1.80* (0.079)
Restatement Magnitude	616	−0.020	−0.027	1.41 (0.166)
Restatement Period	616	9.166	10.273	−1.47 (0.148)
Reasons for Restatement	616	3.548	3.673	−1.89* (0.065)
Regulation Restatement	616	0.123	0.159	−1.34 (0.188)

in magnitude, the number of reasons for the restatement is fewer for noise bundled restatements. We find no significant difference in the restatement magnitude, the length of restatement period, and the proportion of regulation induced restatements for noise bundled and non-bundled restatements.⁸ Thus, preliminary results from [Table 2](#) suggest that the additional adverse information associated with noise bundling exacerbates the stock decline at the announcement.

Given that considerable drops in stock prices following restatements often trigger the filing of shareholder lawsuits, we examine the influence information bundling has on the initial market reaction to the restatement announcement. Preliminary results from [Table 2](#) support our hypotheses that positive bundling mutes and even offsets the stock price decline that is often associated with restatement announcements, and that noise bundling can exacerbate the stock decline.⁹ To formally examine the impact of bundling on stock market reaction of the restatement release while controlling for restatement and firm characteristics, we estimate the following OLS regression model:

$$\begin{aligned}
 \text{CAR}[-1, 1]_{i,t} = & \beta_1 (\text{Bundled}) + \beta_2 (\text{Restatement Magnitude}) + \beta_3 (\text{Restatement Period}) \\
 & + \beta_4 (\text{Reasons for Restatement}) + \beta_5 (\text{Restatement History}) + \beta_6 (\text{Regulation Restatement}) \\
 & + \beta_7 (\text{Fraud}) + \beta_8 (\text{SEC}) + \beta_9 (\text{Size}) + \beta_{10} (\text{Share Turnover}) + \beta_{11} (\text{BHR}) + \beta_{12} (\text{CAR}[-15, -1]) \\
 & + \beta_{13} (\text{Volatility}) + \beta_{14} (\text{Market} - \text{to} - \text{Book}) + \text{Industry Fixed Effects} + \text{Year Fixed Effects}
 \end{aligned} \quad (1)$$

where the dependent variable is the cumulative abnormal return over the three-day window centered on the restatement announcement $[-1, 1]$. $\text{CAR}[-1, 1]$ is calculated as the raw stock return minus the CRSP value-weighted market return. We control for restatement magnitude, restatement period, the number of reasons for restatement, restatement history, firm size, share turnover, prior 12-month market-adjusted returns (*BHR*), cumulative abnormal returns leading up to the announcement ($\text{CAR}[-15, -1]$), prior 12-month return volatility, and market-to-book. We also include indicators for SEC investigation-related restatements, fraud-related restatements, and regulation-related restatements. Following [Petersen \(2009\)](#), year and industry fixed effects are included and standard errors clustered at the industry level using Fama-French's 48-industry classification to control for any time-series and cross-sectional error correlations.

Columns 1 and 2 of [Table 3](#) report evidence on the relative market reaction to restatements bundled with positive information compared to non-bundled restatements. [Table 3](#) indicates that the market reaction is 440 to 450 basis points greater for restatement announcements that are bundled with positive information (columns 1 & 2, p-value = 0.000). These findings are consistent with the notion that the release of positive news has the ability to offset the stock price decline related to the restatement announcement after controlling for restatement and firm characteristics. Importantly, the reduction in the magnitude of the stock price decline associated with positive bundling makes it more difficult to establish loss causation.

Columns 3 and 4 of [Table 3](#) report the evidence on the relative market reaction of noise bundled restatements. As expected, noise bundling has a significant negative impact on the market's reaction compared to non-bundled restatements. After controlling for firm and restatement characteristics, columns 3 and 4 of [Table 3](#) indicate that the market reaction is

⁸ In unreported results, there is no significant difference in cumulative abnormal returns ($\text{CAR}[-1, 1]$) across time periods for each bundling category. The average $\text{CAR}[-1, 1]$ is −3.09% before the *Dura* ruling, and −4.05% after the *Dura* ruling for noise bundled restatements. The average $\text{CAR}[-1, 1]$ is 2.56% before the *Dura* ruling, and 2.08% after the *Dura* ruling for positive bundled restatements. These results are robust to Circuit location.

⁹ Given our ex-ante approach to bundling categorization, it is important to note that it is possible for positive (noise) bundled restatements to have negative (positive) cumulative abnormal returns.

Table 3

Stock market reaction to restatement.

This table reports estimates from OLS regressions explaining the market reactions to restatement announcement. The dependent, $CAR[-1,1]$, is calculated as the cumulative abnormal returns from $[-1,1]$ using the CRSP value-weighted return as the benchmark. Columns 1 and 2 report the coefficients for positive bundled restatements. Columns 3 and 4 report the coefficients for noise bundled restatements. Column 5 reports the coefficients for the subsample of noise bundled restatements that excludes restatements that only bundled with 10-Ks or 10-Qs. Positive Bundled equals one if the restatement was bundled with additional news that is likely to be perceived as positive, and zero if the restatement was made in isolation. Noise Bundled equals one if the restatement was bundled with additional news that is likely to be perceived as negative or neutral, and zero if the restatement was made in isolation. Industry fixed effects are included (Fama and French's 48-industry classification). Year fixed effects are calculated as the calendar year of the restatement announcement. All continuous variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are heteroscedasticity-robust and clustered by industry (48-classification). Please see the [Appendix A](#) for variable definitions. *, **, *** indicates significance at the 1%, 5%, and 10% level, respectively.

	Positive Bundled: $CAR[-1,1]$ (1)	Positive Bundled: $CAR[-1,1]$ (2)	Noise Bundled: $CAR[-1,1]$ (3)	Noise Bundled: $CAR[-1,1]$ (4)	Noise Bundled: $CAR[-1,1]$ (5)
Bundled	0.045*** (0.008)	0.044*** (0.008)	-0.012** (0.006)	-0.013** (0.006)	-0.029*** (0.008)
Restatement Magnitude	0.164*** (0.030)	0.152*** (0.032)	0.148*** (0.023)	0.132*** (0.028)	0.145*** (0.042)
Restatement Periods	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
Reasons for Restatement	-0.002 (0.002)	-0.003 (0.002)	-0.004*** (0.001)	-0.004*** (0.001)	-0.005*** (0.002)
Regulation Restatement	0.018 (0.013)	0.020 (0.012)	0.006 (0.008)	0.006 (0.008)	0.011 (0.010)
Restatement History	0.009** (0.004)	0.010** (0.004)	0.005** (0.002)	0.006** (0.002)	0.009*** (0.003)
Fraud	-0.073 (0.049)	-0.074 (0.047)	-0.036*** (0.012)	-0.042*** (0.012)	-0.034** (0.013)
SEC	-0.010 (0.012)	-0.010 (0.012)	-0.014 (0.009)	-0.014 (0.008)	-0.014 (0.009)
Size	0.001 (0.003)	-0.001 (0.003)	0.003 (0.002)	-0.001 (0.002)	-0.001 (0.002)
Share Turnover	-0.028 (0.020)	-0.018 (0.018)	-0.040** (0.015)	-0.030* (0.016)	-0.038** (0.019)
$CAR [-15, -1]$	—	0.106*** (0.039)	—	0.094*** (0.035)	0.112** (0.044)
Volatility	—	-0.245 (0.444)	—	-0.643* (0.353)	-0.669 (0.459)
Market-to-Book	—	-0.002** (0.001)	—	-0.000 (0.001)	-0.000 (0.001)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Constant	-0.036 (0.024)	-0.001 (0.044)	-0.125*** (0.011)	-0.060* (0.033)	-0.044 (0.042)
Observations	946	935	1288	1276	1075
R-squared	0.169	0.189	0.102	0.122	0.156

120 to 130 basis points less than for non-bundled restatements. Although noise bundling exacerbates the stock price decline of the restatement announcement, it nevertheless might deter litigation due to the difficulty of eliminating other possible explanations for the price decline. In column 5 of [Table 3](#), we exclude the restatements that are bundled with 10-K or 10-Qs, which are classified as noise bundled but may not be perceived as negative, but rather serve the purpose of creating non-directional noise, to ensure that our results are not driven by our classification methodology. The exclusion of the non-directional noise produces stronger results in column 5 of [Table 3](#), indicating that the market reaction is significantly lower by 290 basis points for the subsample of noise bundled restatements. Taken together, these results confirm that information bundling significantly impacts the stock return, which in turn, will likely influence litigation risk.

3.2. Litigation propensity and information bundling

We hypothesize that the *Dura* ruling made it more difficult for plaintiffs to prove loss causation when firms use positive or noise bundling in the Eighth and Ninth Circuits. Specifically, under the heightened loss causation standards, positive bundling makes it more difficult for plaintiffs to demonstrate that damages occurred when there is no stock price decline (part 2 of loss causation test), whereas noise bundling makes it difficult to eliminate other possible explanations for the stock price decline (part 3 of the loss causation test). If information bundling indeed made it more difficult for securities litigation plaintiffs to succeed after the *Dura* ruling, we should expect to see a decrease in litigation against firms that bundled the restatement announcement.

Table 4

Litigation occurrence by information bundling category.

Panel A of Table 4 reports the univariate results for the occurrence of litigation for positive bundled and non-bundled restatements. Panel B of Table 4 reports the univariate results for the occurrence of litigation for noise bundled and non-bundled restatements. Litigation equals one if the firm is subject to a class-action securities lawsuit within one year of the restatement, and zero otherwise. If the firm issues multiple restatements within one year and face litigation, we select the restatement within the class-period and/or closest to the filing date (litigation is only used once). The test for differences in the means is conducted by calculating a two-tail t-statistic with standard errors clustered by industry (48-classification). Diff-in-Diff is the differences between the average differences for the two groups of firms between Pre-and Post-*Dura*. *, **, *** indicates significance at the 1%, 5%, and 10% level, respectively.

