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The Effect of Managerial Litigation Risk on Earnings Warnings: Evidence from a Natural Experiment

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ABSTRACT

We examine the causal effect of managerial litigation risk on managers' disclosure of earnings warnings in the face of large earnings shortfalls. Exploring the staggered adoption of universal demand (UD) laws as an exogenous decrease in litigation risk, we find that the adoption leads to a decrease in managers' issuance of earnings warnings, especially among firms facing a higher litigation risk prior to the adoption. In contrast, we find no change in managers' tendency to alert investors of impending large positive earnings surprises. Collectively, our results provide causal evidence that higher litigation risk incentivizes managers to issue more earnings warnings. Our results differ from Bourveau et al.'s finding of an increase in the frequency of management earnings forecasts after the adoption of UD laws. We reconcile our findings with theirs by demonstrating that the effect of adopting UD laws on manage-

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ment earnings forecasts depends critically on forecast horizon: The adoption increases long-horizon forecasts, but decreases short-horizon forecasts.

JEL codes: K41, M41

Keywords: litigation risk; earnings warning; universal demand laws; management earnings forecast; forecast horizon

1. *Introduction*

Earnings warnings have long attracted attention from market participants and researchers. Distinct from other disclosures, earnings warnings are managers' voluntary disclosure of significant bad earnings news shortly before announcements of earnings, especially large negative earnings surprises (Skinner [1994]). They trigger substantial stock price drops and are closely monitored by investors and analysts.[1] As discussed below, the economic forces shaping managers' warning decisions are fundamentally different from those for long-term forecasts. Because of their economic importance, warnings have inspired a stream of literature devoted to understanding managers' motivation for issuing these unique, preemptive disclosures.[2] Since the seminal work of Skinner [1994], shareholder litigation has long been recognized as an important determinant of managers' incentives to issue earnings warnings, especially when they are facing large earnings shortfalls. However, direct causal evidence on the impact of shareholder litigation risk on earnings warnings is surprisingly rare (see section 2 for a review of this literature). In this study, we provide new direct evidence on the causal effect of managerial legal risk on earnings warnings.

The economic forces governing managers' disclosure decisions differ fundamentally for long-term forecasts versus earnings warnings. For long-term forecasts, the main benefits include reduced information asymmetry and lower costs of capital, whereas the main costs include increased litigation costs and proprietary costs. Long-term forecasts *increase* litigation costs because managers possess less precise information about long-term earnings, and their long-term forecasts can turn out to be inaccurate or misleading ex post and thus trigger litigations (Healy and Palepu [2011], Bourveau, Lou, and Wang [2018]). In contrast, for earnings warnings, Skinner [1994] proposes that their primary benefit is the reduced litigation and reputation costs associated with withholding bad news. Warnings *decrease* litigation costs because managers possess relatively precise information about earnings in the short period prior to the earnings announcement (making it less likely that the forecasts will be inaccurate or considered misleading

[1] Warnings account for a significant fraction of management earnings forecasts for firms facing large negative earnings surprises—in our sample of firm-quarters with large negative earnings surprises, warnings account for 49% of all quarterly forecasts and 30% of all forecasts.

[2] See, for example, Skinner [1994, 1997], Francis et al. [1994], Kasznik and Lev [1995], Libby and Tan [1999], Field et al. [2005], Tucker [2007], Donelson et al. [2012], and Billings and Cedergren [2015].

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ex post) and issuing warnings weakens the claim that managers hide bad earnings news and shortens the class period (Skinner [1994]).[3] Therefore, a lower litigation risk would induce managers to issue *more* long-term forecasts by decreasing their litigation cost, but *fewer* warnings by decreasing their litigation-reduction benefit.

We explore the staggered adoption of universal demand (UD) laws as an exogenous decrease in managerial litigation risk. UD laws require shareholders to seek board approval prior to initiating derivative lawsuits against corporate directors and officers for breach of fiduciary duty. These derivative lawsuits are highly relevant to corporate disclosures because more than 90% of public company derivative lawsuits are related to disclosure issues (Erickson [2010]). However, because the lawsuits typically name the directors themselves as defendants, the board rarely grants this approval. Thus, the adoption of UD laws significantly reduces shareholder litigation risk (Appel [2019]). Further, adoption occurs at the state level and is exogenous to individual firms' disclosure decisions. Therefore, the setting of UD laws adoption helps address the endogeneity of litigation risk and errors in measuring litigation risk using industry and firm characteristics (Kasznik and Lev [1995], Field, Lowry, and Shu [2005]). The staggered adoption across different states also offers cleaner identification than a single event (Johnson, Kasznik, and Nelson [2001]).

Consistent with prior research (e.g., Bourveau, Lou, and Wang [2018], Appel [2019]), we identify 13 states that adopted UD laws at different points in time over the period of 1995 to 2010. We focus on firm-quarters with large earnings shortfalls (denoted hereafter as bad-news quarters), defined as the difference between the consensus analyst forecast during the 30 days after the prior-quarter earnings announcement and the ultimate reported earnings being larger than 1% of the firm's market value (Field, Lowry, and Shu [2005]). Our test sample includes 469 bad-news quarters from the firms in these 13 states between 1995 and 2010, with 132 in the preadoption window and 337 in the postadoption window. For each bad-news quarter in our test sample, we select a matched bad-news quarter in the same quarter and industry from firms incorporated in the states that do not have UD laws over the sample period based on propensity score matching (PSM) over a variety of covariates potentially associated with firms' tendency to issue warnings.

Our difference-in-differences (DID) estimation shows that after the adoption of UD laws, the probability that managers warn investors of impending large earnings shortfalls declines by 13.1 percentage points for test firms relative to control firms. This decline in earnings warnings is persistent and economically significant, representing a 59.5% decrease from

[3] On the other hand, most firms facing large earnings shortfalls decide not to warn (e.g., Tucker [2007]), suggesting that issuing warnings is costly to managers and/or firms. See section 3.2 for a discussion of the costs of issuing warnings.

the base rate for an average treatment firm in the preadoption period. Further, we find that the decrease in earnings warnings for test firms relative to control firms is concentrated in firms facing higher ex ante derivative lawsuit risk prior to the adoption of UD laws.

