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# Disclosure Strategies and Shareholder Litigation Risk Evidence From Restatements <sup>1</sup>

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## Abstract

Using a large set of restatement announcements and regulatory filings by U.S.-listed firms between 2003 and 2009, we find evidence that managers aim to reduce litigation risk by (1) bundling negative information, such as earnings restatements, with other public announcements, and (2) leaking negative information prior to announcements. We use a major 2005 Supreme Court decision requiring that plaintiffs link the revelation of negative information to a statistically significant stock price decline as a natural experiment: after the decision, managers increasingly act opportunistically to obfuscate the negative effects of a restatement by increasing how much they bundle and leak information.

JEL: K22, K41, G14, M41

Key words: Litigation risk, class action lawsuits, disclosure, restatements, event study

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# 1 Introduction

## 1.1 Motivation

We use a natural experiment, the U.S. Supreme Court’s 2005 decision in *Dura Pharmaceuticals v. Broudo*, to test how corporate managers protect themselves and their firms from lawsuits. Before this decision, some courts permitted plaintiffs to establish “loss causation”—a legal requirement that losses be caused by a defendant’s misrepresentations—by alleging merely that a firm’s stock price was inflated by those misrepresentations. *Dura Pharmaceuticals* rejected this price inflation theory and instead required plaintiffs to establish a connection between the revelation of information about the alleged fraud and a decline in the firm’s stock price.

As a result of *Dura Pharmaceuticals*, two disclosure strategies became more effective after 2005. First, managers could simultaneously disclose non-fraud-related information along with information that plaintiffs might regard as fraud-related, a practice we call *bundling*. Bundling makes it more difficult for plaintiffs to establish loss causation because it introduces questions about whether the stock price declined because of disclosures about an alleged fraud or because of other information. For example, the court in *Fener v. Belo* dismissed plaintiffs’ claims because it could not discern whether the stock price of the defendant, a publishing firm, declined because of the revelation of information about an alleged fraud or the revelation of bad news about the firm’s business and the publishing industry overall.

Second, managers could disclose some information related to an alleged fraud privately before they disclose it publicly, a practice we call *leakage*. When managers leak negative information, their firm’s stock price tends to decline. Later, when information about an alleged fraud is revealed publicly, the stock price might decline only slightly, or not at all. Skinner (1997) conjectures that managers could use such a leakage strategy but notes that it is understood to be a violation of U.S. securities laws. Our conversations with managers, journalists, and lawyers have generated anecdotal evidence that some managers use these strategies.

The strict and verifiable pleading requirements established by *Dura* were intended to help the courts distinguish between meritorious cases and frivolous lawsuits, thus increasing the economic

benefits of allowing private securities litigation. We believe that the strict standard of *Dura* works toward achieving this goal. Furthermore, the Supreme Court in *Dura* clarified the standards for the lower courts to use and ensured that cases brought in different jurisdictions would be handled with the same legal standards.

In addition to these benefits, however, there are costs associated with the *Dura* standard. We argue that these pleading requirements have unintended effects that reduce the efficiency of information transmission from firms to investors. In particular, we focus on the timing, presentation, and packaging of firms' disclosures to investors. The results of our study suggest that such pleading requirements can induce some firms to game those requirements by deliberately using news-release strategies that make it difficult for plaintiffs to meet those requirements. In particular, firms will become less likely to disclose certain types of news in a clear and timely manner. These news-release strategies can involve defying the SEC's disclosure guidelines and even running afoul of anti-leakage rules that are meant to protect investors.<sup>3</sup> Furthermore, it is quite possible that some meritorious lawsuits will be unviable because managers will have succeeded in creating obstacles for the plaintiffs in meeting the loss causation standards. Those lawsuits that do proceed may involve greater uncertainty at various stages of the litigation, as well as noisier calculations of damages. The perverse incentives can exacerbate moral hazard and agency problems and reduce investors' confidence in capital markets. Empirical evidence of such strategies, or the absence of such strategies, also would help scholars better understand how managers respond to fluctuations in the legal regime and changes in the prospects of litigation.

## 1.2 Background on disclosure and litigation

The literature has identified several reasons for corporate managers to avoid securities litigation. As Bhattacharya, Galpin, and Haslem (2007), Gande and Lewis (2009), and others show, event

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<sup>3</sup>It can be argued that greater information leakage, by itself, can have benefits as well as costs. Clearly there are costs, as evidenced by the laws and regulations against selective disclosure and trading on privileged information. Some observers argue, however, that benefits of information-leakage and insider-trading can come in the form of stock-price efficiency. In this article we do not attempt to resolve the relative benefits and costs of allowing more of this leakage and insider trading. Nevertheless, we believe that the increase in bundling and leakage is at odds with the stated aims of U.S. securities laws.

studies have found that the filing of a lawsuit against a publicly listed firm has an economically and statistically significant negative effect on the firm’s stock price. In the first decade of the twenty-first century, the eight largest securities class action settlements in the U.S. involved settlement amounts totaling nearly \$27 billion.<sup>4</sup> According to Ryan and Simmons (2010), there were 103 securities class action settlements in 2009, with a mean settlement amount of \$37 million. Securities litigation also involves substantial direct costs and expense and shifts managerial time away from other potentially useful projects. Haslem (2005) shows that managers have a personal incentive to avoid corporate litigation, as the discovery process can erode their informational advantage over investors. Lawsuits can also reduce investors’ confidence in the firm’s managers. While Karpoff and Lott (1993) have shown that managers suffer reputational and career penalties from committing fraud, it is likely the case that managers suffer additional adverse effects from highly publicized litigation over an alleged fraud.

Our empirical study of bundling and leakage is motivated by theoretical scholarship from the law literature. Legal scholars have claimed that *Dura* created incentives for managers to protect themselves from shareholder litigation. Spindler (2007) claims that managers might have greater incentives after 2005 to bundle news in order to minimize liability risk. Some recent work in the behavioral finance literature also supports the idea that managers could limit a stock price decline by simultaneously disclosing different types of news. For example, Hirshleifer, Lim, and Teoh (2009) find that investors are “driven to distraction” and underreact to multiple pieces of simultaneous news.

Empirical evidence suggests that both managers and lawyers respond to changes in the legal regime to the extent those changes impact the viability of securities litigation. Graham, Harvey, and Rajgopal (2005) present stylized facts that emerged from a survey administered to several hundred chief financial officers (CFOs) in the United States. The survey results suggested that “...bad news is sometimes delayed to allow in-depth analysis, interpretation, and consolidation into larger news releases.” A considerable fraction of the respondents said they believed in a strategy of packaging bad news with better news (although for many managers, there are other motives, not related to litigation, for this packaging strategy). In the practitioner literature, Buckberg, Foster, and Miller (2005) find that fewer securities class actions were filed in the first few months of 2005 than in

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<sup>4</sup>Although firms are usually covered to some extent by director and officer (D&O) liability insurance, they can still face substantial risks involving low coverage limits or disputes with the insurer.

the previous few months, and that the decline can be attributed to plaintiffs’ lawyers in the Ninth Circuit waiting to observe the *Dura* decision’s effect on loss causation pleading standards before deciding how to proceed.

In addition, plaintiffs’ law firms consider sudden stock price declines in determining which firms to target for class actions.<sup>5</sup> News bundling could be an effective way to deter these law firms by making it more difficult for them to show that the stock price decline was a response only to information related to an alleged fraud. Likewise, a leakage strategy, by reducing or eliminating a stock price decline at the time of a restatement, might deter lawsuits, both because the plaintiffs are less likely to focus on the restatement as potentially fraudulent and because plaintiffs will see that they will confront obstacles to establishing that the earlier stock price decline was caused by the revelation of information about alleged fraud. The immediate price drop is important, as many class action lawsuits are filed relatively quickly after the restatement: approximately one-fourth of the lawsuits in our sample are filed within five days of a restatement, and nearly half are filed within two weeks.

### 1.3 Policy change through a Supreme Court ruling

*Dura Pharmaceuticals* is a particularly interesting natural experiment, because it not only generated an exogenous shift in legal regimes but also resolved a “circuit split” between how different geographic regimes treated securities class actions before 2005. In the case, the plaintiffs claimed that investors purchased shares at an inflated price because the firm’s managers had made false and misleading statements. The Ninth Circuit Court of Appeals had ruled that this claim of price inflation was sufficient to establish “loss causation,” a requirement in securities class actions that the false and misleading statements caused the plaintiffs’ loss. Other Circuit Courts disagreed with the Ninth Circuit, leading to a geographic split in how courts viewed the loss causation requirement.

*Dura Pharmaceuticals*, the defendant, appealed to the U.S. Supreme Court, which agreed in June

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<sup>5</sup>Graham, Harvey, and Rajgopal (2005) mention anecdotal evidence that some law firms use computer programs to identify firms whose stock prices plunge suddenly and are thus inviting targets for a shareholder suit.

2004 that it would hear statements in January 2005. The Supreme Court unanimously reversed the Ninth Circuit in April 2005, holding that a claim of price inflation was not enough to establish loss causation. Instead, the court held that plaintiffs must show that the value of the stock declined as a result of corrected information reaching the market. Many observers and legal scholars, including Spindler (2007), Ferrell and Saha (2007), and Gelbach, Helland, and Klick (2009), interpreted the new standard as requiring a “corrective disclosure” and a subsequent decline in the price of the stock. In other words, in order to establish loss causation, plaintiffs must be able to point to the date when the firm’s actionable statements were corrected, and must then be able to show that investors incurred losses.

Legal scholars such as Spindler (2007) and Ferrell and Saha (2007) have interpreted the Supreme Court’s ruling as a resolution of a dispute among the Circuit courts. For years, the question of what constitutes loss causation in a securities class action had been a contentious issue surrounding litigation. Under the theory motivating our empirics, firms principally located in the Eighth and Ninth Circuits’ jurisdictions should be more affected than firms principally located in other states.<sup>6</sup> The regions covered by these circuits can be seen on the map in Figure 1.

Although in theory a firm can be sued in any federal jurisdiction regardless of its location, empirical evidence shows that in practice, there is relatively little “forum shopping by plaintiffs in securities class actions. Cox, Thomas, and Bai (2009) find that 85 percent of federal securities class actions are filed in the circuit where the firm is principally located<sup>7</sup>; Cox, Thomas, and Bai (2009) explain that the defendant firm can easily relocate the litigation proceedings to its home circuit. We also believe that large firms were differentially impacted by the heightened awareness and concern regarding pleading standards resulting from the *Dura* decision, since large firms are primary targets for lawsuits.

The plaintiffs in *Dura* alleged that the firm had deceived investors through misleading statements about its medical device products, including statements about the probability of gaining approval from the Food and Drug Administration. There are many types of misstatements and subsequent

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<sup>6</sup>The Eighth and Ninth Circuits’ jurisdictions include Alaska, Arizona, Arkansas, California, Hawaii, Idaho, Iowa, Minnesota, Missouri, Montana, Nebraska, Nevada, North Dakota, Oregon, South Dakota, and Washington.

<sup>7</sup>This result holds in our data as well.

corrections that can trigger shareholder class actions, but in this study, we focus on one for which data exist and we can compare firms and disclosures. In particular, we focus on corrective disclosures in the form of restatements of financial reports, which are at the center of many securities class actions.

We follow a growing subfield of the academic literature that focuses on earnings restatements and non-reliance disclosures. In general, accounting restatements have enormous costs. Palmrose, Richardson, and Scholz (2004) show that for a sample of 403 restatements in the late 1990s, the restating firm’s stock price fell by an average of nine percent during the two trading days surrounding the announcement. According to a 2007 report prepared for Congress by the Government Accountability Office, the market capitalization of firms that restated earnings between July 2002 and September 2007 fell by \$36 billion (adjusted for market movements) in the days immediately surrounding the restatement announcements (G.A.O. (2007)). This literature is consistent with the theory that managers benefit from bundling restatements with other news. Myers, Scholz, and Sharp (2010) find less of a stock price decline when an income-reducing restatement is included in an earnings announcement. Files, Swanson, and Tse (2009) find that restatement disclosures that are disclosed discreetly in press releases about other matters are associated with a lower likelihood of shareholder litigation. These findings suggest that when managers are forced to announce troubling news, they benefit from releasing some other news at the same time.

Our goals in this paper are (1) to estimate the extent to which managers use bundling and leakage strategies, and (2) to determine whether *Dura Pharmaceuticals* has increased the use of these disclosure strategies. Our empirical evidence supports the belief that corporate managers, particularly of larger firms and firms located in the Eighth and Ninth Circuits’ jurisdictions, are indeed using these disclosure strategies more than they were before the *Dura* ruling.