Panel A: Positive Bundled	Obs.	Bundled: Litigation (1)	Non-Bundled: Litigation (2)	Difference (p-value) (3)	Diff-in-Diff (p-value) (4)
Pre- <i>Dura</i> :	337	0.028	0.118	−0.090** (0.013)	
8th & 9th Circuits	98	0.067	0.088	0.021 (0.494)	
Non-8th & 9th Circuits	239	0.013	0.130	0.117*** (0.011)	
Post- <i>Dura</i> :	599	0.018	0.124	0.106*** (0.000)	
8th & 9th Circuits	206	0.000	0.201	0.201*** (0.001)	−0.180*** (0.000)
Non-8th & 9th Circuits	393	0.026	0.079	0.053** (0.027)	0.064 (0.191)
Panel B: Noise Bundled	Obs.	Bundled: Litigation (1)	Non-Bundled: Litigation (2)	Difference (p-value) (3)	Diff-in-Diff (p-value) (4)
Pre- <i>Dura</i> :	470	0.149	0.118	−0.031 (0.362)	
8th & 9th Circuits	126	0.178	0.088	−0.090* (0.065)	
Non-8th & 9th Circuits	329	0.137	0.130	−0.007 (0.840)	
Post- <i>Dura</i> :	804	0.057	0.124	0.067*** (0.002)	
8th & 9th Circuits	284	0.056	0.201	0.145*** (0.001)	−0.235*** (0.003)
Non-8th & 9th Circuits	520	0.058	0.079	0.021 (0.439)	−0.028 (0.494)

Accordingly, we examine the propensity of litigation surrounding restatements before and after the *Dura* ruling. In Table 4, we present the occurrence of litigation by bundling category. To remove any bias due to changes over time in the factors that are common to the propensity to bundle, we report the difference-in-difference estimates and the corresponding p-values in columns 3 and 4, respectively.

Panel A of Table 4 presents the univariate results on the occurrence of litigation for positive bundled restatements versus non-bundled restatements. For the full sample before the *Dura* ruling, only 2.8% of positive bundled restatements result in litigation, whereas 11.8% of non-bundled restatements result in litigation. In the Pre-*Dura* era, positive bundling did not significantly impact litigation occurrence for firms located in the Eighth and Ninth Circuits, whereas positive bundling was associated with significantly less litigation occurrence for firms located outside the Eighth and Ninth Circuits (1.3% vs. 13.0%; p-value = 0.011). This finding is consistent with the notion that lawsuits were brought in the Eighth and Ninth Circuits even though there was not a decline in stock price at the disclosure announcement because plaintiffs were allowed to allege that the firm's stock price was "inflated" at the time of the misstatement or omission.

After the *Dura* ruling, not a single positive bundled restatement results in litigation for firms located in the Eighth and Ninth Circuits. Given the smaller magnitude decline in stock price, plaintiffs appear to have been unable to satisfy part 2 of the loss causation test (showing a stock price dropped soon after the corrective disclosure) under the heightened loss causation standards. In unreported results, the cumulative abnormal return ($CAR[-1,1]$) is significantly lower for the restatements that are legally pursued after the *Dura* ruling in the Eighth and Ninth Circuits (Post *Dura*: −14.77% vs. Pre-*Dura*: −6.11%; p-value = 0.026).¹⁰ Collectively, these results suggest that the heightened loss causation standards impacted the types of restatements that plaintiff attorneys' pursued.

In contrast, the *Dura* ruling had no impact on the treatment of positive bundled restatements for the control group, firms located outside the Eighth and Ninth Circuits. The occurrence of litigation remains small in both time periods, 1.3% and 2.6%, respectively. For the positive bundled restatements that do result in litigation outside the Eighth and Ninth Circuits, the

¹⁰ There is no significant difference in the cumulative abnormal returns for restatements that are litigated outside the Eighth and Ninth Circuits (Post-*Dura*: −11.11% vs. Pre-*Dura*: −11.39%; p-value = 0.920).

sample only consists of 3 restatements, 2 of which that have cumulative abnormal returns less than zero, 1 that is related to option backdating, and 2 result in dismissals. Similarly, the difference-in-difference test indicates that the reduction in litigation occurrence for positive bundled restatements across the two time periods is only significant for the Eighth and Ninth Circuits (-0.180 ; $p\text{-value} = 0.001$).

Panel B of Table 4 presents the univariate results on the occurrence of litigation for noise bundled restatements versus non-bundled restatements. During the *Pre-Dura* era, noise bundled restatements resulted in litigation 14.9% of the time, while non-bundled restatements only resulted in litigation 11.8% of the time. Before the *Dura* ruling, noise bundled restatements resulted in litigation 17.8% of the time in the Eighth and Ninth Circuits, which is significantly greater for non-bundled restatements ($p\text{-value} = 0.065$). In contrast, litigation occurrence is very similar for bundled and non-bundled restatements for firms located outside the Eighth and Ninth Circuits (13.7% vs. 13.0%, $p\text{-value} = 0.840$).

After the *Dura* ruling, litigation occurrence for noise bundled restatements dropped significantly to 5.6% for restatements in the Eighth and Ninth Circuits and 5.8% for other Circuits (collectively 5.7% vs. 14.9%, $p\text{-value} = 0.000$). However, univariate results suggest that noise bundled restatements were significantly less likely to result in litigation versus non-bundled restatements only for firms located in the Eighth and Ninth Circuits (0.056 vs. 0.201; $p\text{-value} = 0.001$). Given the significant negative market reaction associated with noise bundling, it is unlikely that inattention explains the lower litigation propensity. The difference-in-difference tests indicate that the occurrence of litigation was significantly reduced for noise bundled restatements only for the Eighth and Ninth Circuits (-0.235 , $p\text{-value} = 0.001$).

Overall, Table 4 provides preliminary support that both positive and noise bundling became significantly more effective in reducing litigation risk for firms located in the Eighth and Ninth Circuits, and the lack of significance in the overall change in litigation propensity before and after the *Dura* ruling for non-bundled restatements suggests that the results are not driven by an overall reduction in litigation propensity (11.8% vs. 12.4%; $p\text{-value} = 0.824$).

To ensure that our results are not driven by trends unique to bundled restatements, we examine firm and restatement characteristics before and after the *Dura* ruling in Table 5. The primary concern is time-series differences within the treatment group and between the treatment and control group. Panel A of Table 5 reports the firm and restatement characteristics based on bundling for firms located in the Eighth and Ninth Circuits. Difference-in-difference testing indicates that firm and restatement trends are similar between bundled and non-bundled restatements for firms located in the Eighth and Ninth Circuits, suggesting that there are no systematic differences between bundled and non-bundled restatements after the *Dura* ruling. Despite insignificant differences in firm and restatement trends, litigation occurrence decreased from 14.6% to 4.1% for bundled restatements, but increased from 8.8% to 20.1% for non-bundled restatements.

For comparison, Panel B of Table 5 reports the firm and restatement characteristics based on bundling for firms located outside the Eighth and Ninth Circuits. In the absence of changes in loss causation standards, the difference in litigation occurrence across time periods should be the same for both bundled and non-bundled restatements. As expected, the difference-in-difference in litigation occurrence for firms located outside the Eighth and Ninth Circuits is equal to zero. With the exception of the proportion of fraud related restatements, firm and restatement characteristics are similar across both time periods for bundled and non-bundled restatements.

Panel C of Table 5 reports the difference-in-difference tests between the restatement and firm characteristics across Circuits by bundling strategy. With the exception of the number of reasons for restatement, bundled restatements experience similar firm and restatement characteristics trends across Circuits for our sample period. For non-bundled restatements, the only marginal differences in firm and restatement characteristics are share volatility and restatement period for non-bundled restatements. However, the triple difference tests (diff-in-diff-in-diff) suggest that trends do not differ significantly between Panels A and B of Table 5. Overall, the summary statistics from Table 5 demonstrate that firm and restatement characteristic trends were relatively similar within the treatment group and between the treatment and control group.

Fig. 2 reports the distribution of positive and noise bundled restatements by calendar quarter and Circuit. In untabulated results, there are no significant differences in the proportion of positive and noise bundled restatements across time periods for either Circuit groups. The variation in the distribution of bundling for the Eighth and Ninth Circuits suggests that firms did not anticipate the change in loss causation standards.

3.3. Differences-in-differences-in-differences

To formally test whether bundling had a stronger effect in deterring litigation for firms located in the Eighth and Ninth Circuits while controlling for any marginal differences in firm and restatement characteristics, we pool bundled and non-bundled restatements together and perform the following difference-in-difference-in-difference probit regression:

$$\begin{aligned} Litigation_{i,t} = & \beta_1(Post - Dura) + \beta_2(8^{th}\&9^{th}Circuits) + \beta_3(Bundled) + \beta_4(Post - Dura * 8^{th}\&9^{th} \\ & Circuits) + \beta_5(Post - Dura * Bundled) + \beta_6(8^{th}\&9^{th}Circuits * Bundled) + \beta_7(Post - Dura * 8^{th}\&9^{th} \\ & Circuits * Bundled) + Controls + Industry Fixed Effects + Year Fixed Effects \end{aligned} \quad (2)$$

where the dependent variable, *Litigation*, is equal to one if the firm was a target of a class-action lawsuit within one year of the restatement announcement, and zero otherwise. *Post-Dura* is a dummy variable equal to one if the restatement occurs after the *Dura* ruling, and zero otherwise. *8th & 9th Circuits* is a dummy variable equal to one if the firm is headquartered in the Eighth and Ninth Circuits, and zero otherwise. *Bundled* is a dummy variable equal to one if the restatement is bundled

Table 5Summary Statistics by *Dura* Era.