We next conduct similar tests of managers' alerts of impending large good earnings news. Using 284 matched pairs of quarters with large positive earnings surprises (denoted hereafter as good-news quarters), we find no change in good-news alerts after the adoption of UD laws for test firms relative to control firms, and this result holds regardless of whether a firm faces higher or lower ex ante derivative lawsuit risk prior to the adoption. Because lawsuits are usually triggered by large negative earnings surprises but not by large positive surprises, this result from testing good-news alerts increases our confidence that the decrease in earnings warnings we find is attributable to the adoption of UD laws reducing litigation risk, rather than to other confounding factors that may also have changed around the adoption of UD laws.

To address the concern that our bad-news sample is small (469 treatment firm-quarters) and thus the finding may not be generalizable, we examine whether our finding also holds in two larger treatment samples of bad-news quarters. The first broader sample contains 2,484 firm-quarters in the treatment states with negative earnings surprises (i.e., consensus analyst forecast > actual earnings). The second one contains 6,078 firm-quarters in the treatment states with market-adjusted returns beginning two days after the prior earnings announcement and ending one day after the current earnings announcement lower than -1% (Roychowdhury and Sletten [2012]). We find consistent results using both samples. In additional robustness tests, we show that our finding for warnings is robust to using alternative matching methods, a generalized DID approach based on the full sample of bad-news quarters (e.g., Bourveau, Lou and Wang [2018]), the sample period of 1998 to 2010 (Chuk, Matsumoto, and Miller [2013]), and two alternative windows to measure warnings. In addition, we find that the adoption of UD laws is associated with a decrease in the timeliness of overall bad earnings news, measured using Donelson et al.'s [2012] approach.

Our finding of a decrease in earnings warnings after the adoption of UD laws differs from Bourveau, Lou, and Wang's [2018] finding of an increase in the frequency of management earnings forecasts. However, it is important to note that the two studies examine managerial earnings forecasts of different horizons. Bourveau, Lou, and Wang [2018] test all forecasts including both annual and quarterly forecasts. They do not examine annual and quarterly forecasts separately; nor do they examine earnings warnings. As discussed above, the effect of managerial litigation risk on management earnings forecasts depends critically on the forecast horizons: Higher litigation risk will prompt managers to issue fewer regular, long-horizon forecasts to lower the chance of being sued for issuing forecasts that turn out to be overly optimistic ex post, but more short-horizon forecasts such as

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earnings warnings to avoid being sued for withholding bad news (Skinner [1994]).

To test our prediction and to reconcile our findings with Bourveau, Lou, and Wang [2018], we follow their design to first replicate their finding and then examine how the effect of UD laws adoption on management earnings forecasts varies with forecast horizon. First, confirming Bourveau, Lou, and Wang [2018], we find an increase in the frequency of all forecasts after the adoption. However, testing annual and quarterly forecasts separately, we find that the frequency increase is much larger for annual forecasts than for quarterly forecasts. Second, an interesting pattern emerges after we split quarterly forecasts based on forecast horizon: The shorter the horizon, the more negative the effect of the adoption on the forecast frequency. Specifically, we observe a significant increase in the frequency of long-horizon quarterly forecasts (i.e., forecasts issued before the prior-quarter announcement), a significant but economically small decrease in the frequency of medium-horizon quarterly forecasts (i.e., forecasts issued during the 30 days after the prior-quarter announcement), and a significant and economically larger decrease in the frequency of short-horizon forecasts (i.e., forecasts issued between 30 days after the prior-quarter announcement and the current announcement). Third, we find that the increase in the frequency of long-horizon quarterly forecasts is mainly driven by good-news forecasts, whereas the decrease in the frequency of short-horizon forecasts is mainly driven by bad-news forecasts. Overall, these results indicate that higher litigation risk decreases long-horizon forecasts, particularly good-news forecasts, but increases short-horizon forecasts, particularly bad-news forecasts. These results support our prediction that managerial litigation risk influences long-term forecasts and earnings warnings in contrasting ways. On the one hand, when the forecast horizon is long and the final earnings number is uncertain, managers issue forecasts to reduce information asymmetry and are mainly concerned about the risk of their forecasts being construed as misleading ex post. In this case, higher litigation risk leads to fewer forecasts, especially of good news. On the other hand, when the horizon is short, managers issue forecasts to preempt bad earnings news and to update market expectations with the material private information they have (Heitzman, Wasley, and Zimmerman [2010]) and are primarily concerned about the risk of being sued for withholding bad news. Thus, higher litigation risk leads to more short-horizon bad-news forecasts.

Our study makes two primary contributions to the literature. First, it contributes to the earnings warning literature. The most important question in this literature is what motivates mangers to issue warnings. Although Skinner's [1994] litigation reduction hypothesis is economically appealing, prior research has documented mixed evidence on the effect of litigation risk on warnings, primarily because of the joint endogeneity of litigation risk and warnings and errors in measuring litigation risk (e.g., Kasznik and Lev [1995], Johnson, Kasznik, and Nelson [2001], Field, Lowry, and Shu

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[2005]). To our knowledge, our study provides the first clear causal evidence of the effect of litigation risk on warnings. Our results provide strong support for Skinner's [1994] litigation reduction hypothesis.

Second, our study adds to our understanding of how litigation risk influences management earnings forecasts in general. Prior research examines the "average" effect of litigation on management forecasts. In contrast, we propose and show that the effect of litigation on management forecasts depends critically on forecast horizon, switching from a negative effect for long-term forecasts to a positive effect for short-term forecasts including warnings. Our findings demonstrate the two opposing effects that litigation can exert on disclosure as summarized by Healy and Palepu [2011].[4] Thus, the net effect of litigation risk on disclosure depends critically on which force dominates—the benefit of reducing litigation related to withholding information or the cost of increasing litigation related to releasing inaccurate or misleading information.

Recent studies exploring shocks to litigation risk document somewhat conflicting findings. For example, Houston et al. [2019] find that higher litigation risk increases management earnings forecasts and this result is driven by bad-news forecasts. In contrast, Bourveau, Lou, and Wang [2018] find that higher litigation risk decreases management earnings forecasts and this result holds for both good-news and bad-news forecasts. Our findings highlight the key role of forecast horizon in determining the net effect of litigation risk on management forecasts, and thus potentially provide an explanation for the seemingly conflicting results documented by existing studies.

Sections 2 and 3 discuss prior research and our setting and hypothesis. Section 4 describes our research design. Section 5 presents our results, and section 6 reconciles our finding with Bourveau, Lou, and Wang's [2018]. Section 7 concludes.