## 2 Data

The empirics for this paper involved merging a number of data sets from different sources. The data sources and sample construction are discussed in detail below. See Table 1 for details about the sample construction.



## 2.1 Data sources

### 2.1.1 Non-reliance disclosures and financial restatement announcements

Data on firms' announcements of the need to restate came from the Non-Reliance Restatement database distributed by Audit Analytics. The following passage, taken from the database's documentation, explains the database's contents and how it was assembled:

The restatement data set covers all SEC registrants who have disclosed a financial statement restatement in electronic filings since 1 January 2001. The data has been extracted principally from the following form types: 8-K, 8-K/A, 10-K, 10-Q, 10-Q/A, 10-K/A, 10KSB, 10KSB/A, 20-F, 20-F/A, 40-F and 40-F/As. We analyze all 8-K and 8-K/A filings containing restatement information identified with the title 4.02 Non-Reliance on Previously Issued Financial Statements or a Related Audit Report or Completed Interim Review since August 2004. Amended filings are analyzed by queuing for analysis those filings which contain any of the words restate, restatement or restated. Additional analysis includes the quarterly search of all audit opinions for derivatives of the word restate and the subsequent examination of the corresponding filings.

Our data set includes all restatement announcements made by firms listed on the New York Stock Exchange (NYSE), NASDAQ, and the American Stock Exchange (Amex) from January 1, 2003 to December 31, 2009. To investigate empirically *Dura*'s impact on restatements, we partitioned the restatements sample into "before" and "after" samples: a restatement observation is considered to be post-*Dura* if it takes place after April 19, 2005. We chose January 1, 2003 as the starting date for the "before" sample. Using an earlier starting date would have been problematic, for in the wake of the 2001-2002 wave of accounting scandals, the SEC in the summer of 2002 issued an order requiring CEOs of publicly listed firms to confirm personally that their firms' financial statements were correct. Many restatements from mid-to-late 2002 are likely to have been issued and timed with this SEC order in mind, and so we begin our "before" sample in January 2003.

Other fields in the restatements data set were useful for constructing variables for our investigation. We used the main location of the firm to classify each firm as being located in one of two jurisdictional regions based on the careful analyses of the Circuits' positions by Spindler (2007) and Ferrell and Saha (2007): (1) the Eight and Ninth Circuits, as their loss causation standards were overturned by *Dura*, and (2) all other Circuits. Furthermore, the Audit Analytics data set includes fields that allow us to identify the large firms that make up the Fortune 1000.

### **2.1.2 Securities and Exchange Commission filings**

We downloaded a full list of SEC filings over the period 2003-2009 from the SEC's Edgar FTP site. The list includes identifying information for all SEC registrants along with the dates of all filings and the types of forms that were submitted. We do not extract any data from the individual filings listed; in using this data set, we are interested simply in knowing which forms were filed on which day by a particular firm. The firm-level identifier known as the Central Index Key (CIK) can be used to merge the SEC filings listing with other data sets, particularly the restatement data set described above.

For the restatements in our sample that were announced via an 8-K form filed with the SEC, we pulled data on the form from the SEC WRDS Analytics Suite. This database contains information on the set of items disclosed in a particular 8-K filing. Under SEC guidelines, firms are supposed to issue an 8-K, specifying Item 4.02, or "Non-reliance on previously issued financial statements or a related audit report or completed interim review." It is possible, however, to release an 8-K form with multiple specific items. Using the SEC WRDS Analytics Suite data, we can identify restatements disclosed through multiple-item 8-K forms.

### **2.1.3 Securities class action lawsuit data**

Data on class action lawsuits filed against publicly listed firms were taken from the webpage of Stanford Law School's Securities Class Actions Clearinghouse (SCAC). Our data set contains an observation for each class action filed in a federal court between 2000 and 2010. Each observation lists the name of the defendant firm, the exchange on which the defendant firm's stock is traded,

the ticker symbol of the defendant firm’s common stock, the date on which the lawsuit was filed, and the district court in which the suit was filed.

#### **2.1.4 Firm-level accounting and governance data and stock prices**

For daily stock prices and dividends, we used data from the Center for the Study of Securities Prices (CRSP). Firm-level accounting data, as well as data on firm characteristics, come from the Compustat database, also within WRDS, as well as from Audit Analytics. Our data set includes only firms whose stock is traded on the NYSE, NASDAQ, and Amex.

For some (unreported) tests, we use data on board of director composition from The Directors Database Archive (TDDA). For each SEC-registered corporation in the database, TDDA lists the individual directors along with their titles and certain biographical characteristics. Following the results of Myers, Scholz, and Sharp (2010) and Haslem (2005), we believe that corporate governance and monitoring data can be useful for predicting which firms are likely to self-select into the sample of firms that strategize in timing their financial restatements. When merging the director data with the restatements data set, we joined an observation from the restatements data set with the firm-month observation from the directors data set from December of the previous calendar year.

#### **2.1.5 Earnings surprise data**

In some empirical specifications, we control for the earnings surprise in the firm’s initial earnings announcement. The Institutional Brokers Estimate Survey (I/B/E/S) database contains a field called standardized unexpected earnings (SUE), which captures the deviation between the firm’s actual earnings and the ”consensus” analyst forecast. The SUE score is calculated as

$$(ActualEPS - Mean(ForecastedEPS))/StdDev(ForecastedEPS).$$

For many restatement observations, this value is zero, as they did not take place on the same day as a preliminary earnings announcement. It should be noted that a 10-Q or 10-K often would not be classified as a preliminary earnings announcement, since many firms issue the preliminary

announcement several weeks prior in an 8-K or press release. The SUE score is helpful in controlling for tangible, quantifiable earnings data, as well as in distinguishing between positive and negative news with which a restatement can be bundled.

## **2.2 Data issues**

### **2.2.1 Data inclusion and reliability**

As we are investigating accounting restatement announcements and fraud-on-the-market lawsuit risk, our study relates to the literature on corporate fraud and misconduct. In this empirical literature, identifying episodes of fraud is a known challenge. An important paper by Karpoff, Koester, Lee, and Martin (2012) critiques the use of the popular databases in this literature and highlights some of their limitations. In particular, they highlight reliability issues pertaining to the Audit Analytics database as well as other databases used in this literature, such as the GAO (2003, 2007) restatement database. One such issue is that a restatement is not always the first revelation of an accounting issue—rather, a restatement is often part of a chain of events that raise questions about the firm’s accounting. We acknowledge that restatement announcements do not capture the entire universe of actionable corporate events, and that a restatement may not be the only revelation that can trigger litigation over a single accounting fraud or mishap. For our study, however, even if this is the case for a restating firm, the restatement itself still represents a discrete disclosure event that affects the firm’s litigation risk.

A second issue highlighted by the Karpoff et al (2012) study is that the Audit Analytics database, along with the GAO database, does not capture all of the restatement events that occurred in a given time period. It is most likely the case that the restatements not captured in the Audit Analytics database are on average relatively minor compared to the ones that are included. Although the Audit Analytics database has its limitations, we believe that for the purposes of our study, it is the best available data source.<sup>8</sup>

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<sup>8</sup>The authors of the Karpoff et al (2012) study assembled a database of announcement events related to episodes in which the SEC brought an enforcement action. While such a database solves the problem of restatements being part of a larger chain of events, it would not be ideal for our study, because it limits attention to episodes that resulted in SEC enforcement action, while omitting many other similar episodes that likely involved some risk of private securities litigation (even if such litigation did not actually materialize).

### 2.2.2 Restatement severity

Another issue that arises from our data is that restatements vary considerably in their severity. In our empirical investigation, we use several measures to control for restatement severity: (1) length of restatement period, (2) number of items listed as reasons for the restatement, (3) whether fraud was indicated in the Audit Analytics observation, and (4) change in cumulative earnings brought about by the restatement, scaled by lagged assets. The length of the restatement period is useful as a severity measure because it may correspond to some of the litigation risk involved in the restatement, as a longer length for this window allows for a potentially larger class of investors who purchased their shares during the misstated period.

The third measure of restatement severity, the Audit Analytics fraud coding, has limitations but is still useful for our purposes. As Karpoff, Koester, Lee, and Martin (2012) point out, approximately 98 percent of the restatement observations in the Audit Analytics database are not coded as being the result of deliberate misconduct, according to the data provider’s own criteria (a number that holds for our sample as well). This important statistic is alarming for researchers interpreting the restatement announcements as revelations of fraud or misconduct. 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 announcements that could, in a court of law, be alleged or interpreted as resulting from fraud or misconduct. If anything, we are less interested in the ones resulting from obvious fraud, since if the conduct revealed by the restatement is so blatant that an analyst will classify it as fraud-related (or, alternatively, if the SEC is going to consider it fraud and bring an enforcement action), then the restatement is likely to be so clearly fraud-related that bundling or leaking the news would do little to discourage shareholder litigation. In some empirical tests, we do use this binary variable to proxy for the presence of information in the restatement that clearly indicates that fraud has taken place.

It should be noted that the sample probability of being sued in a shareholder class action, conditional on having restated, is several times the fraction of restatements that are coded as fraud-related by the data provider. One of the main concerns behind the Supreme Court’s ruling in *Dura* was that under the “price inflation” standard, plaintiff lawyers could bring a lawsuit too easily, even when

they did not have much—or any—evidence of actual fraud on the part of the defendant firm at the time of the lawsuit’s filing.<sup>9</sup> It is apparent that some innocent restatements may be subject to litigation risk, along with more clearly fraud-related restatements.

This point is useful to keep in mind in the context of the Hennes, Leone, and Miller (2008) study, which finds that many—if not most—restatements result from minor accounting issues rather than fraud or misconduct. Furthermore, the restatements not involving fraud or misconduct are substantially less likely to lead to management turnover. [In our own tests, we also find that the fraud indicator, and the change in cumulative earnings scaled by lagged assets, predict subsequent management turnover.] Nevertheless, non-fraud-related restatements can still have consequences for the firm, especially if costly litigation ensues, and we believe that many restatement events can involve litigation risk even if they are unlikely to result in the managers losing their jobs. Aside from cases of obvious accounting fraud, where the board has a definite motive to rid the firm of the managers responsible for the fraud, a firm facing litigation may actually have incentives to retain its managers. [These points will be discussed further in Section 4.5.] For this reason as well, we believe that it is appropriate to examine a large set of restatements, not just those that are clearly fraud-related and likely to result in harsh disciplining of managers.

One possible issue that arises is that the distribution of restatement severity may have changed over time, in that more minor restatements are disclosed in the post-*Dura* period. We ran a series of regressions looking for evidence of this, but we did not find such evidence. Univariate results, in fact, indicated that post-*Dura* restatements are *more* severe in terms of their impact on earnings. Another potential issue is that bundled restatements are less severe than non-bundled restatements. For this issue, it is true that the average bundled restatement is less severe than the average non-bundled restatement, and for this reason and others, we aim to control for restatement severity in our empirical tests. Nevertheless, as can be seen in Figure 3, there is substantial overlap in the distributions of restatement magnitude of bundled and non-bundled restatements.

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<sup>9</sup>In the Court’s decision, authored by Justice Stephen Breyer, it is clearly stated that one intention of the Court’s ruling is to limit “the abusive practice of filing lawsuits with only a faint hope that discovery might lead to some plausible cause of action.”

### 3 Methodology

#### 3.1 Strategic timing and packaging of the disclosure

We are particularly interested in knowing whether restating firms became more likely to bundle their corrective restatement announcements with other news, such as earnings filings, as a result of the *Dura* ruling in 2005. The empirical approach we take for this problem is the probit binary choice regression. In the main set of regressions, the dependent variable, *EarningsDay*, is a dummy that equals 1 if the restatement announcement was made on the same day as a Section 10 filing and 0 otherwise. Our intuition suggests that large firms, such as those in the Fortune 1000 index, and firms located in the Eighth and Ninth Circuits' jurisdictions, were differentially affected by *Dura* and so those firms may have changed their disclosure strategies more than other firms. For this reason, we include on the right-hand-side a dummy for whether the firm is part of the fortune 1000, and a dummy for whether the firm is located in the Eighth or Ninth Circuit (these binary variables are called *Fortune* and *Circuit89*, respectively).<sup>10</sup> The empirical approach here is a standard difference-in-difference set-up: as we believe that large firms, and firms in the Eighth and Ninth Circuits' jurisdictions, are differentially affected by *Dura*, the main terms of interest here are the *PostDura \* Circuit89* and *PostDura \* Fortune* interaction terms.