Panel A of Table 5 reports summary for before and after the *Dura* ruling for the firms located in the 8th and 9th Circuits. Panel B of Table 5 reports summary statistics for the firms located outside the 8th and 9th Circuits that issued earnings restatements before and after the *Dura* ruling. Panel C of Table 5 reports the difference-in-difference in restatement and firm characteristics across Circuits by bundling group. The test for differences in the means is conducted by calculating a two-tail t-statistic with standard errors clustered by industry (48-classification). Diff-in-Diff is the difference between the average differences for the two groups of firms. All continuous variables are winsorized at the 1% and 99% level. Please see the [Appendix A](#) for variable definitions. *, **, *** indicates significance at the 1%, 5%, and 10% level, respectively.

Panel A: 8th & 9th Circuits Difference-in-Difference							
	Bundled			Non-Bundled			Diff-in-Diff
	Before	After	Diff	Before	After	Diff	
Size	5.952	6.168	0.216	5.873	5.804	−0.069	0.285
BHR	0.042	−0.006	−0.048	0.014	−0.094	−0.108	0.060
Share Turnover	0.756	0.845	0.089*	0.768	0.807	0.039	0.050
Volatility	0.034	0.032	−0.002	0.038	0.033	−0.005	0.003
Skew	0.153	0.229	0.076	0.091	0.062	−0.029	0.105
Restatement History	0.150	0.503	0.353***	0.348	0.543	0.195*	0.158
SEC	0.056	0.074	0.018	0.072	0.080	0.008	0.010
Fraud	0.019	0.011	−0.008	0.029	0.012	−0.017	0.009
Restatement Magnitude	−0.019	−0.029	−0.010	−0.034	−0.044	−0.010	0.000
Restatement Period	8.561	10.189	1.628	9.385	12.945	3.560	1.932
Reasons For Restatement	4.196	3.171	−1.025**	4.174	3.377	−0.797**	−0.228
Regulation Restatement	0.187	0.160	−0.027	0.232	0.247	0.015	−0.042
Litigation	0.146	0.041	−0.105**	0.088	0.201	0.113***	−0.218***

Panel B: Non-8th & 9th Circuits Difference-in-Difference							
	Bundled			Non-Bundled			Diff-in-Diff
	Before	After	Diff	Before	After	Diff	
Size	6.667	6.607	−0.060	6.573	6.532	−0.041	−0.019
BHR	0.116	−0.034	−0.150***	0.174	−0.063	−0.237**	0.087
Share Turnover	0.721	0.766	0.045	0.703	0.727	0.024	0.021
Volatility	0.030	0.030	0.000	0.029	0.030	0.001	−0.001
Skew	0.122	0.179	0.057	0.158	0.203	0.045	0.012
Restatement History	0.341	0.618	0.277***	0.377	0.573	0.196**	0.081
SEC	0.068	0.084	0.016	0.154	0.111	−0.043	0.059
Fraud	0.016	0.028	0.012	0.056	0.022	−0.034	0.046**
Restatement Magnitude	−0.018	−0.015	0.003	−0.021	−0.019	0.002	0.001
Restatement Period	9.508	9.931	0.423	9.605	9.330	−0.275	0.698
Reasons For Restatement	3.867	3.524	−0.343	4.130	3.455	−0.675**	0.332
Regulation Restatement	0.189	0.103	−0.086***	0.160	0.090	−0.071**	−0.015
Litigation	0.098	0.047	−0.051**	0.130	0.079	−0.051*	0.000

Panel C: Difference-in-Difference Across Circuits

	Bundled			Non-Bundled			Diff-in-Diff-in-Diff
	8th & 9th Circuits Diff	Non-8th & 9th Circuits Diff	Diff-in-Diff	8th & 9th Circuits Diff	Non-8th & 9th Circuits Diff	Diff-in-Diff	
Size	0.216	−0.060	0.276	−0.069	−0.041	−0.028	0.304
BHR	−0.048	−0.150	0.102	−0.108	−0.237	0.129	−0.027
Share Turnover	0.089	0.045	0.044	0.039	0.024	0.015	0.029
Volatility	−0.002	0.000	−0.002	−0.005	0.001	−0.006*	0.004
Skew	0.076	0.057	0.019	−0.029	0.045	−0.074	0.093
Restatement History	0.353	0.277	0.076	0.195	0.196	−0.001	0.077
SEC	0.018	0.016	0.002	0.008	−0.043	0.051	−0.049
Fraud	−0.008	0.012	−0.019	−0.017	−0.034	0.017	−0.036
Restatement Magnitude	−0.010	0.003	−0.013	−0.010	0.002	−0.012	−0.001
Restatement Period	1.628	0.423	1.205	3.560	−0.275	3.836*	−2.631
Reasons For Restatement	−1.025	−0.343	−0.682*	−0.797	−0.675	−0.122	−0.560
Regulation Restatement	−0.027	−0.086	0.059	0.015	−0.071	0.086	−0.027

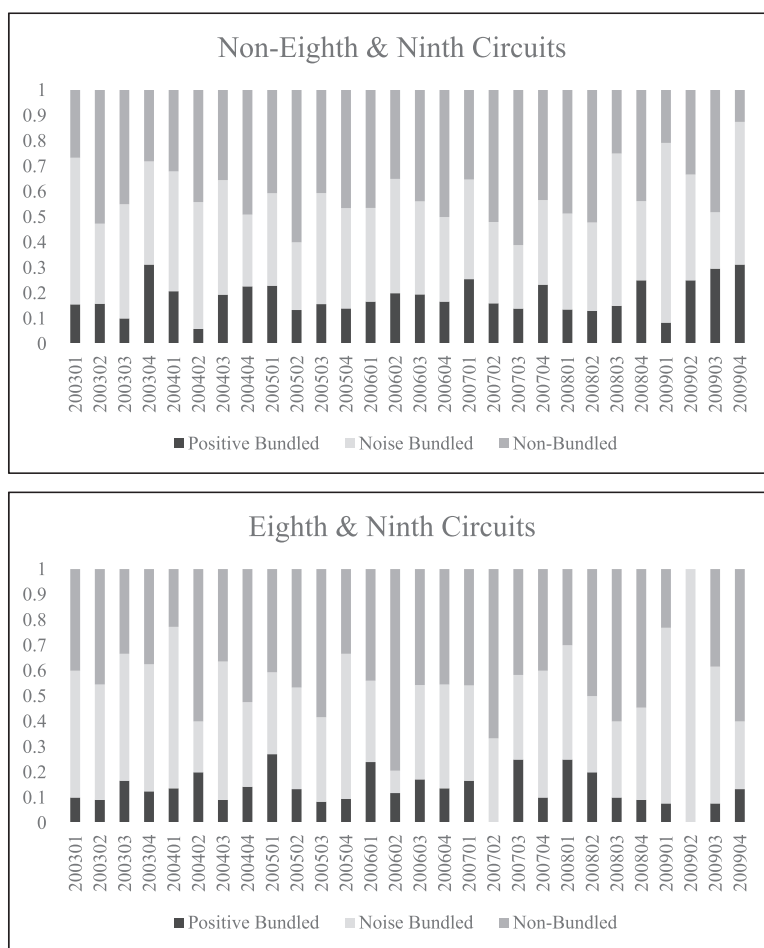


Fig. 2. Proportion of Bundled Restatements by Circuits.

Figure 2 reports the distribution of positive, noise, and non-bundled restatements for firms located outside and inside the Eighth and Ninth Circuits. Distributions are reported by calendar quarter.

with additional news, and zero otherwise. We also include a number of controls including restatement magnitude, restatement period length, reasons for restatement, restatement history, $CAR[-1,1]$ at restatement, firm size, share turnover, prior 12-month market-adjusted returns (BHR), return volatility and skew, and indicator variables for fraud, SEC investigation, and regulation-related restatements. Industry and year fixed effects are included. Standard errors are clustered by industry.

In Table 6, we regress separately for noise bundled restatements (columns 1–3), and all bundled restatements (columns 4–6). Since litigation does not occur for any positive bundled restatement for firms located in the Eighth and Ninth Circuits after the *Dura* ruling, we are unable to run a triple difference-in-difference regression separately for positive bundled restatements. We report the marginal effects and the mean partial effects for the interaction terms following Cornelissen and Sonderhof (2009).¹¹

Size is positive and significantly related to litigation propensity across all time periods. Similar to Donelson et al. (2012) who find that severe price drops increase the likelihood of shareholder litigation, restatement announcement returns ($CAR[-1,1]$) are inversely related to litigation propensity for all time periods. Following Field et al., (2005), we control for share turnover for the one-year period ending in the month prior to restatement, and find that firms facing litigation have significantly higher turnover. We also control for industry to capture the high litigation rates in various industries (Bajaj et al., 2002; Field et al., 2005).

We control for restatement characteristics and find that more adverse restatements (*Restatement Magnitude* and *Reasons for Restatement*) are more likely to face litigation. Restatements associated with an SEC investigation are not associated with higher litigation propensity, highlighting the difference in outcomes for SEC investigation and non-SEC investigation induced

¹¹ Cornelissen and Sonderhof (2009) show how to interpret and calculate the effects of triple dummy variable interactions for non-linear regression models. The standard errors of the partial effects are computed using the delta method (Davidson and MacKinnon, 2004). We follow Cornelissen and Sonderhof (2009) for interpretation of the interactions.

Table 6

Litigation propensity.