2. *Prior Research*

Extant literature on the effect of litigation on disclosure has examined both earnings warnings and general managerial disclosures. We discuss both strands of research below.[5]

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[4] More specifically, Healy and Palepu [2011] conclude: "The threat of shareholder litigation can have two effects on managers' disclosure decisions. First, legal actions against managers for inadequate or untimely disclosures can encourage firms to increase voluntary disclosure. Second, litigation can potentially reduce managers' incentives to provide disclosure, particularly of forward-looking information" (pp. 422–423).

[5] A related stream of research examines how managerial disclosures affect litigation costs, also producing mixed results (e.g., Francis et al. [1994], Skinner [1997], Field et al. [2005], Donelson et al. [2012]).

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2.1 EFFECT OF SHAREHOLDER LITIGATION ON EARNINGS WARNINGS

In his seminal work, Skinner [1994] proposes that shareholder litigation risk affects managers' voluntary disclosure incentives.[6] Although later studies have applied Skinner's [1994] argument to a broader scope of disclosure, Skinner [1994] focuses on earnings warnings—earnings forecasts that managers issue to preempt large negative earnings surprises within a short period prior to earnings announcements. He proposes that managers have an incentive to warn investors of impending earnings shortfalls because warnings reduce litigation costs by mitigating the perception of managers withholding bad news and by shortening the class period. Consistent with this litigation reduction hypothesis, he finds that firms with bad earnings news are more than twice as likely to issue earnings warnings as firms with good earnings news are to issue good-news alerts.

Kasznik and Lev [1995] also find that managers preempt large negative earnings surprises more often than other types of earnings news, especially among firms in industries with higher litigation risk. Field, Lowry, and Shu [2005] simultaneously model the effect of litigation risk on earnings warnings and vice versa, using actual lawsuits to capture expected litigation risk and the industry legal exposure as an instrument. They find that firms with higher litigation risk are more likely to issue warnings. In contrast, examining a sample of high-tech firms around the passage of the Private Securities Litigation Reform Act of 1995 (PSLRA), which was designed to reduce frivolous lawsuits based on the voluntary disclosure of forward-looking information, Johnson, Kasznik, and Nelson [2001] find an increase in bad-news earnings forecasts of a short horizon (defined as less than one quarter ahead of the relevant fiscal period, including earnings warnings), consistent with higher litigation risk leading to fewer warnings.

The mixed evidence is primarily because of two empirical challenges. First, litigation risk is endogenously determined by many observable and unobservable firm and industry characteristics, which may introduce bias into association tests. Second, it is challenging to measure expected litigation risk. Using industry and firm characteristics to capture litigation risk can introduce considerable measurement errors. Johnson, Kasznik, and Nelson [2001] address the endogeneity issue by examining the passage of PSLRA. Their evidence, however, is based on a simple pre-post comparison around a single event, and thus the results can be confounded by the time trend or concurrent confounding factors. We add to this literature by providing new direct evidence on the causal effect of litigation risk on earnings warnings. We explore the staggered adoption of UD laws across states that results in an arguably exogenous shock to managerial litigation risk. Our DID design further mitigates the concern that our findings are confounded by time trends or concurrent events.

[6] Lev [1992] proposes a similar argument.

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2.2 EFFECT OF SHAREHOLDER LITIGATION ON GENERAL CORPORATE DISCLOSURES

Prior studies examining how litigation risk affects broader corporate disclosures (other than earnings warnings) have also generated mixed results (e.g., Johnson, Kasznik, and Nelson [2001], Baginski, Hassell, and Kimbrough [2002], Rogers and Van Buskirk [2009], Cao and Narayanamoorthy [2011]). On the one hand, some studies find that higher litigation risk is associated with fewer disclosures. For example, Johnson, Kasznik, and Nelson [2001] find a significant increase in the frequencies of earnings and sales forecasts following the passage of the PSLRA, consistent with managers providing more forecasts when expected litigation risk is lower.[7] Baginski, Hassell, and Kimbrough [2002] compare management earnings forecasts issued in the U.S. and Canadian markets, and conclude that a less litigious environment induces Canadian firms to provide more forecasts of both bad and good news. Rogers and Van Buskirk [2009] find that firms issue fewer management earnings forecasts after being sued, consistent with litigation risk deterring managers' voluntary disclosure of forward-looking information. Bourveau, Lou, and Wang [2018] show that the adoption of UD laws, which reduces shareholder litigation risk, leads to more corporate disclosures, measured with management earnings forecasts, voluntary 8-K filings, and the length of management discussion and analysis (MD&A) in 10-K filings.

On the other hand, several recent studies that explore different natural experiments find that higher litigation risk increases disclosures. Using three legal events, Houston et al. [2019] find that the treated firms issue more management earnings forecasts relative to control firms when they expect litigation risk to be higher. Naughton et al. [2019] use the U.S. Supreme Court ruling in *Morrison v. National Australia Bank* to measure an exogenous reduction in expected private litigation costs for foreign cross-listed firms and find that the ruling decreases management forecasts. Boone, Fich, and Griffin [2019] find that after the adoption of UD laws, financial statements become more opaque, the accuracy of management forecast decreases, analyst dispersion and forecast error increase, and bid-ask spreads and informed trading increase.[8]

Although our study focuses on warnings, we also add to the literature on general corporate disclosure by examining how the effect of litigation on management earnings forecasts varies with forecast horizon. We conjecture and find that the effect depends critically on forecast horizon. This result reconciles our finding with Bourveau, Lou, and Wang's [2018] and could

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[7] Johnson et al. [2001] examine both long- and short-horizon forecasts. They find that after the passage of PSLRA, firms increase both short- and long-horizon good-news forecasts, but only short-horizon bad-news forecasts.

[8] Studies using directors' and officers' (D&O) liability insurance coverage as an ex ante measure of litigation risk also produce mixed findings (e.g., Wynn [2008], Cao and Narayanamoorthy [2011]).

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provide a possible explanation for the seemingly conflicting findings on the average effect of litigation on disclosure.