We use a number of control variables on the right-hand side. We control for the restating firm's audit fees, the dollar amount paid by the firm to its outside auditor the previous fiscal year; the main interpretation of this variable is that it captures how difficult or complicated the firm's financial bookkeeping is. We also include an indicator for whether the firm had already issued a restatement in the previous three years, as well as an indicator for whether the firm had been the target of a securities class action suit in the previous three years. Also included as controls are two restatement-process variables provided by Audit Analytics. We use a binary variable that equals 1 if the restatement was initiated by the firm's outside auditor (as opposed to the firm's own accounting unit, management, or the board of directors) and 0 otherwise. In addition, we use a board-initiation binary variable that equals 1 if and only if the firm's board of directors had to approve the restatement announcement before it was made public. The board-initiation variable

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<sup>10</sup>This variable is based on the state in which the firm is principally located, not on the state in which the firm is incorporated.

may capture agency problems or conflicts between management and the board; a firm that has in its corporate charter a clause requiring board approval for an announcement like a restatement may be a firm whose founders recognized the potential for such conflicts. Alternatively, a board-initiation value equal to 1 may simply be an indication of strong corporate governance mechanisms.

There are intuitive and plausible reasons why large firms would be differentially affected by the standard set in the *Dura* decision. First, as Field, Lowry, and Shu (2005), Bauer and Braun (2010), and others have argued, managers of large corporations might have more constant concern about shareholder litigation risk than do managers of small companies. Small companies are not as attractive as targets for law firms specializing in bringing securities class action suits, and so small companies' managers might be less worried about the prospect of inviting shareholder litigation risk. Large corporations' managers, therefore, may be more likely to consider it worth the cost of adjusting their disclosure practices. The "cost" of adjusting the news release practices could take a number of forms. For example, the cost could be that of arousing the suspicions of regulatory authorities.<sup>11</sup> The cost could also be a reputational penalty: investors and other members of the business community may recognize a manager's strategic disclosure timing and give him a reputation as one who is not trustworthy.<sup>12</sup> Furthermore, as the findings of Graham, Harvey, and Rajgopal (2005) suggest, managers can be concerned about developing a consistent style for disclosures; deviating from an earlier disclosure style by starting to bundle bad news with earnings filings can perhaps be considered an adjustment cost.

A second possible reason for large corporations to react more strongly to *Dura* has to do with resources and legal sophistication. Large firms are more likely to have in-house counsel with some sophistication about shareholder litigation risk. Finally, large firms are more likely to employ in-

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<sup>11</sup>As Cheffers, Whalen, and Usvyatsky (2010) point out, in December 2006, a high-ranking SEC accounting official spoke about non-reliance restatement disclosures at a practitioners conference. She noted that a non-reliance disclosure made in an 8-K form, filed the same day as a completed and detailed periodic earnings form, would arouse suspicion among SEC staff. "Generally, the staff believes it is unlikely that a nonreliance conclusion [by the firm's accountants, auditors, or managers] and the filing of restated financial statements can happen within 4 days of one another (Dorsey (2006))."

<sup>12</sup>In the investor relations (IR) practitioner literature, authors strongly advise against such leakage and bundling strategies, partly for these reasons. Consider the advice of Bragg (2010): "If there is a solid block of bad news... , then the best approach is to issue it all at once, rather than dribbling it out over a period of time... If [dribbling] were to be used, then investors would gradually form the opinion that the top management team is not credible..." Bragg further advises: "The worst way to release bad news is to bury it in the financial statement footnotes, in the hope that no one will see it... [A] company's propensity to mask bad news will trickle through the investment community, eventually resulting in the departure of some investors..."



ternal and external public relations personnel whose responsibilities include managing the investor reaction and reputational effects of firm disclosures.

For each firm, Audit Analytics can tell us in which state the firm is principally located. It is important to note that principal location is not the same as state of incorporation; it is quite possible, and common, for a firm to be incorporated in a state other than that of its principal location. A firm can be principally based in, say, North Carolina but incorporated in Delaware. In that case, the Delaware incorporation does not tell us much more than that the firm’s charter complies with Delaware corporate law. For our study, we are interested in the principal location because shareholder suits are usually filed “close to home.”

We estimate the probit model

$$Pr(EarningsDay = 1|X) = \Phi(X'\Gamma), \quad (1)$$

where  $X$  is a vector containing the firm-specific and restatement-specific variables described above, and  $\Phi(\cdot)$  is the cumulative distribution function of the standard normal distribution. The estimation results are shown in Table 6 and will be discussed in Section 4.

Considering that we are interested in the interactions between the post-*Dura*, Eighth/Ninth Circuit, and Fortune 1000 dummy variables in our probit regressions, a note about the econometric issues is in order. The marginal effects of the interaction terms are useful for understanding the effect of *Dura* on restating firms’ propensity to bundle the restatement with earnings news. However, we must take caution not only in calculating the interaction terms’ marginal effects, but in interpreting them as well. Ai and Norton (2003) raise important issues about the mechanics of computing an interaction effect in a non-linear discrete choice model. They provide a procedure for computing the interaction term’s marginal effect that is different from the simple marginal effect procedure built into most statistical software packages. Kolasinski and Siegel (2010), however, argue that using the simple interaction term is appropriate. Furthermore, Puhani (2008) shows that if we are interested in an interaction term to give us an estimate of a treatment effect, then the Ai and Norton (2003) marginal effect calculation is not useful. In presenting our probit results, we will use Puhani’s

marginal effects. Unreported Ai and Norton marginal effects were very similar.

In our investigation of news bundling strategies, we focus on the tactic of bundling restatement news with formal Section 10 (mostly 10-Q and 10-K) filings. It should be noted that most publicly traded firms issue a preliminary earnings release, typically in the form of a press release or 8-K, a few weeks before filing a more detailed and comprehensive Section 10 form. According to Amir and Livnat (2005), in a typical quarter, more than 80 percent of firms will issue these preliminary releases.

There are several reasons why managers may prefer to bundle negative disclosures with Section 10 filings, rather than preliminary releases, if their goal is to mitigate a stock price drop or to obfuscate the impact of the negative disclosure on the stock price. First, the information content of a Section 10 filing is more difficult to interpret, summarize, and quantify than that of the preliminary release. Litigation consultants will often decompose the stock-price effects of bundled announcements. If a Section 10 document's information items are numerous and complex, it becomes more difficult for the plaintiffs' experts to present a compelling estimate of a particular item's stock-price effects. Second, managers have more leeway over the full information package of the Section 10 filing. The preliminary earnings announcement is typically a short press release, essentially a small list of highlights of the upcoming Section 10 filing. However, in the Section 10 filing, managers have more discretion over how they discuss the details of complex transactions or projects, and they have opportunities to use tone and nuance to affect analysts and investors reactions to the reports. Indeed, Davis and Tama-Sweet (2012) find evidence that managers—particularly those who are incentivized to elevate the stock price—prefer to use the Management Discussion and Analysis (MD&A) portion of a Section 10 filing, rather than the preliminary press release, to convey negative information. Third, it is conceivable that for firms that issue preliminary releases, a restatement is less likely to dominate the news cycle if it is bundled with a Section 10 filing than if it is bundled with the preliminary announcement. Li, Ramesh, and Shen (2011) find that financial newswires are less likely to run an article about a firms Section 10 filing if that firm has already issued a preliminary release.

### 3.1.1 Additional tests

Thus far, we have focused on the strategy of releasing a restatement disclosure simultaneously with a long, detailed report such as a 10-K or 10-Q. We also examine the use of bundling strategies in more “transparent” disclosures, particularly those made in 8-K form filings with the SEC.

We believe that this empirical exercise is worthwhile for two reasons. First, bundling news within an 8-K form, released separately from any Section 10 filings, still constitutes news bundling, albeit in a less extreme form than a news release that coincides with, or is part of, a Section 10 filing. The second reason concerns the results of the Files, Swanson, and Tse (2009) study, which shows that some firms will bury a restatement in a press release that is primarily about another matter. By looking at restatements disclosed in 8-K forms, we can examine whether, in response to the *Dura* standard, firms are doing more of the behavior documented by Files, Swanson, and Tse (2009). This approach of examining 8-K restatements is not precisely the same as their approach. But we believe that our approach will capture the news-management effect they document, since in a multi-item 8-K, the more non-restatement items are disclosed, the more likely the restatement is to be buried under other items.

## 3.2 Event study methodology

### 3.2.1 Computing abnormal returns

We use an event study framework for two avenues of investigation: (1) to determine if bundling troublesome news with other news is an effective tool for mitigating the stock price reaction, and (2) to investigate whether information leakage is occurring prior to restatement announcements. For more details on the second avenue of investigation, see subsection 3.3.

Following the event study methodology presented by MacKinlay (1997), we must choose an estimation window and an event window. We use days -83 to -21 in event time to calculate firm-specific  $\alpha$  and  $\beta$  estimates according to the single-factor market model. We use the CRSP variable `vwretd`, defined as the value-weighted market return, as our proxy for the overall stock market performance. We will use the 20 trading days immediately before the announcement, the announcement date itself, and 1 day thereafter as our event window. To gauge the immediate stock price reaction to

the public announcement of the restatement, we use the two-day abnormal return for the two days surrounding the public disclosure, which can be denoted by  $CAR(0, 1)$ . In general, we will use  $CAR(t_1, t_2)$ , where  $t_2 > t_1$ , to denote the stock’s cumulative abnormal return only from dates  $t_1$  to  $t_2$  (inclusive) in event time.

The reason for using the window of  $t = 0$  to  $t = 1$  in event time, rather than simply taking the one-day return for  $t = 0$ , is that although we know the date of the public announcement, we do not know the time of the announcement. It is possible that the news broke before trading hours, or during trading hours, or after trading hours. Numerous event studies on firm announcements use this approach. Unfortunately, having a two-day window allows some noise into the estimate of the stock price reaction to the public restatement announcement.

In Table 8, we present the results of a cross-sectional event study approach to examine whether the news bundling strategy is an effective tool for mitigating the stock price effect of a restatement. For a cross section of restatement announcements, we regress the firm’s two-day abnormal return on a number of restatement-specific and firm-specific variables. We are particularly interested in the coefficient on *EarningsDay*. We expect to find a positive coefficient estimate for this variable. We can also check whether our results are roughly consistent with the findings of Files, Swanson, and Tse (2009) and Myers, Scholz, and Sharp (2010), who find that less transparent disclosure venues can attenuate the market’s reaction to the restatement news.

### 3.2.2 Self-selection concerns

It should be mentioned that a restating firm’s decision to bundle its announcements may not be random or exogenous. In other words, *EarningsDay* may be a treatment into which restating firms endogenously self-select. Propensity score matching (PSM) is an approach that can be used to estimate the causal effect of news-bundling on the stock-price reaction. The empirical approach of Section 3.1 can be used to model the first-stage selection process, where the estimated probability from the probit model is considered the propensity score. In the second stage, each treated observation is matched with an untreated observation based on proximity of propensity scores, with the treatment effect being the difference in two-day abnormal returns between treated and untreated

observations. The results are reported in Table 7, and additional details of our PSM methodology will be discussed in the results section.

Although we do not report the results here, we conducted a set of instrumental variables (IV) tests to incorporate these self-selection concerns. We used board-oversight variables—director busyness (how many other boards the average director sits on), board size, fraction of insiders on the board, and whether the CEO is also the board chairman—as instruments for the firm’s bundling decision. The use of these variables is motivated by the findings of Haslem (2005), who finds that governance and board-oversight variables relate to how litigation-averse managers are, as well as by the findings of Myers, Scholz, and Sharp (2010), who find that governance variables are associated with restatement-disclosure choices. The point estimates from this approach were measured imprecisely and hence were not statistically significant, but they were very similar to the OLS and PSM point estimates.<sup>13</sup>

### 3.2.3 News bundling and likelihood of litigation

We are also interested in whether news bundling is ultimately an effective way to discourage shareholder litigation. We can use the SCAC data to test whether releasing a restatement the same day as a Section 10 filing results in a lower likelihood of litigation than a “stand-alone” restatement. We can estimate a probit binary choice model, where the dependent variable equals 1 if a federal class action lawsuit was filed against the firm within six months of the restatement, and 0 otherwise. For restating firms, we estimate the equation

$$Pr(Sued = 1|X) = \Phi(X'\Lambda), \quad (2)$$

where  $\Phi(\cdot)$  is the cumulative distribution function of a random variable with a standard normal distribution,  $X$  is a vector of firm-specific and restatement-specific variables, and  $\Lambda$  is a vector of coefficients.