This table reports the marginal effects from a probit regression where the dependent variable, litigation, is equal to one if the firm is a defendant of a securities class-action lawsuit within one year of the restatement announcement, and zero otherwise. Noise Bundled equals one if the restatement was bundled with additional news that is likely to be perceived as negative or neutral, and zero if the restatement was made in isolation. Bundled (All) is equal to one if the restatement is bundled with additional information (positive or noise), and zero if the restatement is made in isolation. Industry fixed effects are included. The mean interaction term effects are reported following [Cornelissen and Sonderhof \(2009\)](#). All continuous variables are winsorized at the 1% and 99% level. Standard errors are heteroscedasticity robust and clustered by industry (48-classification). Please see the [Appendix A](#) for variable definitions. *, **, *** indicates significance at the 1%, 5%, and 10% level, respectively.

	Noise Bundled: Litigation (1)	Noise Bundled: Litigation (2)	Noise Bundled: Litigation (3)	Bundled (All): Litigation (4)	Bundled (All): Litigation (5)	Bundled (All): Litigation (6)
Post-Dura	−0.049* (0.029)	−0.049 (0.033)	−0.059** (0.027)	−0.45* (0.023)	−0.040 (0.027)	−0.044** (0.021)
8th & 9th Circuits	0.030 (0.022)	0.006 (0.014)	0.004 (0.011)	0.023 (0.018)	0.002 (0.011)	−0.001 (0.008)
Bundled	−0.029 (0.019)	−0.027* (0.013)	−0.035*** (0.012)	−0.048*** (0.018)	−0.040*** (0.013)	−0.037*** (0.011)
Post-Dura * 8th & 9th Circuits	0.056 (0.037)	0.030 (0.029)	0.000 (0.021)	0.020 (0.036)	0.004 (0.025)	−0.018 (0.019)
Post-Dura * Bundled	−0.089** (0.041)	−0.084** (0.034)	−0.066* (0.032)	−0.062* (0.036)	−0.054** (0.028)	−0.038 (0.028)
8th & 9th Circuits * Bundled	−0.047 (0.054)	−0.023 (0.034)	−0.015 (0.026)	−0.049 (0.045)	−0.024 (0.031)	−0.015 (0.024)
Post-Dura * 8th & 9th Circuits * Bundled	−0.206*** (0.074)	−0.130** (0.053)	−0.133** (0.063)	−0.219*** (0.063)	−0.143*** (0.041)	−0.126*** (0.046)
Restatement Magnitude	—	−0.338*** (0.104)	−0.217** (0.086)	—	−0.288*** (0.081)	−0.163*** (0.059)
Restatement Periods	—	0.001 (0.001)	0.001 (0.001)	—	0.001 (0.001)	0.000 (0.000)
Reasons for Restatement	—	0.008** (0.003)	0.005* (0.003)	—	0.007*** (0.003)	0.004* (0.002)
Regulation Restatement	—	−0.032*** (0.012)	−0.025*** (0.008)	—	−0.025** (0.010)	−0.015** (0.007)
CAR[−1,1] at Restatement	—	—	−0.558*** (0.062)	—	—	−0.458*** (0.052)
Restatement History	—	−0.021*** (0.007)	−0.014** (0.006)	—	−0.018*** (0.005)	−0.010** (0.004)
Share Turnover	—	0.202*** (0.026)	0.161*** (0.026)	—	0.178*** (0.022)	0.133*** (0.019)
BHR	—	0.007 (0.013)	−0.004 (0.008)	—	0.002 (0.011)	−0.007 (0.006)
Size	—	0.013*** (0.005)	0.011*** (0.004)	—	0.009** (0.004)	0.007*** (0.002)
Fraud	—	0.072 (0.047)	0.039 (0.034)	—	0.035 (0.031)	0.017 (0.022)
SEC	—	−0.006 (0.018)	−0.016 (0.011)	—	0.003 (0.019)	−0.008 (0.011)
Volatility	—	−0.247 (0.532)	−1.362*** (0.397)	—	−0.357 (0.411)	−1.201*** (0.295)
Skew	—	−0.010*** (0.004)	0.000 (0.003)	—	−0.008** (0.003)	0.001 (0.002)
Industry Fixed Effects	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
Pseudo R ²	0.036	0.199	0.315	0.043	0.1922	0.3168
Observations	1274	1274	1274	1545	1545	1545

restatements. Specifically, SEC investigations and enforcement actions often result in payments from individual officers as well as individuals being banned from serving as directors or officers of public companies ([Choi and Pritchard, 2012](#)), not class-action securities litigation. Although [Kim and Skinner \(2012\)](#) find a positive relation for volatility and skew of prior returns with litigation, return volatility is inversely associated with litigation for our sample.¹²

Columns 1, 2, and 3 of [Table 6](#) report results for noise bundled restatements. Results in column 3 indicate that noise bundled restatements are 3.5% less likely to be litigated (p-value = 0.003). Following [Cornelissen and Sonderhof \(2009\)](#), the interaction term, *Bundled*Post-Dura*, suggests that the *Dura* ruling marginally reduced the occurrence of litigation for all noise bundled restatements (column 3: −0.066; p-value = 0.060). Our main variable of interest, the triple interaction term

¹² The inclusion of Market-to-Book does not have a material impact on the results reported in [Table 6](#). However, we do not include it due to the lack of availability for 15 firms. Results are robust if standard errors are clustered by firm.

Table 7

Litigation outcome across information bundling categories.

Panel A of Table 7 reports the occurrence of dismissals for bundled and non-bundled restatements. Panel B of Table 7 reports the average natural log of settlement values for bundled and non-bundled restatements. Dismissal is equal to one if the lawsuit is dismissed, and zero if the lawsuit is settled. Bundled is equal to one if a restatement was bundled, and zero if a restatement was made in isolation. Difference is the average difference in settlement outcome between bundled and non-bundled restatements. Diff-in-Diff is the difference between the average differences for the two groups of firms between Pre- and Post-*Dura*. The test for differences in the means is conducted by calculating a two-tail t-statistic with standard errors clustered by industry (48-classification). *, **, *** indicates significance at the 1%, 5%, and 10% level, respectively.

Panel A: Dismissal		Bundled (1)	Non-Bundled (2)	Difference (p-value) (3)	Diff-in-Diff (p-value) (4)
8th & 9th Circuits	Pre- <i>Dura</i>	0.444	0.667	−0.223(0.329)	
	Post- <i>Dura</i>	0.538	0.125	0.413*** (0.005)	0.636** (0.020)
Non-8th & 9th Circuits	Pre- <i>Dura</i>	0.450	0.222	0.228(0.142)	
	Post- <i>Dura</i>	0.476	0.240	0.236* (0.095)	0.008(0.968)
Panel B: Ln(Settlement)		Bundled	Non-Bundled	Difference (p-value)	Diff-in-Diff (p-value) (4)
8th & 9th Circuits	Pre- <i>Dura</i>	8.740	5.015	3.725(0.313)	
	Post- <i>Dura</i>	7.211	14.861	−7.650*** (0.001)	−11.375** (0.011)
Non-8th & 9th Circuits	Pre- <i>Dura</i>	8.509	12.858	−3.858(0.133)	
	Post- <i>Dura</i>	8.570	12.302	−3.732(0.105)	0.616(0.856)

(*Post-Dura* * 8th & 9th Circuits * Bundled), is −0.133 (p-value = 0.036) in column 3, which indicates that the change in litigation occurrence, net of change in litigation for firms located outside the Eighth and Ninth Circuits, from before to after the *Dura* ruling is significantly lower for noise bundled restatements by 13.3% for firms located in the Eighth and Ninth Circuits.

Positive bundling and noise bundling are equally likely to confound loss causation for firms located in the Eighth and Ninth Circuits, and because there are no occurrences of litigation for positive bundled restatements after the *Dura* ruling for firms located in the Eighth and Ninth Circuits, we pool all forms of bundling in columns 4, 5, and 6 of Table 6. Similarly, we find that bundled restatements are 3.7% to 4.8% less likely to be litigated (p-value = 0.001). The triple interaction term, *Post-Dura* * 8th & 9th Circuits * Bundled, suggests that litigation occurrence for bundled restatements in the Eighth and Ninth Circuits was significantly reduced by 12.6% (p-value = 0.007) after the *Dura* ruling, in comparison to firms outside the Eighth and Ninth Circuits.¹³

Overall, our results indicate that the decrease in litigation occurrence is restricted to bundled restatements for firms located in the Eighth and Ninth Circuits, which experienced a tightening of the loss causation standards, but not to our control firms that are outside the Eighth and Ninth Circuits.

3.4. Bundling and litigation outcomes

Next, we quantify the benefits of information bundling on litigation settlements. While other studies have found that disclosure practices can reduce litigation, litigation occurrence is often treated as a binary event and thus the literature contains little or no insight as to the actual dollar amounts saved. To further explore the benefits of information bundling, we examine settlement amounts for those restatements that have resulted in securities litigation. In this analysis, lawsuits are classified as Pre- and Post-*Dura* based on the lawsuit filing date.

Out of the full sample of 1562 restatements, 144 result in litigation consisting of 6 positive bundled restatements and 57 noise bundled restatements. Table 7 provides univariate difference-in-difference results on the litigation outcomes by bundling category and Circuit. Again, since no positive bundled restatements for the Eighth and Ninth Circuits result in litigation after the *Dura* ruling, we pool positive and noise bundling together for the rest of our analyses.