3. *Setting and Hypothesis*

3.1 SHAREHOLDER DERIVATIVE LAWSUITS AND UD LAWS

Shareholder derivative lawsuits are lawsuits brought by one or multiple shareholders on behalf of a corporation against a third party, usually officers or directors of the corporation, for transgressing fiduciary duties by engaging in conduct that harms the corporate entity. In a derivative lawsuit, the managers are the defendants and the corporation is the actual plaintiff. By forcing alleged managers to compensate the damage they cause to the corporation, a derivative lawsuit essentially represents the welfare of all shareholders. In contrast, security class action suits are filed against a company by investors who traded the company's securities within a specific period (known as a "class period") and suffered losses because of violations of the securities laws by the company. In class action suits, shareholders are the plaintiffs, whereas a company and possibly its managers are the defendants (Brochet and Srinivasan [2014]). Class action lawsuits often involve only a subset of shareholders and any financial recovery is paid to these shareholders because they are the primary victims (Ferris et al. [2007], Erickson [2010]).

Although D&O liability insurance to some extent shields managers from monetary losses, it does not completely eliminate their financial liabilities in derivative suits (Cox [1999]).[9] More importantly, derivative suits impose significant reputation costs on managers (Srinivasan [2005], Brochet and Srinivasan [2014]). Public accusations of an intentional breach of fiduciary duty can seriously harm managers' reputation and jeopardize their careers. Because derivative suits directly target managers' inability to fulfill fiduciary duties to the corporation, the adverse impacts on their reputation are arguably larger than those of class action suits.

Although shareholder class action lawsuits have received much attention in the legal and accounting literature, studies examining derivative lawsuits are relatively rare (Bourveau, Lou, and Wang [2018]). Derivative suits are arguably at least equally important as class action suits for investigating the effect of litigation on managerial disclosure for two reasons. First, as the firm and possibly the manager are both named as defendants in a class action suit, it is difficult to attribute all documented effects solely to managers' concerns about their own litigation risk. Second, shareholders file

[9] First, the scope of D&O insurance does not apply to wrongdoings that involve managers' intentional misconduct, dishonesty, or breaches in which they have reaped a personal gain. Second, insurers generally require a lengthy application inquiring into events and activities that may give rise to a claim against the policy. Third, insurers can deny coverage on the ground that the insured concealed important information when applying for insurance.

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more derivative lawsuits than class action lawsuits in the federal court. Erickson [2010] finds that, on average, more than 220 derivative lawsuits are filed each year in the federal court, whereas shareholders file fewer than 200 class action lawsuits each year.[10]

To file a derivative suit, shareholders need to first demand that the corporation bring legal action against wrongdoers, which is called "demand requirement" (Kinney [1994]). If the board declines the demand or does not respond within a reasonable time period, the shareholder may start filing the derivative suit with an explanation that demand was made and refused or it went unanswered. Historically, in most states, a shareholder could file a derivative suit without making demand if she believed that it would have been futile to do so (called "futility exception"), for instance, if she believed that the board was so involved in the wrongdoing that it could not make an unbiased decision or appoint an impartial committee (Kinney [1994]).

Since the 1980s, various states have embraced the UD requirement to eliminate the futility exception—instead of excusing demand because of futility, demand should be required in all derivative actions (see table 1). In other words, in jurisdictions with the UD requirement, even if a shareholder doubts that the directors are disinterested or independent or thinks that the wrongdoing is so egregious that the board cannot use its business judgment about whether to sue, demand would still have to be made (Kinney [1994]). As the board rarely grants approval because lawsuits typically name the directors themselves as defendants, UD laws significantly increase the difficulty for shareholders to sue executives. Indeed, Appel [2019] shows that the adoption of UD laws leads to a decline of derivative lawsuits by over one third.

The staggered adoption of UD laws across different states provides a powerful setting for identifying the causal effect of litigation risk on disclosures. First, corporate disclosures are an important trigger of derivative lawsuits. Erickson [2010] finds that more than 90% of derivative lawsuits for public firms are related to disclosure issues. Thus, the reduction in expected litigation risk arising from the adoption of UD laws by a company's incorporation state likely alters the manager's disclosure incentives. Second, as adopting UD laws is a decision made by the court of a firm's incorporation state, it is likely exogenous to the firm's voluntary disclosure decisions. Third, the staggered adoption of UD laws across states allows us to implement a DID design and mitigate the concern of confounding factors—a major threat for relying on a single event or events clustered within a short period.

[10] Most of the derivative lawsuits are filed to federal courts and about half of them are filed on behalf of companies incorporated outside of Delaware. This is probably one reason why legal scholars have largely ignored derivative lawsuits as they have focused on derivative lawsuits filed in the state courts, particularly the Delaware Court of Chancery (Thompson and Thomas [2004], Davis [2008]).

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TABLE 1
Universal Demand Laws Adoption by States

Adoption Year	State	Citation
1989	Georgia	Georgia Code Ann. § 14-2-742
1989	Michigan	Michigan Comp. Laws Ann. § 450.1493a
1990	Florida	Florida Stat. Ann. § 607.07401
1991	Wisconsin	Wisconsin Stat. Ann. § 180.742
1992	Montana	Montana Code. Ann. § 35-1-543
1992	Virginia	Virginia Code Ann § 13.1-672.1B
1992	Utah	Utah Code. Ann. § 16-10a-740(3)
1993	New Hampshire	New Hampshire Rev. Stat. Ann. § 293-A:7.42
1993	Mississippi	Mississippi Code Ann. § 79-4-7.42
1995	North Carolina	North Carolina Gen. Stat. § 55-7-42
1996	Arizona	Arizona Rev. Stat. Ann. § 10–742
1996	Nebraska	Nebraska Rev. Stat. § 21–2072
1997	Connecticut	Connecticut Gen. Stat. Ann. § 33–722
1997	Maine	Maine Rev. Stat. Ann. 13-C, § 753
1997	Pennsylvania	Cuker v. Mikalauskas (547 Pennsylvania. 600, 692 A.2d 1042)
1997	Texas	Texas Bus. Org. Code. Ann. 607.07401
1997	Wyoming	Wyoming Stat. § 17-16-742
1998	Idaho	Idaho Code § 30-1-742
2001	Hawaii	Hawaii Rev. Stat. § 414-173
2003	Iowa	Iowa Code Ann. § 490.742
2004	Massachusetts	Massachusetts Gen. Laws. Ann. Ch. 156D, § 7.42
2005	Rhode Island	Rhode Island Gen. Laws. § 7-1.2-710(C)
2005	South Dakota	South Dakota Codified Laws 47-1A-742

This table reports the states that have adopted the universal demand laws and the corresponding effective years and statute references. It is reproduced from table 1 of Appel [2019].