The results in Table 8 that have the *Two-day abnormal return* variable on the right-hand side should be taken with an important caveat. The *Two-day abnormal return* variable may be slightly

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<sup>13</sup>Some of these results can be found in previously circulated drafts of this paper.

endogenous, since the stock’s return may incorporate investors’ worries about the possibility of ensuing litigation. However, we believe the endogeneity bias to be second-order, considering that expected litigation costs for sued firms are a relatively small fraction of the market capitalization loss that has occurred. A simple analysis of the two-day abnormal returns around restatements in the pre-*Dura* years, when firms in different circuits were subject to different levels of litigation risk, yields no evidence that restating firms in high-litigation-risk circuits saw sharper stock price falls on average. Furthermore, private conversations with litigation practitioners suggest that accounting for this possible endogeneity is not standard practice in calculating damages.

### 3.2.4 Likelihood of litigation: challenges and alternative approaches

Once again, we must recognize that the decision to bundle a restatement with other news may be an endogenous decision on the part of the managers. Like in Section 3.2.2, we can use an endogenous treatment effects approach. One approach involves using “director busyness” as an exclusion restriction in a control-function set-up to address selection into the *EarningsDay* group. Selection is modeled as a probit and the second stage is a linear probability model, and the model is estimated jointly using maximum likelihood estimation and an assumption of bivariate normality for the error terms. We should note that the second stage here is a linear probability model as opposed to a probit, since binary endogenous regressors in non-linear discrete choice models are inherently problematic. The results from this exercise will not be reported; the point estimates for the effect of news bundling were similar to the OLS result but were measured less precisely and were not statistically significant.

One issue posing a challenge is that the bundled restatements are likely to consist disproportionately of minor or trivial restatements, the type that are less likely to trigger a shareholder lawsuit. We must be mindful of this issue in the contexts of both regression and matching estimates. To some extent, we can mitigate this issue in the regressions by including several right-hand-side control variables that measure the restatement severity. We can also conduct regression and matching tests on a subsample of non-trivial restatements (such as those whose effect on stated earnings is strictly negative, thus ignoring the sizable number of restatements for which this number is zero and the few for which it is positive).

Many of the bundled restatements are likely too minor to trigger litigation, and in the PSM setting, they may be matched with similar (similarly likely to be bundled) restatements, which are also likely to be trivial ones that will not trigger litigation. For these relatively trivial restatements, the sample averages of the outcome variable (whether the firm was subsequently sued) are close to zero for both treated and untreated firms. The estimate for the average treatment effect on the treated (ATT) may be driven toward zero for this reason. It is thus worthwhile to look at the average treatment effect (ATE) as well as the ATT. Simple comparison of sample means for the outcome variable between the treated and untreated groups, restricted to various subsamples based on restatement severity, is also useful in this context. It is also worthwhile to match firms based on restatement severity alone, so as to reduce substantially the problem of the restatement severity affecting the likelihood of litigation beyond the estimate of the firm's propensity to bundle.

### **3.3 Leaking of the information**

#### **3.3.1 Pre-announcement cumulative abnormal returns**

If managers quietly leak sensitive information to investors prior to announcing it publicly, then the information would travel gradually through networks of investors and analysts. For that reason, we can hypothesize that if troubling news is leaked, then the stock price should experience a gradual descent in the days leading up to the public announcement. In short, if leakage is occurring, then we should observe negative cumulative abnormal returns prior to the time of the restatement announcement.

Some existing work has documented negative cumulative abnormal returns on average for restating firms in the days and weeks preceding a restatement announcement. Agrawal and Cooper (2008) even find direct evidence of insider trading by managers prior to restatements. The empirical literature is decidedly mixed on the magnitude and significance of pre-announcement information leakage. Some studies notice negative abnormal returns on average, but find that they are small in magnitude or not statistically significant. That is the conclusion reached by both Palmrose, Richardson, and Scholz (2004) and Hranaiova and Byers (2007). Other studies, however, such as Agrawal and Chada (2005), find a significant negative trend in abnormal returns for restating firms that starts a couple of months before the restatement announcement. Empirical studies generally attribute

to information leakage any pre-announcement negative abnormal returns there are to be found.<sup>14</sup> For example, Agapova and Madura (2011) use abnormal returns prior to issuances of guidance as a measure of information leakage.

One challenge for this study is that we cannot observe directly the occurrence of information leakage. We can only search for evidence of leakage. Furthermore, information leakage can occur in a variety of mechanisms. For instance, leakage can occur through private communications between managers and selected investors, analysts, and journalists. This type of information leakage is generally understood to be inconsistent with securities regulations. Alternatively, leakage can also occur through public communications between the firm and the market, such as through smaller signals or subtle guidance of expectations. Some investor-relations (IR) and securities litigation practitioners refer to this strategy as "dribbling" of the news. Since we can only search for evidence of information leakage, rather than observe leakage directly, we will examine pre-announcement drifts in stock prices. Inducing a slight downward trend in the stock price, it should be recalled, would be the goal of these leakage strategies.<sup>15</sup>

Figure 2 presents a graph of the mean  $CAR$  for restating firms in event time. We can investigate whether certain firms—particularly those that were differentially affected by the change in the courts' standards for loss causation—began to leak more information prior to restatements after *Dura*. This would involve a modified event study regression, in which we regress  $CAR(-2, -1)$  (our rough measure of information leakage) on a similar set of variables as the controls in Table 6. A strongly negative  $CAR(-2, -1)$  is indicative of information leakage; a negative coefficient estimate on a variable suggests that variable predicts more leakage. We perform the analysis for the entire set of restatements in our sample, as well as for a sub-sample of non-bundled restatements (for reasons that will be explained below). We conduct similar tests using the  $(-5, -1)$  event window as well. All of these results are presented in the first two columns in each of Tables 12 through 15.

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<sup>14</sup>We acknowledge that factors other than information leakage could be driving pre-announcement negative returns. Negative abnormal returns prior to the announcement would be consistent with the information leakage story.

<sup>15</sup>We examined firms' issuances of 8-K forms in the weeks leading to the restatement announcement. In this case, 8-K releases can be thought of as bits of other news, not previously scheduled, that the firm can release prior to the restatement. We did not find significant associations between the number of 8-K releases and any of our variables of interest.



There are advantages, and some limitations, of the timing that we use in our leakage measures. In the tables, we present regression results examining CARs starting five trading days before the restatement announcement, as well as CARs starting two trading days before the announcement. Accounting regulations require a firm to release a non-reliance disclosure within four business days of discovering the need for a restatement. We believe that of whatever information leakage there is, much of it will occur in the few days prior to the public announcement.<sup>16</sup> [We also examined, but do not report, CARs for longer pre-announcement windows, but do not find significant associations between those CARs and variables of interest; this result is partly due to greater idiosyncratic stock-price movements incorporated in these longer even windows.]

### 3.3.2 Fractional leakage

We also present estimates of what might be called fractional leakage: the fraction of the stock’s cumulative abnormal return, over the entire event window, that occurs slightly before the announcement. The last two columns of Tables 12 and 13 present regression results with  $CAR(-2, -1)/CAR(-20, +1)$  as the dependent variable. This variable is meant to measure the fraction of the restating firm’s total abnormal return that happened in a couple of days prior to the announcement. As before, we repeated this exercise for the period  $(-5, -1)$ , and these results are presented in Tables 14 and 15.

When focusing on fractional leakage, a complication arises because bundled restatements—which are associated with two-day abnormal returns that are smaller in magnitude—will naturally be associated with higher levels of fractional leakage. For this reason, we present in Tables 13 and 15 regression results for which the sample is limited to “stand-alone” restatements, or restatements that are not bundled with a Section 10 filing.

It should also be noted that we Winsorized the left-hand-side variable at the 5th and 95th percentiles due to the presence of extreme outliers. Results from Winsorizing at the 99th and 1st, and at the 90th and 10th percentiles were similar. Extreme outliers (in some cases, orders of magnitude greater than the observations at the 99th percentile) can arise because this variable is constructed

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<sup>16</sup>In at least one recent high-profile securities class action, there were allegations of pre-announcement information leakage on the same day as the formal announcement.

as a ratio of two small numbers which have their own randomness. A few severe outliers can result from taking a ratio in which the denominator is very close to zero.

## 4 Empirical results

### 4.1 Strategic timing and packaging of the disclosure

The results of the probit parameter estimation suggest that the *Dura* decision had some effects on firms' disclosure strategies, and that those effects were concentrated in large firms and in firms located in the Eighth and Ninth Circuits. Because the probit model is not a linear regression, we unfortunately cannot interpret coefficient estimates as marginal effects; however, the signs and relative magnitudes of parameter estimates can be informative, and we will provide some discussion of the marginal effects.

The first set of probit results is presented in Table 6. This set of results should help us understand the determinants of managers' decisions of whether to bundle a restatement with quarterly or annual reports. The left-hand-side variable, *EarningsDay*, equals 1 if the restatement happened on the same day as a quarterly or annual Section 10 statement. The coefficient on *Fortune* (an indicator for membership in the Fortune 1000) and its marginal effect negative and significant, whereas the marginal effect of the interaction term *PostDura \* Fortune* is positive and of a similar magnitude, and also significant. Together, these results indicate that prior to *Dura*, after controlling for a number of variables, large firms were less likely than others to bundle their restatement news with an earnings announcement, but that result reversed after *Dura*. This result is consistent with the idea that *Dura* had a differential effect on the disclosure strategies of large firms. The coefficient estimate for *Circuit89* (whether the firm is principally located in either the Eighth or Ninth Circuit's jurisdiction) and the marginal effect of its interaction with *PostDura* indicate that restating firms in the Eighth and Ninth Circuits' jurisdictions became more likely to package their restatements with Section 10 filings after April 2005. The triple-interaction term is not significant in the results, suggesting that the *PostDura \* Circuit89* effect is broad-based across the sample firms. The marginal effects results suggest that after *Dura*, Fortune 1000 firms were about 20 percentage points more likely to bundle their restatement news than before the Court decision. The marginal

effects also suggest that firms in the Eighth and Ninth Circuit states were roughly 13 percentage points more likely to bundle their news than before.

A few other interesting findings from this first set of results are worth mentioning. The coefficient on the board-approval variable is negative and very significant, and the coefficient on the auditor-initiation variable is positive and significant. There may be multiple interpretations of the board variable; perhaps firms that require their managers to go through the board of directors give their managers less freedom over the announcements, or perhaps the board variable is merely picking up more general corporate governance quality. The coefficient on the auditor variable may mean that if the firm's internal governance and monitoring mechanisms are weak (as indicated by the fact that it was the outside auditor, not the internal accountants, who discovered the problem with the books), the managers may have more discretion to time the announcement strategically, or the firm may have managers who are more willing to engage in strategic activities related to disclosure. In addition, there is some (albeit weak) evidence, which can be seen in Table 8, that auditor-initiated restatements are associated with more litigation risk, and therefore firms may be more likely to employ bundling tactics for an auditor-initiated restatement. Finally, we should mention that the signs on the coefficient estimates for the variables proxying for the severity of the disclosure may suggest that relatively minor restatement announcements are more likely to be bundled with quarterly or annual filings because minor accounting problems can be discovered and fixed immediately before a scheduled filing.

Table 16 presents the results from a set of regressions examining a less-extreme form of restatement-bundling. In these regressions, the dependent variable is the number of distinct items, including the restatement announcement itself, disclosed in the (Item 4.02) restatement 8-K. The results are generally supportive of the hypothesis that firms responded to Dura by bundling restatement news, even for restatements disclosed in the Item 4.02 8-K form. Interestingly, we find that for this particular form of bundling, the effect of being in the Eighth or Ninth Circuit appears to be more sensitive to firm size (as is evidenced by the significance of the triple-interaction term, rather than simply the  $PostDura * Circuit89$  term) than the more drastic form of news bundling that we discussed previously. When the empirical sample is restricted to Fortune 1000 firms, the regression estimates show as well that firms based in the Eighth and Ninth Circuits were more likely to have

additional items to disclose in their 8-K restatements.

## 4.2 Disclosure and stock price reaction

The next set of regressions, which we present in Table 8, examines the effects of various firm-specific and restatement-specific variables on the restating firm's stock price. The dependent variable is the abnormal return for the two-day window surrounding the restatement announcement. The OLS results indicate that restatements that are bundled with Section 10 filings are associated with higher—or less negative—abnormal returns. In fact, the coefficient estimates suggest that the abnormal returns are 1.8 to 2.2 percentage points higher. This result is consistent with the results of the studies by Files, Swanson, and Tse (2009) and Myers, Scholz, and Sharp (2010).