Panel A of Table 7 reports the occurrence of dismissal by bundling category and Circuit. Before the *Dura* ruling, only 44.4% of lawsuits associated with bundled restatements were dismissed, compared to 66.7% of lawsuits associated with non-bundled restatements for firms located in the Eighth and Ninth Circuits. After the *Dura* ruling, dismissals increased to 53.8% for bundled restatements, whereas the percentage of dismissals decreased significantly to 12.5% of non-dismissed cases for firms located in the Eighth and Ninth Circuits. In other words, bundled restatements were 8.17 times more likely to result in a dismissal than non-bundled restatements. The difference-in-difference testing indicates the average difference in dismissal rates was significantly higher for bundled restatements for the Eighth and Ninth Circuits (0.636, p-value = 0.020). In contrast, the dismissal rate remained relatively constant across both time periods for firms located outside the Eighth and Ninth Circuits (0.008, p-value = 0.968).

Panel B of Table 7 reports the natural log of settlement value by bundling category and Circuit. For our sample, the average (median) settlement value including dismissals is \$9.31 million (\$1.60) for bundled restatements, and \$40.27 million (\$10.00) for non-bundled restatements. Given that the sample of settled cases contains large settlement amounts, we use the natural log of settlement amounts in our analysis. For the Eighth and Ninth Circuits, settlement values are significantly

¹³ Results are similar and robust to the exclusion of restatements that bundled with 10-K and 10-Q forms.

higher for non-bundled restatements after the *Dura* ruling, and the difference-in-difference testing indicates that the differential in settlement amounts for bundled restatements is significantly less than for non-bundled restatements (-11.375 , $p\text{-value} = 0.011$). Similar to dismissal rates, settlement values are roughly the same across time periods by bundling category for firms outside the Eighth and Ninth Circuits, and we find no evidence of any significant differential in the settlement amounts across both time periods (0.161 , $p\text{-value} = 0.856$).¹⁴

To formally control for firm, restatement, and lawsuit characteristics that may influence settlement values, we run the following triple difference-in-difference OLS regression:

$$\begin{aligned} \ln(1 + \text{Settlement Value})_{i,t} = & \beta_1 (\text{Post} - \text{Dura}) + \beta_2 (\text{Bundled}) + \beta_3 (8^{\text{th}} \& 9^{\text{th}} \text{ Circuits}) + \beta_4 (\text{Post} - \\ & \text{Dura} * 8^{\text{th}} \& 9^{\text{th}} \text{ Circuits}) + \beta_5 (\text{Post} - \text{Dura} * \text{Bundled}) + \beta_6 (8^{\text{th}} \& 9^{\text{th}} \text{ Circuits} * \text{Bundled}) \\ & + \beta_7 (\text{Post} - \text{Dura} * 8^{\text{th}} \& 9^{\text{th}} \text{ Circuits} * \text{Bundled}) + \text{Controls} + \text{Year Fixed Effects} + \text{Industry Fixed Effects} \end{aligned} \quad (3)$$

where the dependent variable is the natural logarithm of one plus the settlement value. We control for the length of the class period stated in the lawsuit, restatement magnitude, restatement period length, number of reasons for restating, firm size, restatement history, cumulative abnormal returns surrounding the restatement announcement ($CAR[-1,1]$ at *Restatement*), prior 12-month market-adjusted returns (*BHR*), market-to-book, share turnover, and return volatility and skew. We include indicators for fraud, SEC investigation, regulation-related restatements, and cases that involve option backdating (*Option Backdating*). Industry fixed effects and year fixed effects are included. Due to the smaller sample size, standard errors are clustered at the firm level.

In addition to restatement and firm characteristics, we include the class period length of lawsuit to control for lawsuit severity. We include *Option Backdating*, which is an indicator variable that equals one if the case summary mentions option backdating as a reason for the lawsuit. We include return volatility and skew, which might make it difficult to calculate damages based off stock return data. We include the market-to-book at the restatement announcement to control for potential over-valuation at the time of the corrective disclosure.

Table 8 reports coefficients of an OLS regression. Columns 1 and 2 of Table 8 report the coefficients for the entire sample of litigated restatements, and columns 3 and 4 of Table 8 report the coefficients for the sample of non-dismissed litigated restatements.

For the full sample of lawsuits, the coefficients in columns 1 and 2 of Table 8 indicate that the number of reasons for the restatements and size significantly increases the settlement amount. The interaction, *Post-Dura * 8th & 9th Circuits*, indicates that the settlement values were significantly higher after the *Dura* ruling in Eighth and Ninth Circuits. However, the triple interaction, *Bundled * 8th & 9th Circuits * Post-Dura*, is significantly negative, indicating that the increase in settlement values occurred primarily for non-bundled restatements. The combined effect indicates that bundling reduced settlement amounts by 56% to 58% for the full sample.

To ensure that dismissals are not the primary driver of lower settlement amounts, columns 3 and 4 of Table 8 report the coefficients for the sample of non-dismissed lawsuits. Table 8 indicates that larger firms paid significantly larger settlement amounts, whereas fraud related restatements were associated with significantly smaller payouts. Similar to results for the full sample of lawsuits, our variable of interest, the triple interaction term, *Bundled * 8th & 9th Circuits * Post-Dura*, is negative and significant, indicating that settlement values were significantly lower for our treatment firms that used information bundling after the increase in loss causation standards. The combined effect indicates that bundling reduced settlements by 50% to 67% after the *Dura* ruling.¹⁵ With an average settlement amount of \$40.09 million for the sample of non-dismissed cases, this translates into a savings of \$23.45 million. Our results contrast with the findings of Donelson and Hopkins (2016) that confounding factors (e.g., large market declines) increase settlement amounts.

3.5. Litigation and litigation outcome for matched sample

In this study, we use both full sample and matched sample to test our hypotheses, and to address questions about whether the decision to bundle a restatement with other information might be endogenous to the nature of the firms and the restatement. For example, bundled restatements might not be as damaging to shareholders as non-bundled restatements. As a robustness test, we implement the Abadie and Imbens (2006) matching estimator. Specifically, under this approach, we first identify a set of “treated” observations, i.e., firms that bundle other information with restatements. From the remaining population of non-treated observations (firms that released a restatement but did not bundle), we search for control firms that match treatment firms along multiple dimensions. In our estimations, we select one match control for each treatment firm and allow control firms to serve as matches more than once.¹⁶ This matching procedure allows us to isolate the effect of

¹⁴ We find similar results in Table 8 if we exclude option-backdating cases from the difference-in-difference analyses.

¹⁵ The summation of the *Post-Dura*, *Bundled*, *8th & 9th Circuits*, *Post-Dura * 8th & 9th Circuits*, *Post-Dura * Bundled*, and *Bundled * 8th & 9th Circuits * Post-Dura* coefficients is -0.838 and -1.119 for columns 1 and 3, respectively. The aggregated exponentiated coefficients reduce settlements by 56% and 67% for columns 1 and 3, respectively.

¹⁶ Allowing control firms to serve as matches more than once lowers estimation bias, but can increase variance, compared to matching without replacement.

Table 8

Determinants of settlement values across alternative information bundling categories.

This table reports coefficients and standard errors obtained from an OLS regression where the dependent variable is the natural logarithm of one plus the settlement amount (in millions). If the lawsuit is dismissed, settlement amount is equal to zero. Columns 1 and 2 report the coefficients for all litigated cases, and columns 3 and 4 report the coefficients for the subsample of non-dismissed cases. Year and industry fixed effects are included. All continuous variables are winsorized at the 1% and 99% level. Standard errors (in parentheses) are heteroscedasticity robust and clustered by firm. Please see the [Appendix A](#) for variable definitions. *, **, *** indicates significance at the 1%, 5%, and 10% level, respectively.

	All Cases: Ln(Settlement) (1)	All Cases: Ln(Settlement) (2)	Non-Dismisssed: Ln(Settlement) (3)	Non-Dismisssed: Ln(Settlement) (4)
Post-Dura	0.521 (0.915)	0.607 (0.916)	−0.933* (0.536)	−0.250 (0.570)
Bundled	−0.713 (0.567)	−0.718 (0.573)	−1.107** (0.453)	−1.330*** (0.469)
8th & 9th Circuits	−0.702 (0.605)	−0.814 (0.612)	−0.928 (0.584)	−1.392** (0.674)
Post-Dura * 8th & 9th Circuits	1.639** (0.676)	1.611** (0.698)	1.259** (0.606)	1.566** (0.601)
Post-Dura * Bundled	0.304 (0.709)	0.302 (0.697)	1.199** (0.598)	1.426** (0.617)
Bundled * 8th & 9th Circuits	0.916 (0.778)	1.078 (0.782)	1.753** (0.855)	2.242** (0.896)
Bundled * 8th & 9th Circuits * Post-Dura	−2.803*** (0.991)	−2.940*** (0.993)	−2.362** (0.959)	−2.952*** (0.924)
Class Period Length	0.008 (0.023)	0.006 (0.023)	0.013 (0.021)	0.017 (0.022)
Restatement Magnitude	−0.126 (1.994)	−0.571 (1.976)	−1.041 (1.636)	−1.523 (1.908)
Restatement Period Length	0.033* (0.018)	0.029 (0.018)	0.024 (0.017)	0.025 (0.016)
Reasons for Restatement	0.118* (0.063)	0.106* (0.063)	0.037 (0.050)	0.022 (0.054)
Regulation Restatement	−0.554 (0.414)	−0.430 (0.421)	−0.391 (0.440)	−0.403 (0.352)
CAR[−1,1] at Restatement	—	−0.313 (1.183)	—	−2.119** (0.819)
Size	0.249* (0.127)	0.270** (0.130)	0.375*** (0.130)	0.405*** (0.121)
Share Turnover	1.637 (1.061)	1.104 (1.070)	0.763 (1.194)	0.680 (1.377)
Fraud	−0.331 (0.573)	−0.349 (0.592)	−0.748* (0.378)	−0.811* (0.430)
SEC	0.668 (0.510)	0.559 (0.526)	0.300 (0.385)	0.038 (0.458)
Restatement History	0.104 (0.221)	0.190 (0.232)	0.077 (0.203)	0.361 (0.237)
BHR	0.188 (0.214)	0.119 (0.218)	0.100 (0.244)	−0.072 (0.290)
Volatility	0.752 (15.747)	1.852 (16.136)	1.904 (13.366)	0.232 (14.384)
Option Backdating	0.529 (0.492)	0.554 (0.496)	0.692 (0.507)	0.619 (0.429)
Skew	−0.076 (0.094)	−0.070 (0.092)	−0.047 (0.102)	−0.040 (0.100)
Market-to-Book	—	0.048** (0.022)	—	0.026 (0.024)
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Constant	−2.371 (1.820)	−2.207 (1.880)	−1.162 (1.438)	−2.118 (1.491)
Observations	144	143	96	95
R-squared	0.521	0.525	0.707	0.731

bundling on litigation with respect to similar firm and restatement characteristics. We match on the following dimensions:

$$\text{Treatment group} = f(\text{Restatement Magnitude, Restatement Period, SEC, Size, Share Turnover, BHR}) \quad (4)$$

We conduct exact matches by industry (17-industry classification codes), Circuit (8th & 9th vs. non-8th & 9th), and match along the following dimensions that may influence the firm's decision to bundle and affect likelihood of litigation:

Table 9

Litigation propensity after matching on restatement and firm characteristics.