3.2 HYPOTHESIS

Managers trade off costs and benefits when deciding on whether to issue earnings warnings. The fact that most firms facing large earnings shortfalls do not warn suggests that issuing warnings is costly to managers and/or firms.[11] Extant literature suggests several costs of issuing warnings. One cost is excess negative market reaction (Kasznik and Lev [1995], Libby and Tan [1999], Tucker [2007]). For example, Tucker [2007] finds that after controlling for self-selection, the return difference between warning and nonwarning firms in the short-term window is significantly negative at −6.4%, though it disappears in the long-term window, and Libby and Tan [1999] show in an experimental setting that analyst forecasts of future earnings are significantly lower for warning firms than for nonwarning firms.[12] The short-run price decreases can impose significant costs on managers, such as

[11] For example, Tucker [2007] reports that 85.7% of firms with large negative earnings surprises do not issue earnings warnings. In our sample, 87.8% firms with large negative earnings surprises do not issue earnings warnings.

[12] Tucker's [2007] further analysis suggests that the warning penalty is potentially because of investor overreaction to warnings. Libby and Tan [1999] conclude that the cognitive process

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reduced managerial stock compensation, a higher risk of hostile takeovers, and greater career concerns (e.g., Palepu [1986], Weisbach [1988], Desai, Chris, and Wilkins [2006], Kraft, Vashishtha, and Venkatachalam [2018]), and also on firms such as higher costs of capital and lower credit ratings.[13] Further, the costs of issuing warnings also arise from managerial benefits of delaying bad news, because not issuing warnings allows managers more time to take corrective action or wait for offsetting good news (Graham et al. [2005], Kothari, Shu, and Wysocki [2009], Roychowdhury and Sletten [2012]), and gives them more opportunities to sell their shares before the revelation of bad news to the market (e.g., Billings [2008], Roychowdhury and Sletten [2012], Ertimur, Sletten and Sunder [2014], Billings and Cadergren [2015]). Additionally, not issuing warnings helps avoid setting a disclosure precedent that is hard to maintain in the future, which, according to Graham et al. [2005], is the most common reason that managers limit voluntary disclosure. Finally, issuing warnings also involves some direct costs of preparing and releasing the disclosures.

Skinner [1994] argues that the main benefits of issuing earnings warnings are to reduce the litigation and reputation costs related to withholding bad news. More specifically, Skinner [1994, p. 38] argues that "to prevent large stock price declines on earnings announcement dates (and thereby reduce the potential costs of shareholder suits), managers have incentives to preempt the announcement of large negative earnings surprises." Skinner [1994] proposes two ways that warnings can reduce expected legal costs. First, warnings make it more difficult for the plaintiff, who does not know for sure when the manager first received the bad news, to argue that the manager withheld information. Second, warnings limit the period of nondisclosure, thereby reducing the damages that plaintiffs can claim. Skinner [1994, p. 38] further argues that managers may also "have reputational incentives to preempt negative earnings news" because security analysts and other investors may impose costs on firms when their managers appear to delay bad news disclosures. It is difficult for managers to argue credibly that they too were surprised by the earnings outcome, because there is a time lag between the end of the fiscal quarter and the

amplifies the negative effect of the earnings surprise for warning firms, causing analysts to believe the earnings surprise of warning firms is more permanent.

[13] In Graham et al.'s [2005] survey, managers explicitly acknowledge that "given the reality of severe market (over-) reactions to earnings misses," they are willing to sacrifice "long-term value to avoid short-term turmoil" (p. 5). To summarize why managers care about short-run stock prices, they state "Our analysis suggests that managers worry about short-run stock prices because (i) they believe that short-run stock price volatility affects a firm's cost of capital; (ii) CFOs, and by extension CEOs, are concerned about losing their jobs if the stock price falls; (iii) managers think that the labor market assesses their skill level based on short-run stock prices; (iv) managers seek to attract equity analysts to cover their stock; and (v) they seek to avoid embarrassing inquisitions by stock analysts in conference calls, if stock price falls. Although we do not find strong support for the bonus hypothesis, exercisable stock options held by managers suggest another reason why managers care about short-run stock prices" (p. 67).

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earnings announcement date. Thus, managers could acquire a reputation for failing to disclose bad news, leading to some adverse outcomes such as being less likely to be followed by analysts and money managers.[14] As we discuss in section 3.1, the adoption of UD laws significantly increases the difficulty for shareholders to proceed with derivative suits and thereby reduces managers' risk of being sued by shareholders. Thus, if managerial litigation risk incentivizes managers to issue more earnings warnings in the face of large earnings shortfalls, as Skinner [1994] suggests, this incentive would become weaker after firms' incorporation states adopt UD laws, and we would expect managers to issue fewer warnings after such adoption. Hence, we propose the following hypothesis:

H1: The adoption of UD laws reduces managers' issuance of earnings warnings in the face of large earnings shortfalls, ceteris paribus.

This prediction, however, is not a forgone conclusion because the literature does not have a consensus regarding whether warnings actually reduce lawsuits. For instance, Francis, Philbrick, and Schipper [1994] find that predisclosure does not appear to be a deterrent to litigation. Johnson, Kasznik, and Nelson [2001] also conclude that their evidence "does not support the argument that managers' primary motivation for the preemptive disclosure of bad news is to protect against shareholder litigation" (p. 299).

4. *Research Design*

Our main test examines the likelihood of managers issuing earnings warnings in the face of large earnings shortfalls. As a comparison, we also examine the likelihood of managers issuing alerts of upcoming large positive earnings surprises. We follow Kasznik and Lev [1995] and measure earnings surprises as actual quarterly earnings per share minus the consensus analyst forecast during the 30 days after the previous earnings announcement, scaled by the stock price at the beginning of the quarter (*EARN_SURPRISE*). A firm-quarter is classified as a bad-news (good-news) quarter if *EARN_SURPRISE* is below −1% (greater than 1%). For each bad-news (good-news) quarter, we define an indicator variable *WARNING* (*GNEWS_ALERT*) that equals one if a firm issues a bad-news (good-news) quarterly earnings forecast in the period from 30 days after the prior quarter earnings announcement to the current quarter earnings announcement, and zero otherwise (e.g., Kasznik and Lev [1995]). A bad-news (good-news) forecast is defined as a forecast that is below (above) the most recent consensus analyst forecast prior to the forecast date.