This set of regressions gives us a chance to examine certain other features of a restatement that are important. One such feature is the presence of quantifiable earnings news, particularly if it is the firm's first announcement of its quarterly or annual earnings. By including the SUE score in the regressions, we can control for the earnings surprise, as well as examine the difference between bundling with positive and negative earnings news. In addition, bundling with a Section 10 filing (such as a 10-Q or 10-K) is not the only type of news-bundling that can occur with a restatement. Most firms issue a preliminary earnings announcement several weeks before the more detailed Section 10 filing; some restatements are bundled with this preliminary filing.

In some regression specifications, we include a separate dummy variable for whether the restatement is bundled with the preliminary earnings announcement. While restatements bundled with a Section 10 have a 2-percentage point higher (less negative) stock-price reaction, the effect from bundling with a preliminary announcement is a more temperate 0.9-1.2 percentage points. Although tests of equality of these two coefficients did not reject the null hypothesis that they are the same, these results are weakly supportive of the notion that bundling with a long and dense Section 10 filing is more effective for protecting the stock price than bundling with a much-anticipated and easily-digested preliminary earnings release.

Not surprisingly, we find that a non-zero earnings surprise affects the stock-price reaction. There

is weak (not statistically significant) evidence of asymmetry of stock-price reaction to positive or negative earnings news. Specifically, in specifications (3) and (4), we find the coefficient estimate on negative SUE score to be 0.016 or 0.017, whereas it is only 0.007 for positive SUE score. However, a test of the equality of these coefficients does not reject the null hypothesis that they are equal.

Table 7 presents PSM estimates to incorporate the non-random nature of firms’ selection of news-bundling. The point estimates are generally similar to those from the OLS results. Some of the issues involved in interpreting the treatment effects here will be discussed in Section 4.3.

### 4.3 Likelihood of litigation

Table 9 presents the results of probit estimates in which the dependent variable equals 1 if the firm became the target of a shareholder class action within six months of the restatement, and 0 otherwise. It should be no surprise that firms with sharply negative two-day abnormal returns are more likely to be sued. We find that larger firms face a higher likelihood of litigation, a result that supports the “deep pockets” hypothesis described by Field, Lowry, and Shu (2005) and Bauer and Braun (2010). There is also some evidence that after controlling for all of these variables and a few others, firms in the post-*Dura* period were less likely to be sued after issuing a restatement. This result holds when including various measures of the severity of the restatement. As expected, we found that after controlling for these other variables, restatements issued on the same day as a Section 10 filing were associated with less likelihood of litigation, and this result is statistically significant in all specifications.<sup>17</sup>

The results from the matching and univariate analyses generally support the hypothesis that news-bundling can reduce the firm’s vulnerability to shareholder litigation. This result can be seen in the univariate analysis presented in Table 10. A noticeable result in this table is that the difference in litigation likelihood between bundled and non-bundled restatements is more pronounced for more severe restatements. When the sample of restatements is partitioned into quintiles, the most severe and second-most severe quintiles see a 10.7-percentage point and 8-percentage point difference in litigation likelihood, both significant at the 10 percent level. This difference is much more dramatic

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<sup>17</sup>In an earlier version of this article, we distinguished between “stealth” restatements that were included in the Section 10 filings, and more transparent announcements in 8-K’s or press releases on the same day as a Section 10. We found the latter group to be associated with a lower likelihood of litigation in our empirical model.

than the 1.9-percentage point difference (not statistically significant) seen for the quintile of the least-severe restatements.

This result is likely driving some of what we see in the matching estimates in Table 11. It is a counter-intuitive result that the estimated average treatment effect (ATE) is greater in magnitude than the estimate average treatment effect on the treated (ATT). In other words, the average episode of bundling appears to reduce litigation risk less than what the reduction from bundling would be for the average restatement in the sample. However, this result can occur when many of the bundled (treated) restatements are ones that are too minor to pose serious litigation risk. This estimation problem is mitigated somewhat by restricting the sample to non-trivial (strictly negative) restatements. Furthermore, when we match restatement observations exclusively on the severity, the ATT and ATE are estimated at a 3.3-percentage point reduction a 5.0-percentage point reduction, respectively, in litigation risk.<sup>18</sup> A compelling explanation is that on average, the cost of bundling a severe restatement is substantially greater than the cost of bundling a trivial one.

## 4.4 Quiet leaking of the information

### 4.4.1 Empirical results

Figure 2 presents a visualization of mean *CAR*'s for restating firms in the pre- and post-*Dura* periods. As is clear from the graph, pre-announcement negative abnormal returns are a larger issue in the post-*Dura* period than in the pre-*Dura* period. This graph by itself, however, does not tell us much about whether the Court's decision is what drives this result. The regression results aim to answer this question by investigating whether firms differentially impacted by *Dura* saw more pre-announcement leakage after the Supreme Court case.

Tables 12 and 13 present regression results that estimate the determinants of pre-announcement *CAR*s for the (-2,-1) window. [In Tables 14 and 15, we present analogous results for the (-5,-1) window.] In the first two columns of results, the left-hand-side variable is the two-day abnormal return covering the days *before* the restatement announcement date. Because the pre-announcement

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<sup>18</sup>Unreported matching results using caliper matching, in which observations were matched with other observations if and only if their propensity scores were within a small scalar distance of one another, were similar.

$CAR$ 's tend to be negative on average, and we interpret the pre-announcement  $CAR$  as a measure of leakage, negative coefficient estimate for a right-hand-side variable suggests that the variable is associated with more severe leakage. The coefficient estimate on the post-*Dura* and *Circuit89* interaction term is negative and significant, which is consistent with Eighth and Ninth Circuit firms responding to the legal regime change through more information leakage.

In the last two columns of these two tables, we present results for regressions in which the left-hand-side variable is the fractional leakage measure described at the end of Section 3. The interactions of *PostDura* with the *Circuit89* and *Fortune* variables are both positive and significant, indicating that large firms and Eighth and Ninth Circuit-based firms saw a larger fraction of their announcement-window abnormal returns happen in the two days prior to the announcement. This result holds in Table 13, when we exclude bundled restatements, which are associated with less stock price movement in the two days surrounding the announcement. This evidence is consistent with the hypothesis that certain firms are reacting to *Dura* through increased leakage, although as we discuss below, we cannot positively rule out alternative explanations.

#### 4.4.2 A note on Regulation Fair Disclosure

It should be noted that privately leaking information to selected analysts and investors is a violation of a number of securities rules, including Regulation Fair Disclosure (Reg FD). Enacted by the SEC in 2000, Reg FD prohibits managers from transmitting information to selected analysts or investors prior to announcing the information publicly to all analysts and investors in the market. According to many observers and commentators, the “selective disclosures” that were outlawed by Reg FD had been common practice in high-tech firms (disproportionately located in California, in the Ninth Circuit) during the late 1990s.

The reason it is important to mention Reg FD when discussing our leakage results is that managers may have perceived time-varying levels of Reg FD enforcement. Although the SEC brought a number of Reg FD enforcement actions between 2002 and the first half of 2005, there were no additional Reg FD enforcement actions until late 2009. In other words, Reg FD, which is effectively an anti-leakage ordinance, did not appear to be enforced visibly in our post-*Dura* sample period as it had been in the pre-*Dura* period. Whether time-varying Reg FD enforcement bears responsibility

for changes in the amount of information leakage over our sample period is a question we leave for additional research.

#### 4.5 A note on personal incentives for disclosure strategies

In this study, our motivating hypothesis is that the *Dura* standard's strict requirement induces managers to use non-forthright disclosure techniques that involve a cost to the firm or its managers. The recent study by Files (2012) finds evidence that non-forthright disclosure of a restatement raises the stakes for managers in terms of SEC penalties: specifically, a non-forthright disclosure is less likely to result in SEC penalties, but if the firm is penalized, the punishment will be greater. If managers are risk-averse, then raising the stakes with the SEC can be thought of as a cost that managers bear by using non-forthright disclosure strategies.

From the Execucomp database, we pulled data on managers' levels of compensation and also constructed indicators of management turnover in the year surrounding the restatement. We did not find that, conditional on a restatement being disclosed, bundling and leakage are associated with higher rates of CEO or CFO turnover. Nor did we find compelling evidence that the sensitivity of turnover to disclosure strategy has changed in the post-*Dura* period.<sup>19</sup> One interesting finding that is more robust is that if a firm has already been sued in the 3 years prior to the restatement, the CEO is more likely to be replaced. This result suggests that firms that have already been taken to task for accounting issues will allow less room for error on the part of their managers.

We also looked for evidence of a less-severe form of discipline: reductions in executive compensation. Similarly, we found no evidence that bundling or leakage was associated with subsequent post-restatement pay reductions, or that any such relationship emerged after 2005. An interesting result that emerged from this exercise is that auditor-initiated restatements were associated with subsequent pay reductions, suggesting that managers who appear ineffective at keeping the firm's books in order (as evidenced by the outside auditor's need to initiate the restatement) are punished via pay reductions.

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<sup>19</sup>There was some weak statistical evidence that post-restatement turnover rates have gone down in the post-*Dura* period, particularly at small firms, but this relationship is not robust.



The lack of significant results for management turnover is not surprising, as the expected results are ambiguous. On one hand, it is plausible that by using non-transparent disclosure, a manager can lose the confidence of the board or the firm’s investors, or draw the ire of regulators, and be at risk of losing his job. On the other hand, counter-intuitively, a firm that becomes exposed to higher litigation risk may be *less* likely to dismiss its manager; if the manager remains in the firm’s employ, the firm can exert more control over his statements during the litigation proceedings, or he will simply cooperate more. It is not clear, therefore, whether we should expect more management turnover in firms that disclose restatements through controversial bundling and leakage strategies. Despite these insignificant empirical results, we believe that non-forthright disclosure strategies involve risks and costs to the firm and its managers, even if the risks and costs are open to interpretation. Reputational damage and loss of credibility are perhaps the most likely of such risks and costs. For risk-averse managers, raising the stakes with regard to SEC punishment can be considered a significant cost. Furthermore, recent accounting studies have argued that managers prefer to develop consistent styles for making financial disclosures (e.g. Bamber, Jiang, and Wang (2010)), and having to deviate from one’s style can also be considered a cost.

## 5 Discussion

For years, there has been concern in legal and accounting circles that corporate managers could insulate themselves from litigation risk by using certain controversial disclosure strategies. We have examined managers’ use of two such strategies: packaging troublesome news with other news, and quietly leaking the troublesome news to the market prior to a disclosing the news formally. There has long been an intuition among lawyers and academics that mitigating the stock price drop attributable to troubling news may reduce the risk of a shareholder class action occurring. The Supreme Court’s decisive ruling in 2005 in the case of *Dura Pharmaceuticals v. Broudo* defined and underscored the requirements for securities class action plaintiffs to establish loss causation. At the time of this Supreme Court case, and since then, concerns about managers’ use of questionable disclosure strategies has heightened. Furthermore, the recent studies by Files, Swanson, and Tse (2009) and Myers, Scholz, and Sharp (2010) have found evidence that disclosure packaging can affect stock price reactions; furthermore, Files, Swanson, and Tse (2009) show evidence that disclosing a restatement discreetly and quietly can reduce litigation risk. Our results indicate that the legal

regime affects managers' decisions of whether to use these disclosure strategies.

Our empirical investigation finds that prior to the *Dura* decision, large and prominent firms were less likely to package their restatement news with earnings news. However, it appears that after the 2005 Court decision, the firm size effect went away, and large firms were more likely to bundle news than they were before. These results are consistent with the belief that large firms are more likely to be targeted in securities class actions, and so large firms' managers are more likely to consider it worth the cost of adopting these disclosure strategies. Furthermore, our results suggest that firms based in the jurisdictions of the Eighth and Ninth Circuits (whose loss causation standards were overturned in *Dura*) changed their disclosure behaviors more than firms in other locations. We then examine whether the pre-announcement movements of stock prices are indicative of some leakage of the sensitive information. Our results for the leakage investigation suggest that negative information making its way into stock prices prior to formal announcements—particularly for large firms and firms in the Eighth and Ninth Circuits—may be more of a practical issue now than at the time of the Supreme Court's important *Dura* decision in 2005. As for whether this increased observed leakage is partly due to perceived weak enforcement of Regulation Fair Disclosure, we pose this as a separate research question.