Panel A of Table 9 reports the Abadie-Imbens (AI) matching estimator of the average effect of the treatment (ATE). Panel B of Table 9 reports the matching estimator of the average effects of the treatment (ATE) for the subsample of control firms that release a non-bundle restatement announcement within 7 days of an earnings announcement. Treated firms bundled other information with a restatement announcement. Non-treated firms are remaining firms that did not bundle other information with the restatement announcement. Control firms are a subset of the non-treated firms selected in the same industry (17-classification) and the closest match to the treated firms based on restatement magnitude, restatement period, SEC, firm size, share turnover, and prior 12-month return (BHR). We run the matching procedure separately on both time periods to achieve covariate balance for each period. ***, **, * indicates significance at the 1%, 5%, and 10% level, respectively.

Panel A: Litigation Propensity		Pre- <i>Dura</i>	Post- <i>Dura</i>
Positive Bundled:	8th & 9th Circuits	0.044 (0.086)	−0.176*** (0.028)
	Non-8th & 9th Circuits	−0.122*** (0.027)	−0.062** (0.023)
Noise Bundled:	8th & 9th Circuits	0.154** (0.069)	−0.157*** (0.040)
	Non-8th & 9th Circuits	−0.058* (0.035)	−0.063*** (0.023)
Panel B: Litigation Propensity For Limited Sample		Pre- <i>Dura</i>	Post- <i>Dura</i>
Positive Bundled:	8th & 9th Circuits	0.051 (0.073)	−0.374*** (0.099)
	8th & 9th Circuits	0.209*** (0.049)	−0.279** (0.141)

restatement magnitude, restatement period, SEC investigation related restatement, firm size, share turnover, and prior 12-month returns (*BHR*).¹⁷ We run the matching procedure separately on both time periods to achieve covariate balance for each period. We match on restatement magnitude and restatement period to control for any differences in the severity of the restatement that may influence the decision to bundle the restatement and litigation propensity. We match on firm size to control for any legal sophistication that larger firms might have with respect to damaging disclosures and the higher likelihood of litigation associated with larger firms. We match on SEC investigation to control for the lack of discretion that firm might have with regards to the timing of the announcements given that these restatements are likely induced by a third-party. We match on share turnover and prior 12-month returns (*BHR*) to capture the higher probability of litigation. By controlling for restatement characteristics and firm characteristics, we are able to estimate the impact that information bundling had on litigation propensity.¹⁸ Panels A and B of [Appendix B](#) reports the mean values of the restatement and firm characteristics of the treated, non-treated firms, and control firms by time period and Circuit.¹⁹

The Abadie-Imbens estimator applies a bias-correction to the estimates of interest and produces heteroskedastic-robust standard errors. For purposes of analyzing the impact of positive and noise bundling on litigation, we perform the estimation in which we compare the occurrence of litigation between the treated and control groups.

As a measure of the differential occurrence in litigation between firms that bundled restatements (treatment firms) and their matched counterparts (control firms), Panel A of [Table 9](#) reports the [Abadie-Imbens \(2006\)](#) matching estimator of the average treatment effect (ATE). For the Eighth and Ninth Circuits, positive bundling only significantly reduces litigation occurrence after the *Dura* ruling (−0.176; *p*-value = 0.001). Consistent with results in [Table 4](#), positive bundling is associated with significantly lower litigation occurrence for both time periods for firms located outside the Eighth and Ninth Circuits.²⁰

Similarly, we find that noise bundling only significantly reduces litigation for firms located in the Eighth and Ninth Circuits after the increase in loss causation standards. Before the *Dura* ruling, noise bundled restatements were 15.4% more likely to face litigation for firms located in the Eighth and Ninth Circuits. After the *Dura* ruling, we find that noise bundled restatements are 15.7% less likely to result in litigation, reversing the relationship between noise bundling and litigation.²¹ Similar to positive bundling, we find that noise bundling is associated with lower litigation occurrence, by 5.8 to 6.3%, for both time periods for firms outside the Eighth and Ninth Circuits once we control for restatement severity and firm characteristics.²²

¹⁷ We use 17-industry classification code instead of 48-industry classification code to generate a higher percentage of exact matches.

¹⁸ Results are similar if $CAR[-1,1]$ is used as a matching dimension.

¹⁹ In untabulated results, the median value of restatement magnitude is statistically different between the treated and non-treated restatements for positive bundling after *Dura*, and for noise bundling before *Dura*, suggesting that 9 out of 24 of the dimensions are statistically different from one another. We would like to emphasize that the number of observations is relatively low due to the various cuts of the data, which impacts the power of our tests.

²⁰ It is possible that our measures of restatement severity are rough and that unobservable differences potentially bias the estimates.

²¹ Results for noise bundled restatements are robust to the exclusion of the restatements bundled with 10-K or 10-Q. Litigation occurrence is 18.4% (*p*-value=0.020) before the *Dura* ruling, and −15.61% (*p*-value=0.000) after the *Dura* ruling.

²² Propensity score matching produces similar results.

Table 10

Estimates of settlement values after matching on restatement and firm characteristics.

Panel A of Table 10 reports the Abadie-Imbens (AI) matching estimator of the average effect of the treatment on the treated (ATE) for the full sample of lawsuits. Panel B of Table 10 reports the matching estimator of the average effect of the treatment on the treated (ATE) for the subsample of non-dismissed lawsuits. The treated firms are defined as those firms that bundled a restatement announcement. The non-treated firms are the remaining firms that did not bundle the restatement. Settlement amounts are in millions. Exact matches are made on industry (17-classification code), and selected as the closest match to the treated firms based on restatement magnitude, class period length of lawsuit, firm size, share turnover, CAR[−1,1] at restatement, and prior 12 month returns (BHR). We run the matching procedure separately on both time periods to achieve covariate balance for each period. ***, **, * indicates significance at the 1%, 5%, and 10% level, respectively.

Panel A: Settlement Amount (All Lawsuits)	Pre- <i>Dura</i>	Post- <i>Dura</i>
8th & 9th Circuits	5.276*** (1.836)	−21.168*** (8.409)
Non-8th & 9th Circuits	−42.684 (38.592)	−2.906 (5.006)
Panel B: Settlement Amount (Non-Dissmised)	Pre- <i>Dura</i>	Post- <i>Dura</i>
8th & 9th Circuits	1.937 (2.297)	−23.356*** (9.001)
Non-8th & 9th Circuits	−37.501 (48.059)	−8.694 (6.930)

Even though bundling is a function of a firm's endowment of other information to use in conjunction with a restatement, it could be argued that firms can always provide new earnings guidance or provide company updates on items such as product development. Although we do not observe the firms that have information to bundle with a restatement but choose not to do so, we are able to identify firms that could have potentially bundled a restatement with an earnings announcement but chose not to do so. As a robustness check, we restrict our control sample of non-bundled firms to those that did not bundle the restatement and were within 7 days of an earnings announcement.

Panel B of Table 9 reports the Average Treatment Effect for firms located in the Eighth and Ninth Circuits for our limited sample of control firms (those that did not bundle but were within 7 days of earnings announcement). Not surprisingly, positive and noise bundling only reduced litigation occurrence after the *Dura* ruling for our treatment firms. Results indicate that positive bundling reduced litigation occurrence by 37.4% and noise bundling reduced litigation occurrence by 27.9%.²³

After controlling for restatement and firm characteristics, these findings support the view that positive and noise bundling became more effective in reducing litigation risk after the *Dura* ruling for firms located in the Eighth and Ninth Circuits. Information bundling already was effective in reducing litigation risk for firms located in Circuits with stringent loss causation standards. In other words, information bundling reduced litigation risk for firms located in Circuits that faced more stringent loss causation requirements.

Again, to address concerns about potential endogeneity, we implement the Abadie and Imbens (2006) matching estimator to estimate the cost savings associated with bundled restatements with respect to settlement values. Under this approach, “treated” firms are those that bundled other information with restatements, and non-treatment firms are those that announced a restatement but did not bundle it with other information. In our estimations, we select two matched control firms for each treatment firm and allow control firms to serve as matches more than once due to the lower number of observations. We run our matching procedure separately on both time periods to achieve covariate balance, and conduct exact matches on Fama-French 17-industry classification codes, Circuit, and match along the following dimensions:

$$\text{Treatment group} = f(\text{Restatement Magnitude, Size, Class Period Length, Share Turnover, BHR, CAR}[-1, 1] \text{ at Restatement}) \quad (5)$$

We match along restatement magnitude and CAR[−1,1] at restatement to control for restatement severity. We match along the lawsuit's class period length to control for the lawsuit severity and potential damages, and along share turnover to capture the size of shareholder base. We match along prior returns (*BHR*) to capture the cumulative performance before the restatement. We match along firm size since larger firms may pay higher settlement values given their available resources and the larger shareholder base associated with large firms.