We use a matched sample design to investigate how the adoption of UD laws affects managers' issuance of warnings and good-news alerts. Because

[14] In addition, warnings can also bring the benefit of reducing information asymmetry. However, prior research finds that forecasts of shorter horizon have a relatively smaller effect in reducing information asymmetry (Rogers [2008]).

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the application of UD laws is based on a firm's incorporation state, we match each bad-news (good-news) firm-quarter in an incorporation state that adopted UD laws during our sample period (i.e., treatment sample) to a bad-news (good-news) firm-quarter in the same industry and quarter from an incorporation state that did not have UD laws during our sample period (i.e., control sample) using a PSM method. Our results are robust to industry-size matching and a generalized DID design similar to Bourveau, Lou, and Wang [2018], which does not rely on matching (see section 5.3).

To calculate the propensity score, we estimate the following logit model:

$$\begin{aligned} \mathit{TREAT} = \alpha &+ \beta_1 \mathit{SIZE} + \beta_2 \mathit{BTM} + \beta_3 \mathit{INST\_OWN} + \beta_4 \mathit{ABRET} \\ &+ \beta_5 \mathit{ROA} + \beta_6 \mathit{LOSS} + \beta_7 \mathit{EARN\_SURPRISE} \\ &+ \beta_8 \mathit{EARN\_VOL} + \beta_9 \mathit{NUM\_SEG} + \beta_{10} \mathit{M\&A} \\ &+ \beta_{11} \mathit{LITRISK} + \varepsilon, \end{aligned} \tag{1}$$

where *TREAT* is an indicator variable for firm-quarters in the treatment sample. The matching variables in equation (1) are the three sets of control variables we use in the main regression (see equation (2) below), which, based on prior research (e.g., Baginski and Hassell [1997], Miller [2002], Ajinkya, Bhojraj, and Sengupta [2005], Lennox and Park [2006], Bourveau, Lou, and Wang [2018]), capture major economic factors that are likely associated with management's disclosure decisions. The first set controls for stock and financial performance, including abnormal stock returns (*ABRET*), return on assets (*ROA*), an indicator variable for loss firms (*LOSS*), and earnings surprise (*EARN_SURPRISE*). The second set, which controls for the complexity and volatility of firm operations and inherent risk, includes earnings volatility (*EARN_VOL*), the number of business and geographic segments (*NUM_SEG*), and an indicator variable for merger and acquisition activities (*M&A*).[15] The third set includes the logarithm of market capitalization (*SIZE*), book-to-market ratio (*BTM*), and institutional ownership (*INST_OWN*), which control for the demand for information, and an ex ante measure of litigation risk (*LITRISK*), which controls for ex ante class action litigation risk. The appendix provides detailed variable definitions. To ensure that the treatment and control firms are similar before the earnings shock, we measure the matching variables in the previous quarter or year.

After obtaining propensity scores from equation (1), we match each firm-quarter in the treatment sample to a firm in the same quarter and industry (Fama-French 48 industries) from the control group that has the closest propensity score (with replacement) using a caliber of 0.05.[16] After the matching, we create an indicator variable *POST*, which equals 1 for a

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[15] Results are similar if we control separately for the number of business segments and the number of geographic segments.

[16] Results are similar if we match without replacement (see section 5.3.3).

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firm-quarter in the treatment sample if the firm-quarter is in the period after the firm's incorporation state adopts UD laws and 0 if the firm-quarter is in the preadoption period. For a firm-quarter in the control sample, *POST* takes the same value as the firm-quarter in the treatment sample it matches.

We estimate the following model to examine the effect of UD laws adoption on managers' tendency to issue earnings warnings in bad-news quarters:

$$\begin{aligned} WARNING = \alpha &+ \beta_1 POST + \beta_2 POST \times TREAT \\ &+ \beta_3 TREAT + Controls + Industry - Year\ FE \\ &+ Headquarters\ State\ FE + \varepsilon. \end{aligned} \tag{2}$$

The coefficient β_2 captures the effect of UD laws adoption on managers' tendency to issue earnings warnings for test firms relative to matched control firms. We expect β_2 to be negative. *Controls* refers to control variables as described in equation (1) measured for the same quarter or year. We also control for industry-year and headquarters state fixed effects. Because the adoption is at the incorporation state level, we follow Bourveau, Lou, and Wang [2018] and cluster standard errors by the state of incorporation to account for any within-state dependence (Petersen [2009]).[17]

Similarly, we estimate the following model to examine the effect of UD laws adoption on managers' tendency to issue good-news alerts in the good-news quarters:

$$\begin{aligned} GNEWS\_ALERT = \alpha &+ \beta_1 POST + \beta_2 POST \times TREAT \\ &+ \beta_3 TREAT + Controls + Industry - Year\ FE \\ &+ Headquarters\ State\ FE + \varepsilon. \end{aligned} \tag{3}$$

The coefficient β_2 measures the effect of UD laws adoption on managers' tendency to issue good-news alerts for test firms relative to matched control firms. A negative (positive) β_2 indicates that the adoption leads to fewer (more) good-news alerts. *Controls* is the same set of control variables as used for equation (2).

5. *Empirical Analysis*

5.1 DATA AND SAMPLE

We obtain management earnings forecasts data from First Call's Corporate Investor Guidelines (CIG) database, financial data from Compustat, stock return information from CRSP, analyst forecasts from I/B/E/S, institutional ownership data from Thomson Reuters, and derivative lawsuit data

[17] The inferences are similar when we use bootstrap-based standard errors to address potential inflation of standard errors because of a small number of clusters (Cameron et al. [2008, 2011]).

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from Audit Analytics. Our sample period is from 1995 to 2010. We begin our sample period in 1995 because this is the first year First Call provides forecast data. We end the sample in 2010 for two reasons. First, because the last year a state adopted UD laws is 2005 (South Dakota), we end the sample in 2010 to allow five years of postadoption period for that state. Second, First Call stopped providing forecast data in 2010.[18]

We follow Houston, Lin, and Xie [2018] and use the historical incorporation state information provided by Bill McDonald, who uses textual analysis to compile relevant data based on firms' SEC filings since 1994.[19] For those firms that are missing in the historical states data set, we use the information of incorporation state provided by Compustat. As our sample period is from 1995 to 2010, most of the incorporation state information is sourced from McDonald's data set, which minimizes the measurement error.