Our results have implications for the empirical literature on restatements, which often assumes that restatements impact the stock price at the time of the announcement and afterward. Policy makers and the courts should also be aware that managers have these news release strategies at their disposal, and that news bundling appears to be an effective tool both for protecting the firm's stock price and for discouraging litigation from taking place. The imposition of strict pleading standards was intended to stop inefficient transfers from firms to the plaintiffs' bar, but it appears to have decreased the timeliness and clarity of information that investors receive from firms. Our results may have the normative implication that the courts should consider relaxing the *Dura* loss causation pleading requirements if there are multiple pieces of firm-specific news occurring simultaneously or if there is evidence of a pre-announcement downward drift in the share price. In establishing legal precedents and statutes, courts and policy makers should also be aware that managers can and do choose their disclosure strategies to adapt to the litigation environment.

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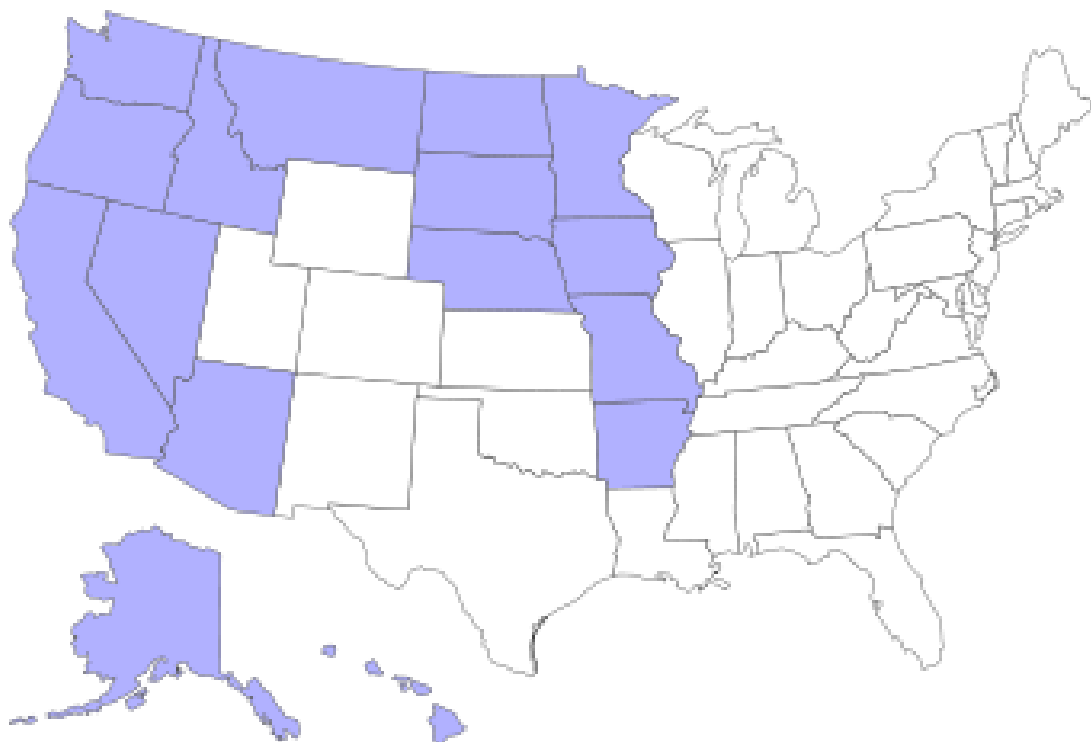
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Figure 1: Jurisdictions of U.S. Courts of Appeals for the Eighth and Ninth Circuits



The states in the Eighth and Ninth Circuits’ jurisdictions 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.

Figure 2: Cumulative abnormal returns, pre- and post-*Dura*

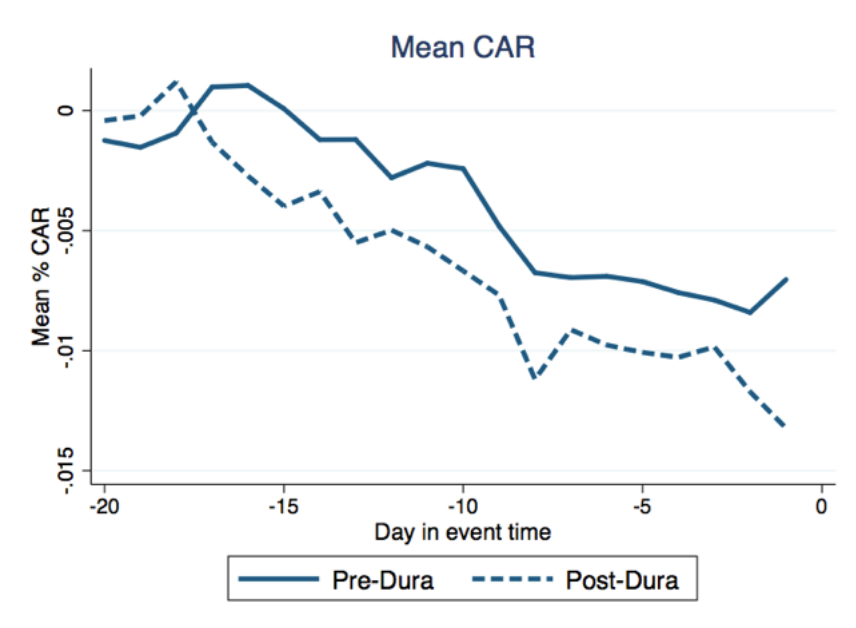


Table 1: Sample construction and attrition

Data source merged	Reason for dropping observations	# Obs dropped	Obs remaining in sample
Audit Analytics (obs. with populated data fields)			2,310
CRSP	Missing stock price data	681	1,629
The Directors Database Archive	Missing directors data	335	1,294
SEC Edgar	Missing filings information	3	1,291



Table 2: Industry classifications of firms in restatement sample

Industry	Number	Percentage of sample
Consumer durables	34	2.6%
Consumer Nondurables	55	4.3%
Energy	69	5.3%
Finance	244	18.9%
Health and Medicine	95	7.4%
High-Tech	234	18.1%
Manufacturing	142	11.0%
Telecom	34	2.6%
Utilities	58	4.5%
Wholesale/Retail	174	13.5%
Other	152	11.8%
Total	1291	100%

Notes: The “Other” category includes mining, construction, transportation, hotels, business services, building materials, and entertainment. These industry classifications were made according to the restating firm’s four-digit SIC code and the lists of industry definitions available on Kenneth French’s website: [http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data\\_library.html](http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html)

Table 3: Form in which restatement is announced

Form	No.	Percentage of sample
10-K	120	9.30%
10-K/A	90	6.97%
10-Q	64	4.96%
10-Q/A	93	7.20%
10-QT	1	0.08%
10KSB	3	0.23%
10KSB/A	8	0.62%
10QSB	2	0.15%
10QSB/A	6	0.46%
20-F	31	2.40%
20-F/A	15	1.16%
40-F	2	0.15%
40-F/A	4	0.31%
6-K	13	1.01%
8-K	583	45.16%
8-K/A	9	0.70%
ARS	1	0.08%
NT 10-K	27	2.09%
NT 10-Q	38	2.94%
NT 20-F	1	0.08%
NTN 10Q	1	0.08%
Press Release	178	13.79%
S-1	1	0.08%
Total	1291	100%

Table 4: Characteristics of firms in restating sample

Variable	Mean	Std. Dev.	Min.	Max.	N
Market cap (millions USD)	3607.96	18943.84	0.96	386402.06	1291
Assets (millions USD)	10682.25	84167.63	2.47	1908635.25	1291
Audit fees (millions USD)	2.3	6.82	0.02	90.2	1291
Firm is in Fortune 1000	0.22	0.41	0	1	1291
US firm	0.89	0.31	0	1	1291
Firm is in 8th or 9th Circuit	0.29	0.45	0	1	1291
Firm is in 2nd, 3rd, or 11th Circuit	0.23	0.42	0	1	1291
Listed on NYSE	0.4	0.49	0	1	1291
Listed on AMEX	0.07	0.26	0	1	1291
Listed on NASDAQ	0.53	0.5	0	1	1291

Table 5: Restatement-specific variables

Variable	Mean	Std. Dev.	Min.	Max.	N
Restatement on same day as Section 10 filing	0.25	0.43	0	1	1291
Restatement included in Section 10 filing	0.17	0.38	0	1	1291
Restatement not in Section 10 filing but bundled with it	0.08	0.27	0	1	1291
Multiple SEC filings on restatement day	0.41	0.49	0	1	1291
Year 2003	0.1	0.29	0	1	1291
Year 2004	0.1	0.3	0	1	1291
Year 2005	0.24	0.43	0	1	1291
Year 2006	0.18	0.38	0	1	1291
Year 2007	0.18	0.38	0	1	1291
Year 2008	0.11	0.32	0	1	1291
Year 2009	0.09	0.29	0	1	1291
Post-Dura	0.69	0.46	0	1	1291
Restatement effect on stated earnings* (Winsorized)	-0.01	0.02	-0.1	0.01	1291
Fraud indicated	0.02	0.13	0	1	1291
SEC investigation pending	0.11	0.31	0	1	1291
Restatement has negative effect on books	0.82	0.39	0	1	1291
Restatement has positive effect on books	0.18	0.39	0	1	1291
Number of quarters restated	9.32	8.65	1	89	1291
Number of reasons cited for restatement	2.44	1.58	0	13	1291
CAR(-20,-1)	-0.01	0.15	-0.70	1.15	1291

\* Defined as cumulative change in earnings, scaled by lagged assets.

Table 6: Probit regression results, LHS variable = EarningsDay

	Probit1 Coeff/SE	Probit2 Coeff/SE	Probit3 Coeff/SE	MFX Coeff/SE
Firm is in Fortune 1000	-0.53*** (0.19)	-0.57*** (0.19)	-0.67*** (0.20)	-0.16*** (0.04)
Firm is in 8th or 9th Circuit	-0.43** (0.20)	-0.42** (0.20)	-0.41** (0.20)	-0.11** (0.05)
Circuit89 * Fortune	0.52 (0.42)	0.62 (0.45)	0.59 (0.46)	0.20 (0.17)
Post-Dura	-0.03 (0.11)	0.15 (0.12)	0.12 (0.12)	0.03 (0.03)
Post-Dura * Circuit89	0.38* (0.22)	0.47** (0.23)	0.47** (0.23)	0.15* (0.08)
Post-Dura * Fortune	0.42* (0.23)	0.67*** (0.24)	0.66*** (0.24)	0.22** (0.09)
Post-Dura * Circuit89 * Fortune	-0.09 (0.50)	-0.43 (0.53)	-0.34 (0.53)	-0.09 (0.11)
US firm	-0.43*** (0.12)	-0.19 (0.12)	-0.25** (0.13)	-0.08* (0.04)
Restatement required board approval		-1.12*** (0.10)	-1.06*** (0.10)	-0.31*** (0.03)
Outside auditor initiated restatement		0.19* (0.10)	0.21* (0.11)	0.06** (0.03)
Firm had restatement in previous 3 years		-0.00 (0.08)	-0.01 (0.09)	-0.00 (0.02)
Securities class action in past 3 years		-0.05 (0.15)	-0.05 (0.15)	-0.01 (0.04)
Restatement improves numbers			-0.02 (0.11)	-0.01 (0.03)
Number of reasons cited for restatement			-0.02 (0.03)	-0.01 (0.01)
Number of quarters restated			-0.01** (0.01)	-0.00** (0.00)
Log audit fees			0.01 (0.04)	0.00 (0.01)
Restatement effect on stated earnings			7.93*** (2.66)	2.30*** (0.77)
Avg boards per director			0.18* (0.10)	0.05* (0.03)
Constant	-0.20 (0.13)	-0.19 (0.15)	-0.30 (0.49)	
Number of observations	1291	1291	1291	1291
Pseudo-R2	0.02	0.13	0.14	0.14
Chi-square	34.02	183.79	213.45	213.45
Standard errors	robust	robust	robust	robust

Notes: Dependent variable is *EarningsDay*, which equals 1 if the restatement was disclosed on the same day as a Section 10 filing, and 0 otherwise. *Circuit89* equals 1 if the firm is headquartered in the jurisdiction of either the Eighth or Ninth Circuit, and 0 otherwise. The last column, labeled “MFX,” contains computed marginal effects (evaluated at the sample means of all right-hand-side variables) from the “Probit3” specification in the previous column. These marginal effects were calculated with the Puhani (2008) method for interaction terms in a non-linear binary choice model; unreported computed marginal effects using the Ai and Norton (2003) method were qualitatively similar. The symbols \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.