In this analysis, we exclude option-backdating cases to produce conservative estimates of the cost savings associated with information bundling.²⁴ The Abadie-Imbens estimator applies a bias-correction to the estimates of interest and produces heteroskedastic-robust standard errors. Panel C and D of Appendix B reports the mean values of the restatement and firm characteristics of the treated, non-treated, and control firms by time period and Circuit.

Panel A of Table 10 reports the Abadie-Imbens (2006) matching estimator of the average effect of the treatment on the treated firms (ATE). For bundled restatements in the Eighth and Ninth Circuits, the ATE difference is equal to 5.276 (p-

²³ Results for noise bundled restatements are robust to the exclusion of the restatements bundled with 10-K or 10-Q. Litigation occurrence is 13.17% (p-value=0.047) before the *Dura* ruling, and −33.45% (p-value=0.008) after the *Dura* ruling.

²⁴ The inclusion of option backdating cases increases the cash savings associated with bundling, and provides results of similar significance.

value = 0.001) before the *Dura* ruling and –21.168 (p-value = 0.012) after the *Dura* ruling. This indicates that information bundling was associated with significantly higher settlement values, but the increase in loss causation standards after the *Dura* ruling impacted the settlement calculations and effectively reduced settlement amounts by roughly \$21 million.²⁵ For cases located outside the Eighth and Ninth Circuits, bundling was associated with lower settlement values across both time periods, but the estimates are not statistically significant.²⁶

Panel B of Table 10 reports the settlement values for sample of non-dismissed cases only. Similarly, we find that information bundling significantly reduces settlement amounts by \$23.356 million (p-value = 0.000) after the *Dura* ruling for firms located in the Eighth and Ninth Circuits. Overall, our results demonstrate how disclosure practices can reduce litigation risk and the severity of litigation outcomes.²⁷

3.6. Additional robustness tests

For robustness, we replicate the same “experiment” for litigation occurrence that we run for the *Dura* ruling using placebo Circuits and placebo ruling dates. Such falsification tests can help rule out alternative explanations (Roberts and Whited, 2012). For example, there could be unobservable characteristics that generally predict both the choice to bundle and a decrease in litigation propensity. If that is the case, then bundling choice and litigation should be correlated in other Circuits and time periods as well, and not just in the Eighth and Ninth Circuits (Almedia et al., 2011).

In the first placebo test, we use two random Circuits as our treatment group and examine litigation occurrence before and after the *Dura* ruling. We re-run the difference-in-difference-in-difference test for all of the possible 36 Circuit combinations as our placebo treated firms. In untabulated results, our variable of interest, *Post-Dura * Placebo Circuits * Bundled*, is not significant for any of the 36 Circuit combinations. In the second placebo test, we re-run the difference-in-difference-in-difference test twice using January 1, 2004 and January 1, 2007 as the placebo dates for the *Dura* ruling. Similarly, our variable of interest, *Placebo Ruling Date* 8th & 9th Circuits * Bundled*, is insignificant for noise bundled and all bundled restatements for both placebo dates used. In addition, placebo tests for settlement values produce insignificant triple interaction terms as well.

As another robustness check, we implement an instrumental variable regression and find similar results to our main findings in Table 6. We use how close the restatement release day is to the fiscal quarter end date as our instrument to capture the firm's information endowment.²⁸ In untabulated results, the second stage of the IV regressions indicates that bundling significantly reduced litigation occurrence for firms outside the Eighth and Ninth Circuits during both time periods, before and after the *Dura* ruling. However, bundling only reduced litigation occurrence for firms located in the Eighth and Ninth Circuits after the *Dura* ruling.

4. Conclusion

Anecdotal evidence has long suggested that disclosure practices can reduce firms' litigation risk. Studies by Files et al. (2009) and Myers et al. (2013) found evidence that disclosure packaging can affect stock price reactions and that disclosing a restatement discreetly can reduce litigation risk. We add another disclosure practice to this list: information bundling. We show that information bundling has generated real, tangible benefits for firms: bundling significantly reduced the likelihood of being sued and enabled firms that were sued to settle those lawsuits for significantly lower sums.

Our results have implications for both the literature on the timing of disclosures and the literature on disclosure practices that reduce the cost of securities litigation. We show that information bundling discourages litigation and reduces its costs. Courts and policy makers arguably should consider the prevalence and effectiveness of information bundling. For example, courts might consider relaxing the *Dura* loss causation requirements if multiple pieces of firm-specific news are disclosed simultaneously. Regulators might consider requiring that restatements be disclosed on a stand-alone basis during non-announcement days, so that the stock price response to a restatement can be more easily discerned. At minimum, policy makers should be aware that information bundling affects the litigation environment.

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²⁵ Propensity score matching produces similar estimates of cash savings.

²⁶ Results are similar if CAR[-1,1] at restatement announcement is excluded as a matching dimension.

²⁷ If restatements bundled with 10-Ks and 10-Qs are excluded, bundling reduced settlement values by \$26.532 million (p-value=0.001) for all cases, and \$18.812 million (p-value=0.035) for non-dismissed cases for firms located in the Eighth and Ninth Circuits after the *Dura* ruling.

²⁸ As the fiscal quarter end approaches, firms typically have more information at their disposal to bundle with a restatement (e.g., earnings forecasts, product development, performance metrics, or changes in payout policy – all of which are related to bundling propensity but arguably not related to litigation propensity; the correlation between distance to fiscal quarter end and litigation is 0.125), and high F-Statistics suggest that our selected instrument is not weak.

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Appendix A. Variable Definitions

Variable	Description
8th & 9th Circuits	8th & 9th Circuits equals one if the firm is headquartered in the states belonging to the Eighth and Ninth Circuits, and zero otherwise.
Positive Bundled	Positive Bundled is equal to one if a restatement was made on the same day as a positive earnings announcement (EPS equal or greater than the mean analyst forecast), a dividend increase announcement, a new positive earnings guidance, a tender offer announcement, announcement of a merger, announcement of a special dividend, positive product development news, or a share repurchase announcement. If a restatement is made in isolation, positive bundled is equal to zero.
Noise Bundled	Noise Bundled is equal to one if a restatement was made on the same day as a disappointing earnings announcement (actual EPS was less than the mean analyst forecast), a dividend decrease announcement, a decrease in earnings guidance, a change in management announcement, announcements related to other lawsuits, registration of an SEO, an announcement related to credit or convertible debt arrangements, announcements of acquisitions, or delisting from exchanges. If the restatement is bundled with a quarterly or annual filing (10-Q or 10-K), the restatement is categorized as noise bundled. If a restatement is made in isolation, noise bundled is equal to zero.
BHR	BHR is calculated as the stock return minus the CRSP value-weighted market return for the 12-months prior to the restatement announcement.
CAR[−15,−1]	CAR[−15,−1] is calculated as the stock return minus the CRSP value-weighted market return for 15-days prior to the restatement announcement.
CAR[−1,1] at Restatement	CAR[−1,1] is calculated as the stock return minus the CRSP value-weighted market return for 3-days surrounding the restatement announcement.
Class Period Length	Class Period length is the span of the class period of the securities litigation, measured in quarters.
Dismissal	Dismissal is equal to one if the lawsuit is dismissed, and zero if the lawsuit results in a settlement.
Fraud	Fraud is equal to one if the restatement pertains to fraudulent misreporting, and zero otherwise.
Litigation	Litigation is equal to one if the firm is a target of class-action lawsuit within one year of issuing a restatement, and zero otherwise. If the firm makes multiple restatements that correspond to a class-action lawsuit, the restatement that occurs during the class period of the lawsuit is used.
Ln(Settlement)	Ln(Settlement) is the natural logarithm of 1 plus the settlement amount (in millions). Settlement amount is set to zero for cases that are dismissed.
Market-to-Book	Market-to-book is measured as the price times the number of shares outstanding, divided by book value of equity, prior to the restatement announcement.
Option Backdating	Option backdating is equal to one if the lawsuit case summary mentions option backdating, zero otherwise.
Post-Dura	Post-Dura is equal to one if the restatement is announced after April 18th, 2005, and zero otherwise.
Reasons for Restatement	Reasons for restatement is the number of reasons for restating contained in restatement description that is provided by Audit Analytics.
Regulation Restatement	Regulation Restatement is equal to one if the main reason for restatement is lease-related in 2005 and option backdating-related in 2006, and zero otherwise.
Restatement History	Restatement history is the number of times the firm has announced a restatement in the previous 5 years.
Restatement Magnitude	Restatement magnitude is the cumulative change on earnings, scaled by lagged assets.
Restatement Period	Restatement period is the number of quarters that the restatement period spans.
SEC	SEC is equal to one if the restatement pertains to an SEC investigation, and zero otherwise.
Share Turnover	Share Turnover is calculated as $[1 - \frac{1}{\text{volume traded/total shares}}]$, accumulated over the 1-year period ending on the month prior to the restatement (Field et al., 2005).
Size	Size is the natural logarithm of total assets. Total assets are measured in the fiscal year prior to the restatement announcement.
Skew	Skew is measured using the firm's stock returns in the 12-months prior to the restatement announcement.
Volatility	Volatility is the standard deviation of the firm's stock returns in the 12-months prior to the restatement announcement.