We require nonmissing data to measure the test and control variables and drop a small number of observations from firms that change states of incorporation to ensure that our results are not contaminated by firms endogenously choosing to reincorporate into states that offer a higher level of protection from shareholder litigation (around 3% of the sample). We further exclude utilities (SIC 4000–4999) and financial (SIC 6000–6999) firms. All continuous variables are winsorized at the 1% and 99% levels to mitigate the influence of outliers.

Our initial bad-news (good-news) quarter sample contains 8,875 (6,442) firm-quarters, including 543 (351) from the 13 states that adopted UD laws during the sample period and 8,332 (6,091) from the states with no change in UD laws over the sample period.[20] The final matched bad-news (good-news) quarter sample consists of 938 (568) firm-quarters, including 469 (284) matched pairs of treatment and control quarters. The decrease in the number of treatment quarters (from 543 to 469 for the bad-news quarters and from 351 to 284 for the good-news quarters) is because of some treatment quarters having no PSM peers in the same industry and quarter.

Table 2 reports the summary statistics for our samples. Panels A and B report results for the bad-news and good-news quarter samples, respectively. In panel A, all firm characteristics are similar across the treatment and control firms. The average negative earnings surprise (*EARN_SURPRISE*) is −3.3% and −3.0%, respectively, in the treatment and control groups. In panel B, the average positive earnings surprise is 2.3% in the treatment

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[18] Management forecast data in the post-2010 period can be obtained from I/B/S/E Guidance. Focusing on the forecast data provided by a single provider helps mitigate the concern that our results are driven by the inconsistency in data collection procedures of two data providers.

[19] Specifically, we obtain the historical states of incorporation and location from Bill McDonald's website: https://sraf.nd.edu/data/augmented-10-x-header-data/.

[20] The treatment states are Arizona, Nebraska, Connecticut, Maine, Pennsylvania, Texas, Wyoming, Idaho, Hawaii, Iowa, Massachusetts, Rhode Island, and South Dakota (see table 1). Because our sample period starts in 1995, we exclude firms incorporated in North Carolina, which adopted UD laws in 1995, from our treatment group.

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TABLE 2
Summary Statistics

Panel A: The bad-news quarter sample

	Treatment		Control		Difference	
	($N = 469$)		($N = 469$)			
	Mean	Std	Mean	Std	Diff.	*t*-stat
Dependent Variable						
WARNING	0.151	0.359	0.124	0.330	−0.028	−1.23
Independent Variables						
POST	0.719	0.450	0.719	0.450	0.000	0.00
SIZE	5.090	1.285	5.139	1.323	0.048	0.57
BTM	0.838	0.611	0.826	0.658	−0.012	−0.30
INST_OWN	0.495	0.255	0.479	0.259	−0.017	−0.99
ABRET	−0.093	0.275	−0.096	0.293	−0.003	−0.18
ROA	−0.019	0.055	−0.017	0.051	0.001	0.40
LOSS	0.516	0.500	0.507	0.500	−0.009	−0.26
EARN_SURPRISE	−0.033	0.036	−0.030	0.027	0.002	1.11
EARN_VOL	0.039	0.039	0.042	0.044	0.002	0.90
NUM_SEG	4.433	3.110	4.527	2.865	0.094	0.48
M&A	0.028	0.164	0.032	0.176	0.004	0.38
LITRISK	0.399	0.315	0.426	0.327	0.027	1.30

Panel B: The good-news quarter sample

	Treatment		Control		Difference	
	($N = 284$)		($N = 284$)			
	Mean	Std	Mean	Std	Diff.	*t*-stat
Dependent Variable						
GNEWS_ALERT	0.046	0.209	0.046	0.209	0.000	0.00
Independent Variables						
POST	0.852	0.356	0.852	0.356	0.000	0.00
SIZE	5.298	1.411	5.377	1.493	0.079	0.64
BTM	0.813	0.694	0.906	0.730	0.093	1.55
INST_OWN	0.519	0.268	0.521	0.256	0.002	0.09
ABRET	0.006	0.303	0.022	0.330	0.016	0.58
ROA	−0.025	0.073	−0.023	0.067	0.002	0.28
LOSS	0.500	0.501	0.553	0.498	0.053	1.26
EARN_SURPRISE	0.023	0.018	0.021	0.018	−0.001	−0.78
EARN_VOL	0.044	0.037	0.045	0.038	0.001	0.33
NUM_SEG	4.771	3.631	4.873	3.290	0.102	0.35
M&A	0.049	0.217	0.021	0.144	−0.028*	−1.82
LITRISK	0.398	0.313	0.427	0.321	0.029	1.09

This table reports summary statistics for the propensity score matched samples. The sample period is from 1995 to 2010. Panel A reports summary statistics for the sample of firms with large negative earnings surprise. To construct the matched sample, we first exclude firm-quarters without large negative earnings surprise. Next, for each firm-quarter in the state that adopts UD laws during the sample period (treatment firm), we match one firm-quarter in the same quarter and industry from a state that does not have UD laws during the sample period based on propensity score matching. Panel B reports summary statistics for the sample of firms with large positive earnings surprise. We use a similar matching procedure as in panel A. All continuous variables are winsorized at the 1% level. Variable definitions are provided in the appendix. *denotes statistical significance at 10% level based on a two-sided test.

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group and is 2.1% in the control group. The firm characteristics across the treatment and control groups are also balanced, except that M&A activities (*M&A*) are significantly lower for the control group.

5.2 THE EFFECT OF ADOPTING UD LAWS ON EARNINGS WARNINGS VERSUS GOOD-NEWS ALERTS

Table 3 reports results of estimating the effect of UD laws adoption on the likelihood of earnings warnings for the bad-news quarter sample. Panel A reports univariate results. For treatment firms, the average likelihood of issuing earnings warnings is 22.0% in the preadoption period and 12.5% in the postadoption period, with the difference between these two (9.5%) being statistically significant. In contrast, for control firms, we do not observe a similar pattern: The likelihoods of earnings warnings are not statistically distinguishable from each other in the pre- and postadoption periods. The DID estimate is 12.0% (= 0.095 − (−0.025)) and is significant, suggesting that relative to control firms, treatment firms are 12.0 percentage points less likely to issue earnings warnings after the adoption of UD laws.