Table 7: PSM Estimates of News-Bundling on Stock-Price Reaction

First-stage model	Predicted abnormal return			SE	t-stat	N
	Bundled	Not bundled	ATT			
<i>Basic NN matching</i>						
Probit1	.00526	−.0364	.0416	.0190	2.19**	1,291
Probit2	.00526	−.0236	.0289	.0162	1.78*	1,291
Probit3	.00526	−.0111	.0163	.00698	2.34***	1,291
<i>NN matching, only on restatement magnitude</i>						
On restatement mag., full sample	.00526	−.00863	.0139	.0344	0.40	1,291
On restatement mag., strictly neg.	.00558	−.0290	.0345	.0104	3.31***	849

Notes: Dependent variable is the two-day abnormal return surrounding the restatement announcement, as defined in the text. The three different first-stage models, labeled Probit1, Probit2, and Probit3, correspond to the models estimated in Table 6. \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.

Figure 3: Severity of bundled vs. stand-alone restatements

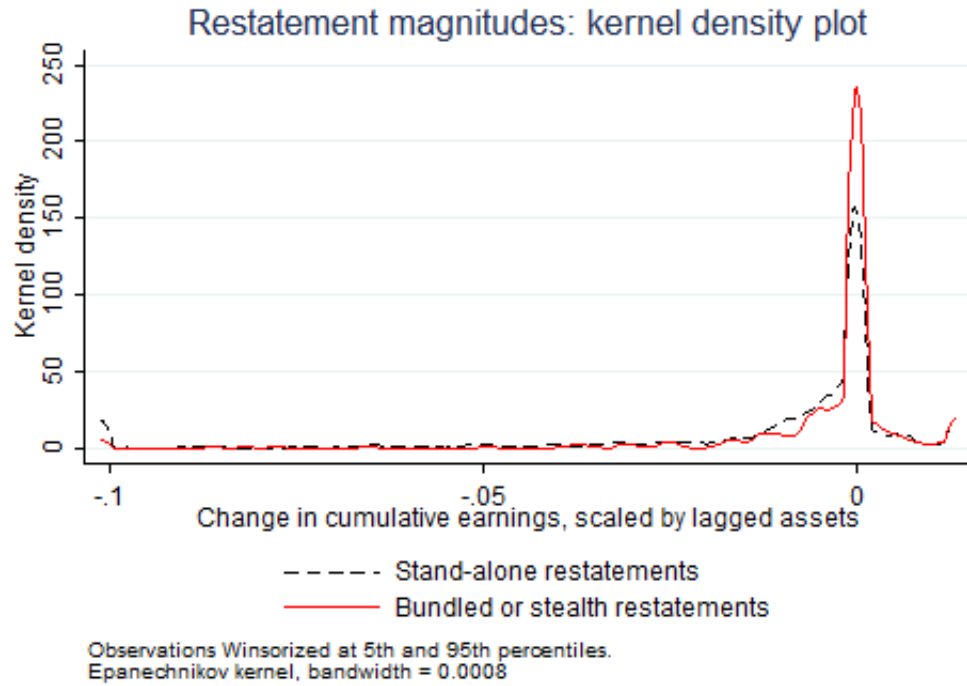


Table 8: Stock price reaction to restatement disclosures: LHS variable = 2-day abnormal return

	(1) Coeff/SE	(2) Coeff/SE	(3) Coeff/SE	(4) Coeff/SE
Restatement on same day as Section 10 filing	0.020*** (0.005)	0.022*** (0.005)	0.022*** (0.005)	0.018*** (0.006)
Bundled with prelim earnings		0.009 (0.006)	0.013** (0.006)	0.012* (0.007)
Firm is in Fortune 1000	0.006 (0.007)	0.004 (0.007)	0.002 (0.007)	0.003 (0.007)
Firm is in 8th or 9th Circuit	-0.001 (0.010)	-0.002 (0.010)	-0.000 (0.010)	-0.001 (0.009)
Circuit89 * Fortune	-0.003 (0.014)	-0.003 (0.014)	-0.008 (0.015)	-0.007 (0.015)
Post-Dura	-0.003 (0.006)	-0.003 (0.006)	-0.003 (0.006)	-0.001 (0.006)
Post-Dura * Circuit89	0.000 (0.011)	0.001 (0.011)	-0.002 (0.011)	0.001 (0.011)
Post-Dura * Fortune	-0.012 (0.011)	-0.011 (0.010)	-0.010 (0.010)	-0.007 (0.010)
Post-Dura * Circuit89 * Fortune	-0.006 (0.023)	-0.007 (0.023)	0.004 (0.021)	0.001 (0.020)
US firm	-0.002 (0.008)	-0.002 (0.008)	-0.002 (0.008)	-0.001 (0.007)
SUE score * positive			0.007*** (0.003)	0.007*** (0.003)
SUE score * negative			0.017*** (0.004)	0.016*** (0.004)
Restatement required board approval				-0.010* (0.006)
Outside auditor initiated restatement				0.005 (0.005)
Firm had restatement in previous 3 years				-0.001 (0.004)
Securities class action in past 3 years				-0.003 (0.008)
Number of reasons cited for restatement				0.001 (0.001)
Number of quarters restated				-0.000 (0.000)
Log audit fees				-0.002 (0.002)
Restatement effect on stated earnings				0.128*** (0.036)
Constant	-0.010 (0.007)	-0.011 (0.007)	-0.012* (0.007)	0.015 (0.028)
Number of observations	1291	1291	1291	1291
R-squared	0.02	0.02	0.06	0.07
Adjusted R-squared	0.01	0.01	0.05	0.06
Standard errors	robust	robust	robust	robust

Notes: The left-hand-side variable is the restating firm's abnormal stock return (calculated using a single-factor market model) over the two-day period from  $t=0$  to  $t=1$  in event time, where  $t=0$  is the restatement-announcement date. The variables "SUE score \* positive" and "SUE score \* negative" refer, respectively, to the earnings surprise variable interacted with an indicator for whether the SUE score is positive or negative. The symbols \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.

Table 9: Estimation results, LHS variable = Sued

	Probit1A Coeff/SE	Probit1B Coeff/SE	Probit2 Coeff/SE	MFX Coeff/SE
Restatement on same day as Section 10 filing	-0.66** (0.29)	-0.66** (0.29)	-0.57* (0.32)	-0.03*** (0.01)
Section 10 bundled * Post-Dura * Circuit89	0.52 (0.43)	0.53 (0.43)	0.19 (0.58)	0.05 (0.06)
Bundled with prelim earnings	-0.10 (0.15)	-0.11 (0.20)	-0.24 (0.22)	-0.01 (0.01)
Firm is in Fortune 1000	-0.09 (0.27)	-0.06 (0.28)	-0.09 (0.31)	-0.01 (0.02)
Firm is in 8th or 9th Circuit	0.50* (0.27)	0.49* (0.27)	0.44 (0.28)	0.04 (0.03)
Circuit89 * Fortune	-0.64 (0.59)	-0.58 (0.59)	-0.40 (0.63)	-0.03** (0.01)
Post-Dura	-0.32 (0.21)	-0.31 (0.22)	-0.26 (0.22)	-0.02 (0.02)
Post-Dura * Circuit89	-0.43 (0.33)	-0.41 (0.34)	-0.30 (0.34)	-0.02 (0.02)
Post-Dura * Fortune	0.08 (0.35)	0.07 (0.36)	0.06 (0.40)	0.01 (0.03)
Post-Dura * Circuit89 * Fortune	1.11 (0.70)	1.00 (0.71)	0.73 (0.74)	0.18 (0.20)
US firm	0.11 (0.24)	0.07 (0.24)	0.27 (0.27)	0.01 (0.01)
Outside auditor initiated restatement	0.31* (0.16)	0.34** (0.16)	0.47** (0.19)	0.02** (0.01)
Number of quarters restated	0.00 (0.01)	0.00 (0.01)	-0.00 (0.01)	0.00 (0.00)
Number of reasons cited for restatement	0.04 (0.04)	0.05 (0.04)	-0.02 (0.04)	0.00 (0.00)
Log audit fees	0.23*** (0.05)	0.24*** (0.06)	0.25*** (0.06)	0.02*** (0.00)
Restatement effect on stated earnings	-9.75*** (2.17)	-9.80*** (2.17)	-6.82*** (2.11)	-0.66*** (0.18)
Fraud indicated	0.43 (0.34)	0.45 (0.34)	0.44 (0.30)	0.04 (0.05)
Earnings surprise score * positive		-0.17 (0.11)	-0.32* (0.17)	
Earnings surprise score * negative		-0.12* (0.07)	-0.11 (0.07)	
Constant	-5.30*** (0.76)	-5.31*** (0.79)	-5.31*** (0.82)	
Number of observations	1291	1291	849	1291
Pseudo-R2	0.15	0.17	0.14	0.15
Standard errors	robust	robust	robust	robust

Notes: Left-hand-side variable is a binary variable that equals 1 if the restating firm was the target of a securities class action suit within six months of the restatement announcement and 0 otherwise. The first two columns of results, labeled “Probit1A” and “Probit1B,” present the coefficient estimates from probit regressions. The third column of results presents estimates from a similar probit run on the subsample of strictly negative restatements. The right-most column, labeled “MFX,” presents marginal effects estimates from Probit1A. The symbols \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.

Table 10: Lawsuit occurrence, bundled vs. non-bundled restatements

Restatement severity Quintile (1=most severe)	Fraction sued				
	Frac. bundled	Bundled	Not bundled	Diff.	T-stat
1	0.104	0.045	0.153	0.107	1.37*
2	0.155	0.030	0.111	0.081	1.43*
3	0.204	0.023	0.064	0.041	1.06
4	0.220	0.021	0.042	0.021	0.66
5	0.267	0	0.019	0.019	1.05
Full sample	0.192	0.019	0.079	0.060	3.13***

This table presents within-quintile mean-comparisons between bundled and bundled restatements, where the outcome variable equals one if the firm was sued by shareholders within six months of the restatement, and zero otherwise. The quintiles are based on restatement severity, as measured by the cumulative change in earnings scaled by lagged total assets. Restatements for which this value is zero, as well as restatements that are flagged as fraud-related by Audit Analytics, are excluded. \*, \*\*, and \*\*\* represent statistical significance at the 10-percent, 5-percent, and 1-percent levels, respectively. N=1,093

Table 11: PSM Estimates of News-Bundling on Lawsuit Occurrence

First-stage model	Sample probability of lawsuit		ATT	t-stat	ATE	t-stat	N
	Bundled	Not bundled					
<i>NN matching, full sample</i>							
Probit1	.015	.064	−.045***	-3.62	−.049***	-3.90	1,291
Probit3	.015	.064	−.015	-1.09	−.032*	-1.87	1,291
<i>NN matching, strictly neg. restatements</i>							
Probit3	.026	.093	−.022	-0.87	−.074**	-2.37	849
<i>NN matching, only on restatement magnitude</i>							
On restatement magnitude	.015	.064	−.033**	-2.36	−.050***	-3.60	1,291

Notes: Outcome variable equals one if the firm was sued within six months of the restatement, zero otherwise. The three first-stage models labeled Probit1 and Probit3, correspond to the models estimated in Table 6. Restatement magnitude is defined as the cumulative change in earnings, scaled by lagged assets. \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.

Table 12: Regression estimates: Pre-announcement leakage (full sample)

Dependent variable:	CAR(-2,-1)		$CAR(-2, -1)/CAR(-20, +1)$	
	(1) Coeff/SE	(2) Coeff/SE	(3) Coeff/SE	(4) Coeff/SE
Firm is in Fortune 1000	0.001 (0.003)	0.001 (0.003)	-0.195** (0.079)	-0.173** (0.081)
Firm is in 8th or 9th Circuit	0.006 (0.004)	0.006 (0.004)	-0.155* (0.089)	-0.150* (0.090)
Circuit89 * Fortune	0.008 (0.006)	0.007 (0.007)	0.055 (0.180)	0.054 (0.180)
Post-Dura	-0.000 (0.003)	0.000 (0.003)	-0.138** (0.057)	-0.154*** (0.060)
Post-Dura * Circuit89	-0.010** (0.005)	-0.010** (0.005)	0.222** (0.102)	0.214** (0.102)
Post-Dura * Fortune	0.001 (0.005)	0.001 (0.005)	0.246** (0.103)	0.243** (0.104)
Post-Dura * Circuit89 * Fortune	0.003 (0.010)	0.002 (0.010)	-0.039 (0.226)	-0.030 (0.225)
US firm	0.003 (0.003)	0.004 (0.003)	-0.027 (0.054)	-0.046 (0.055)
Restatement required board approval		-0.005** (0.002)		0.048 (0.047)
Outside auditor initiated restatement		0.000 (0.002)		0.008 (0.051)
Firm had restatement in previous 3 years		0.000 (0.002)		-0.004 (0.037)
Securities class action in past 3 years		-0.000 (0.003)		-0.041 (0.055)
Number of reasons cited for restatement		-0.001 (0.001)		-0.020* (0.012)
Number of quarters restated		0.000 (0.000)		-0.000 (0.002)
Log audit fees		0.001 (0.001)		-0.008 (0.015)
Restatement effect on stated earnings		0.029 (0.049)		-0.286 (0.875)
Constant	-0.006 (0.003)	-0.010 (0.011)	0.234*** (0.065)	0.396* (0.210)
Number of observations	1291	1291	1291	1291
R-squared	0.01	0.02	0.01	0.01
Adjusted R-squared	0.01	0.01	0.00	0.00
Standard errors	robust	robust	robust	robust

Notes: This table presents event-study regression results on stock price movement in the days before a restatement announcement. For regressions (1) and (2), the dependent variable is CAR(-2,-1), or the stock's cumulative abnormal return from day t=-2 to t=-1 in event time. For regressions (3) and (4), the dependent variable is CAR(-2,-1) as a fraction of the stock's cumulative abnormal return over the (-20,+1) period in event time. The LHS variable in regressions (3) and (4) has been Winsorized at the 5th and 95th percentiles due to the presence of extreme outliers. The symbols \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.