Appendix B. Matching Characteristics

Appendix B compares the mean values of various restatement and firm characteristics of treated, non-treated, and control firms before and after matching. Treated firms bundled other information with a restatement announcement. Non-treated firms are remaining firms that did not bundle other information with the restatement announcement. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms based matching dimensions. Panel A reports the mean value of restatement and firm characteristics for treated, non-treated, and control firms for firms located in the 8th and 9th Circuits for the matching procedure reported in [Table 9](#) that examines litigation occurrence. Panel B reports the mean value of restatement and firm characteristics for treated, non-treated, and control firms for firms located outside the 8th and 9th Circuits for the matching procedure reported in [Table 9](#) that examines litigation occurrence. For litigation occurrence, firms are matched along the following dimensions: 17-industry classification (exact match), restatement magnitude, restatement period, firm size, SEC investigation related restatements, share turnover, and prior 12-month return (BHR). Panel C reports the mean value of restatement and firm characteristics for treated, non-treated, and control firms for firms located in the 8th and 9th Circuits for the matching procedure reported in [Table 10](#) that examines settlement values for those restatements that end up in litigation. Panel D reports the mean value of restatement and firm characteristics for treated, non-treated, and control firms for firms located outside the 8th and 9th Circuits for the matching procedure reported in [Table 10](#) that examines settlement values for those restatements that end up in litigation. For settlement values, firms are matched along the following dimensions: 17-industry (exact match), restatement magnitude, firm size, class period length, share turnover, CAR[−1,1] at restatement, and prior 12-month returns (BHR). A two-tailed t-statistic with standard errors clustered by industry is used to test for a difference in the mean between the treated and non-treated, and treated and control.

Panel A: 8th & 9th Circuits		Restatement Magnitude	Restatement Periods	SEC	Size	Share Turnover	BHR
Positive Bundling: Pre-Dura							
Treated	30	-0.010	10.484	0.033	6.002	0.767	0.010
Non-Treated	68	-0.037	9.179	0.072	5.873	0.768	0.182
T-Statistic		1.68	1.65	0.74	-0.35	-0.02	0.76
Treated	30	-0.010	10.484	0.033	6.002	0.767	0.010
Control	30	-0.008	11.084	0.069	5.852	0.829	-0.090
T-Statistic		0.65	0.65	1.00	-0.78	1.46	-1.27
Matched Distance: 2.650		Non-Matched Distance:		14.096		T-statistic:	-2.23
Positive Bundling: Post-Dura							
Treated	47	-0.044	9.036	0.041	6.056	0.861	0.221
Non-Treated	161	-0.064	13.13	0.08	5.803	0.807	-0.065
T-Statistic		0.51	2.68	-0.91	0.87	1.39	-2.23
Treated	47	-0.044	9.036	0.041	6.056	0.861	0.221
Control	47	-0.037	10.409	0.044	6.218	0.859	-0.027
T-Statistic		-1.49	1.25	0.01	1.41	-0.16	-2.08
Matched Distance: 2.847		Non-Matched Distance:		8.015		T-statistic:	-2.24
Noise Bundling: Pre-Dura							
Treated	72	-0.023	7.989	0.065	5.932	0.751	0.065
Non-Treated	68	-0.036	9.179	0.073	5.873	0.768	0.011
T-Statistic		1.01	-1.78	-0.18	0.20	-0.40	-0.48
Treated	72	-0.023	7.989	0.065	5.932	0.751	0.065
Control	72	-0.014	7.253	0.101	5.773	0.804	0.020
T-Statistic		1.30	-0.88	1.14	-1.03	1.08	-0.24
Matched Distance: 4.552		Non-Matched Distance:		15.299		T-statistic:	-2.77
Noise Bundling: Post-Dura							
Treated	124	-0.030	10.610	0.086	6.210	0.839	-0.073
Non-Treated	161	-0.064	13.138	0.080	5.804	0.806	-0.093
T-Statistic		1.47	1.81	0.19	2.07	1.18	-0.54
Treated	124	-0.030	10.610	0.086	6.210	0.839	-0.073
Control	124	-0.024	10.855	0.076	6.119	0.841	-0.082
T-Statistic		0.91	0.23	-0.39	-0.95	1.22	-0.32
Matched Distance: 2.846		Non-Matched Distance:		4.6803		T-statistic:	-2.34
Panel B: Non- 8th & 9th Circuits		Restatement Magnitude	Restatement Period	SEC	Size	Share Turnover	BHR
Positive Bundling: Pre-Dura							
Treated	78	-0.012	10.880	0.050	7.000	0.768	0.280
Non-Treated	161	-0.029	9.640	0.154	6.576	0.703	0.174
T-Statistic		-1.02	-1.17	2.37	-1.74	-1.99	-1.20

(continued on next page)

Treated	78	-0.012	10.880	0.050	7.000	0.768	0.280
Control	78	-0.007	10.273	0.051	7.157	0.739	0.172
T-Statistic		1.41	-0.93	0.01	1.28	-1.58	-1.39
Matched Distance:	2.673	Non-Matched Distance:		5.694		T-statistic:	-3.18
Positive Bundling: Post-Dura							
Treated	116	-0.014	11.831	0.095	6.634	0.757	-0.011
Non-Treated	277	-0.029	9.284	0.111	6.506	0.727	-0.063
T-Statistic		-1.10	-2.19	0.48	-0.61	-1.09	-1.24
Treated	116	-0.014	11.831	0.095	6.634	0.757	-0.011
Control	116	-0.013	10.070	0.091	6.680	0.789	-0.056
T-Statistic		0.67	-1.75	-1.00	-0.27	1.47	-1.50
Matched Distance:	1.620	Non-Matched Distance:		4.975		T-statistic:	-3.42
Noise Bundling: Pre-Dura							
Treated	171	-0.020	8.930	0.077	6.493	0.697	0.040
Non-Treated	162	-0.029	9.640	0.154	6.576	0.703	0.174
T-Statistic		-0.69	1.00	2.21	0.43	0.17	2.13
Treated	171	-0.020	8.930	0.077	6.493	0.697	0.040
Control	171	-0.018	8.602	0.083	6.586	0.695	0.068
T-Statistic		0.45	-1.08	1.00	1.38	-0.02	0.72
Matched Distance:	2.708	Non-Matched Distance:		5.391		T-statistic:	-2.99
Noise Bundling: Post-Dura							
Treated	244	-0.015	8.991	0.078	6.585	0.771	-0.045
Non-Treated	277	-0.029	9.335	0.111	6.506	0.727	-0.063
T-Statistic		-1.37	0.45	1.27	-0.47	-2.01	-0.42
Treated	244	-0.015	8.991	0.078	6.585	0.771	-0.045
Control	244	-0.020	9.512	0.077	6.562	0.773	-0.059
T-Statistic		-0.97	0.54	-1.00	-0.54	0.16	-0.59
Matched Distance:	2.106	Non-Matched Distance:		3.804		T-statistic:	-2.69
Panel C: 8th & 9th Circuits							
		Restatement Magnitude	Size	Class Period Length	Share Turnover	BHR	CAR[−1,1] at Restatement
All Bundling: Pre-Dura							
Treated	9	-0.028	5.976	5.353	0.915	-0.159	-0.069
Non-treated	6	-0.049	4.512	5.100	0.862	0.079	-0.082
T-Statistic		-0.82	-2.46	-0.11	-0.75	0.80	-0.21
Treated	9	-0.028	5.976	5.353	0.915	-0.159	-0.069
Control	18	-0.042	5.356	5.976	0.888	0.147	-0.106
T-Statistic		1.07	1.56	-1.55	0.67	1.68	1.25
Median Matched Distance:	4.36	Median Non-Matched Distance:		14.64		Chi²	5.65**
All Bundling: Post-Dura							
Treated	13	-0.022	6.620	7.140	0.867	-0.225	-0.150
Non-treated	33	-0.106	6.517	9.873	0.946	-0.038	-0.123
T-Statistic		1.65	-0.19	1.31	1.95	1.05	0.56
Treated	13	-0.022	6.620	7.140	0.866	-0.225	-0.150
Control	26	-0.036	6.443	7.937	0.938	-0.283	-0.160
T-Statistic		-1.32	0.21	0.51	1.37	-0.82	0.07
Median Matched Distance:	5.714	Median Non-Matched Distance:		158.97		Chi²	9.95***
Panel D: Non-8th & 9th Circuits							
		Restatement Magnitude	Size	Class Period Length	Share Turnover	BHR	CAR[−1,1] at Restatement
All Bundling: Pre-Dura							
Treated	20	-0.058	7.152	8.657	0.816	-0.101	-0.126
Non-treated	18	-0.046	6.807	8.202	0.810	0.157	-0.129
T-Statistic		0.43	-0.54	-0.22	-0.08	1.25	-0.07
Treated	20	-0.058	7.152	8.656	0.816	-0.101	-0.126
Control	40	-0.042	6.811	8.372	0.853	-0.200	-0.111
T-Statistic		0.95	-0.93	-0.43	1.06	-1.46	0.62
Median Matched Distance:	2.45	Median Non-Matched Distance:		21.38		Chi²	11.39***
All Bundling: Post-Dura							
Treated	21	-0.039	6.726	6.898	0.906	0.179	-0.139
Non-treated	24	-0.034	7.203	5.727	0.955	-0.125	-0.067
T-Statistic		0.35	1.09	-0.68	1.84	-1.93	2.19
Treated	19	-0.029	6.703	5.621	0.907	0.117	-0.135
Control	38	-0.020	6.831	4.681	0.952	-0.082	-0.103
T-Statistic		1.28	0.45	-0.84	2.10	1.63	1.62
Median Matched Distance:	7.57	Median Non-Matched Distance:		11.78		Chi²	2.834*

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