Table 13: Regression estimates: Pre-announcement leakage (stand-alone restatements)

Dependent variable:	CAR(-2,-1)		$CAR(-2,-1)/CAR(-20,+1)$	
	(1) Coeff/SE	(2) Coeff/SE	(3) Coeff/SE	(4) Coeff/SE
Firm is in Fortune 1000	0.003 (0.004)	0.002 (0.004)	-0.169** (0.085)	-0.118 (0.088)
Firm is in 8th or 9th Circuit	0.008* (0.004)	0.009** (0.004)	-0.187** (0.095)	-0.177* (0.096)
Circuit89 * Fortune	0.004 (0.007)	0.004 (0.007)	0.019 (0.203)	-0.001 (0.204)
Post-Dura	0.004 (0.003)	0.003 (0.003)	-0.170*** (0.065)	-0.170** (0.071)
Post-Dura * Circuit89	-0.014*** (0.005)	-0.014*** (0.005)	0.271** (0.111)	0.261** (0.111)
Post-Dura * Fortune	-0.002 (0.005)	-0.002 (0.005)	0.256** (0.116)	0.249** (0.118)
Post-Dura * Circuit89 * Fortune	0.013 (0.011)	0.013 (0.011)	-0.086 (0.257)	-0.083 (0.254)
US firm	0.006 (0.004)	0.007* (0.004)	-0.044 (0.062)	-0.062 (0.063)
Restatement required board approval		-0.006** (0.003)		0.068 (0.056)
Outside auditor initiated restatement		0.002 (0.003)		-0.058 (0.064)
Firm had restatement in previous 3 years		0.002 (0.002)		-0.026 (0.042)
Securities class action in past 3 years		-0.001 (0.004)		-0.066 (0.059)
Number of reasons cited for restatement		-0.001 (0.001)		-0.027** (0.013)
Number of quarters restated		-0.000 (0.000)		0.000 (0.002)
Log audit fees		0.001 (0.001)		-0.018 (0.018)
Restatement effect on stated earnings		0.048 (0.050)		-0.565 (0.871)
Constant	-0.011*** (0.004)	-0.016 (0.012)	0.275*** (0.076)	0.610** (0.240)
Number of observations	968	968	968	968
R-squared	0.02	0.03	0.01	0.02
Adjusted R-squared	0.01	0.02	0.00	0.01
Standard errors	robust	robust	robust	robust

Notes: This table presents event-study regression results on stock price movement in the days before a restatement announcement. For this table, the sample is limited to restatement announcements that were NOT made on the day of a Section 10 filing. For regressions (1) and (2), the dependent variable is CAR(-2,-1), or the stock's cumulative abnormal return from day  $t=-2$  to  $t=-1$  in event time. For regressions (3) and (4), the dependent variable is CAR(-2,-1) as a fraction of the stock's cumulative abnormal return over the (-20,+1) period in event time. The LHS variable in regressions (3) and (4) has been Winsorized at the 5th and 95th percentiles due to the presence of extreme outliers. The symbols \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.

Table 14: Regression estimates: Pre-announcement leakage (full sample)

Dependent variable:	CAR(-5,-1)		$CAR(-5, -1)/CAR(-20, +1)$	
	(1) Coeff/SE	(2) Coeff/SE	(3) Coeff/SE	(4) Coeff/SE
Firm is in Fortune 1000	-0.002 (0.005)	-0.002 (0.005)	-0.350*** (0.100)	-0.380*** (0.103)
Firm is in 8th or 9th Circuit	0.002 (0.006)	0.002 (0.006)	-0.277** (0.118)	-0.279** (0.120)
Circuit89 * Fortune	0.011 (0.011)	0.011 (0.011)	0.593** (0.271)	0.600** (0.276)
Post-Dura	-0.003 (0.004)	-0.001 (0.004)	-0.105 (0.075)	-0.099 (0.079)
Post-Dura * Circuit89	-0.001 (0.008)	-0.001 (0.008)	0.279** (0.135)	0.276** (0.136)
Post-Dura * Fortune	0.005 (0.007)	0.005 (0.007)	0.416*** (0.133)	0.414*** (0.135)
Post-Dura * Circuit89 * Fortune	-0.008 (0.016)	-0.011 (0.016)	-0.426 (0.324)	-0.449 (0.327)
US firm	0.001 (0.005)	0.002 (0.005)	-0.085 (0.075)	-0.071 (0.077)
Restatement required board approval		-0.005 (0.004)		-0.033 (0.063)
Outside auditor initiated restatement		-0.003 (0.004)		-0.010 (0.069)
Firm had restatement in previous 3 years		0.003 (0.003)		-0.008 (0.050)
Securities class action in past 3 years		0.004 (0.005)		0.051 (0.069)
Number of reasons cited for restatement		0.000 (0.001)		-0.001 (0.016)
Number of quarters restated		0.000 (0.000)		0.003 (0.003)
Log audit fees		-0.000 (0.001)		0.019 (0.021)
Restatement effect on stated earnings		0.083 (0.076)		-0.368 (1.126)
Constant	-0.003 (0.005)	-0.001 (0.018)	0.447*** (0.088)	0.175 (0.275)
Number of observations	1291	1291	1291	1291
R-squared	0.00	0.01	0.02	0.02
Adjusted R-squared	-0.00	-0.00	0.01	0.01
Standard errors	robust	robust	robust	robust

Notes: This table presents event-study regression results on stock price movement in the days before a restatement announcement. For regressions (1) and (2), the dependent variable is CAR(-5,-1), or the stock's cumulative abnormal return from day t=-5 to t=-1 in event time. For regressions (3) and (4), the dependent variable is CAR(-5,-1) as a fraction of the stock's cumulative abnormal return over the (-20,+1) period in event time. The LHS variable in regressions (3) and (4) has been Winsorized at the 5th and 95th percentiles due to the presence of extreme outliers. The symbols \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.

Table 15: Regression estimates: Pre-announcement leakage (stand-alone restatements)

Dependent variable:	CAR(-5,-1)		$CAR(-5, -1)/CAR(-20, +1)$	
	(1) Coeff/SE	(2) Coeff/SE	(3) Coeff/SE	(4) Coeff/SE
Firm is in Fortune 1000	-0.001 (0.006)	-0.002 (0.006)	-0.300*** (0.112)	-0.340*** (0.118)
Firm is in 8th or 9th Circuit	0.002 (0.007)	0.003 (0.007)	-0.197 (0.129)	-0.201 (0.132)
Circuit89 * Fortune	0.008 (0.012)	0.007 (0.012)	0.504 (0.310)	0.530 (0.322)
Post-Dura	-0.001 (0.005)	-0.001 (0.005)	-0.123 (0.090)	-0.137 (0.097)
Post-Dura * Circuit89	-0.002 (0.009)	-0.002 (0.009)	0.199 (0.149)	0.198 (0.151)
Post-Dura * Fortune	0.004 (0.008)	0.004 (0.008)	0.373** (0.152)	0.387** (0.156)
Post-Dura * Circuit89 * Fortune	0.003 (0.018)	0.004 (0.018)	-0.416 (0.369)	-0.467 (0.373)
US firm	0.007 (0.006)	0.007 (0.006)	-0.138 (0.089)	-0.125 (0.091)
Restatement required board approval		-0.004 (0.005)		-0.074 (0.077)
Outside auditor initiated restatement		-0.001 (0.005)		0.051 (0.089)
Firm had restatement in previous 3 years		0.004 (0.003)		0.013 (0.057)
Securities class action in past 3 years		0.001 (0.006)		0.041 (0.077)
Number of reasons cited for restatement		-0.000 (0.001)		-0.010 (0.018)
Number of quarters restated		0.000 (0.000)		0.005* (0.003)
Log audit fees		0.001 (0.001)		0.016 (0.025)
Restatement effect on stated earnings		0.130* (0.078)		-0.192 (1.179)
Constant	-0.010 (0.006)	-0.020 (0.020)	0.485*** (0.111)	0.254 (0.328)
Number of observations	968	968	968	968
R-squared	0.00	0.01	0.01	0.02
Adjusted R-squared	0.00	0.00	0.00	0.00
Standard errors	robust	robust	robust	robust

Notes: This table presents event-study regression results on stock price movement in the days before a restatement announcement. For this table, the sample is limited to restatement announcements that were NOT made on the day of a Section 10 filing. For regressions (1) and (2), the dependent variable is CAR(-5,-1), or the stock's cumulative abnormal return from day t=-2 to t=-1 in event time. For regressions (3) and (4), the dependent variable is CAR(-5,-1) as a fraction of the stock's cumulative abnormal return over the (-20,+1) period in event time. The LHS variable in regressions (3) and (4) has been Winsorized at the 5th and 95th percentiles due to the presence of extreme outliers. The symbols \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.

Table 16: Regression results for number of items in 8-K restatements

Subsample: restatements disclosed in 8-K forms	LHS: number of items in 8-K form			
	Full subsample		Fortune 1000 firms	
	(1) Coeff/SE	(2) Coeff/SE	(3) Coeff/SE	(4) Coeff/SE
Firm is in Fortune 1000	-0.00 (0.16)	-0.01 (0.15)		
Firm is in 8th or 9th Circuit	-0.04 (0.14)	-0.11 (0.14)	-0.26 (0.29)	-0.27 (0.22)
Circuit89 * Fortune	-0.24 (0.32)	-0.17 (0.29)		
Post-Dura	0.05 (0.11)	-0.12 (0.11)	-0.12 (0.17)	-0.28 (0.19)
Post-Dura * Circuit89	-0.00 (0.18)	0.05 (0.18)	0.76* (0.40)	0.93*** (0.34)
Post-Dura * Fortune	-0.20 (0.21)	-0.20 (0.20)		
Post-Dura * Circuit89 * Fortune	0.79* (0.44)	0.76* (0.42)		
US firm	0.33* (0.19)	0.33* (0.18)	0.15 (0.34)	0.12 (0.36)
Log audit fees	0.14*** (0.03)	0.12*** (0.03)	0.11 (0.09)	0.11 (0.09)
Restatement required board approval		0.44*** (0.11)		0.51** (0.22)
Outside auditor initiated restatement		0.11 (0.12)		-0.00 (0.23)
Securities class action in past 3 years				-0.28 (0.21)
Firm had restatement in previous 3 years				-0.05 (0.17)
Restatement improves numbers				0.47* (0.26)
Number of reasons cited for restatement				0.16*** (0.05)
Number of quarters restated				-0.01 (0.01)
Fraud indicated				-0.49 (0.52)
Restatement effect on stated earnings				4.99 (5.44)
Constant	-0.02 (0.46)	-0.12 (0.45)	0.51 (1.23)	-0.09 (1.25)
Number of observations	660	660	169	169
R2	0.04	0.08	0.03	0.21
Adj. R2	0.03	0.06	0.00	0.14
F-stat	3.37	7.27	1.27	3.35
Standard errors	robust	robust	robust	robust

Notes: The sample in these regressions is the subset of restatements that were disclosed in an 8-K form and not on the same day as a Section 10 filing. Dependent variable is the number of separate, discrete items (including the restatement itself) disclosed in the form. *Circuit89* equals 1 if the firm is headquartered in the jurisdiction of either the Eighth or Ninth Circuit, and 0 otherwise. The symbols \*, \*\*, and \*\*\* denote statistical significance at the 10%, 5%, and 1% levels respectively.