Panel B presents multivariate results of estimating equation (2). We estimate equation (2) using an ordinary least squares (OLS) model, instead of a probit or logit model, because with the fixed effects structure in equation (2), we run into some computational issues when using a probit or logit model.[21] The estimated coefficient of $TREAT \times POST$ is negative and significant (−0.131, *t*-statistic = −3.14), indicating that after a state adopts UD laws, firms incorporated in that state are 13.1 percentage points less likely, relative to control firms, to issue earnings warnings. Given that the average likelihood of issuing earnings warnings for the treatment firms in the preadoption period is 22.0% (table 3, panel A), this effect is economically large. This finding suggests that a decrease in litigation risk reduces managers' incentive to issue warnings to preempt large negative earnings surprises. Turning to control variables, we find that firms that are larger (*SIZE*) and do not experience loss (*LOSS*) are more likely to issue warnings.

Next, we examine the dynamics of the effect in different years by replacing *POST* and $TREAT \times POST$ with several year indicators and their interactions with *TREAT* (e.g., Autor [2003]). Specifically, we estimate the following regression:

$$\begin{aligned} WARNING = \alpha &+ \beta_1 YEAR(-3) + \beta_2 YEAR(-2) + \beta_3 YEAR(-1) \\ &+ \beta_4 YEAR(0) + \beta_5 YEAR(1) + \beta_6 YEAR(2) \\ &+ \beta_7 YEAR(3) + \beta_8 YEAR(4+) + \beta_9 YEAR(-3) \\ &\times TREAT + \beta_{10} YEAR(-2) \times TREAT \\ &+ \beta_{11} YEAR(-1) \times TREAT + \beta_{12} YEAR(0) \end{aligned}$$

[21] The issues include functional nonconcavity in the maximum likelihood estimation and a substantial portion of the sample being dropped automatically because of perfect prediction.

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TABLE 3

The Effect of UD Laws on the Issuance of Earnings Warnings for Bad-News Quarters

Panel A: Univariate results

	Preadoption ($N = 132$)	Postadoption ($N = 337$)	Difference	Diff. in Diff.
Treatment firms	0.220	0.125	0.095*	0.120**
Control firms	0.106	0.131	−0.025	

Panel B: Multivariate results

Dependent variable:	*WARNING*	
	Coefficient	*t*-statistic
TREAT × *POST*	−0.131***	−3.14
TREAT	0.075*	1.72
POST	0.154***	4.30
SIZE	0.039***	3.21
BTM	−0.016	−1.28
INST OWN	0.028	0.41
ABRET	−0.091	−1.69
ROA	0.122	0.45
LOSS	−0.053**	−2.61
EARN_SURPRISE	0.090	0.31
EARN_VOL	−0.092	−0.31
NUM_SEG	−0.006	−0.88
M&A	−0.029	−0.60
LITRISK	0.061	1.42
Industry-year fixed effects	Yes	
Headquarters state fixed effects	Yes	
Observations (firm-quarters)	938	
Adj. *R*-squared	0.350	

Panel C: Effects by years

Dependent variable:	*WARNING*			
	Coefficient	*t*-statistic	Coefficient	*t*-statistic
TREAT × *YEAR(−3)*	0.129	1.29		
TREAT × *YEAR(−2)*	−0.011	−0.17		
TREAT × *YEAR(−1)*	−0.144	−1.59		
TREAT × *YEAR(0)*	−0.273**	−2.48	−0.255**	−2.15
TREAT × *YEAR(1)*	−0.121	−0.78	−0.102	−0.63
TREAT × *YEAR(2)*	−0.177***	−3.49	−0.166**	−2.37
TREAT × *YEAR(3)*	−0.131	−1.21	−0.120	−1.08
TREAT × *YEAR(4+)*	−0.126***	−3.67	−0.114***	−4.01
TREAT	0.090**	2.48	0.069*	1.77
YEAR(−3)	−0.042	−0.17		
YEAR(−2)	0.228*	1.85		
YEAR(−1)	0.089	0.81		
YEAR(0)	0.367***	3.63	0.351***	3.74
YEAR(1)	0.245	1.45	0.219	1.37
YEAR(2)	0.087	1.33	0.069	0.94
YEAR(3)	0.101	0.68	0.068	0.57
YEAR(4+)	0.189*	1.85	0.128**	2.38

(Continued)

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TABLE 3—*Continued*

Panel C: Effects by years

Dependent variable:	*WARNING*			
	Coefficient	*t*-statistic	Coefficient	*t*-statistic
Control variables	Yes		Yes	
Industry-year fixed effects	Yes		Yes	
Headquarters state fixed effects	Yes		Yes	
Observations (firm-quarters)	938		938	
Adj. *R*-squared	0.360		0.358	

This table reports the results for the effect of adopting UD laws on the likelihood of issuing earnings warnings (*WARNING*) for bad-news quarters using the propensity score matched sample. The sample period is from 1995 to 2010. Panels A and B report univariate and OLS multivariate results, respectively. Panel C reports the treatment effects by years. For the matching of firms with large negative earnings surprise, we first exclude firm-quarters without large negative earnings surprise. Next, for each firm-quarter in the state that adopts UD laws during the sample period (treatment firm), we match one firm-quarter in the same quarter and industry from a state that does not have UD laws during the sample period (control firm) using propensity score matching. All regressions include industry-year (Fama-French 48 industries) and headquarters state fixed effects. Standard errors are clustered at the incorporation state level. All continuous variables are winsorized at the 1% level. Variable definitions are provided in the appendix. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

$$
\begin{aligned}
&\times TREAT + \beta_{13} YEAR\,(1) \times TREAT \\
&+ \beta_{14} YEAR\,(2) \times TREAT + \beta_{15} YEAR\,(3) \times TREAT \\
&+ \beta_{16} YEAR\,(4+) \times TREAT + Controls + Industry \\
&- Year\ FE + Headquarters\ State\ FE + \varepsilon.
\end{aligned}
\tag{4}
$$

YEAR(−1) (*YEAR(−2), YEAR(−3)*) is an indicator variable for the first (second, third) year prior to the adoption year of UD laws. *YEAR(0)* is an indicator variable for the adoption year. *YEAR(1)* (*YEAR(2), YEAR(3)*) is an indicator variable for the first (second, third) year after the adoption year. *YEAR(4+)* is an indicator variable for the fourth year after the adoption year and all later years. The coefficients of *YEAR(−3)* × *TREAT*, *YEAR(−2)* × *TREAT*, and *YEAR(−1)* × *TREAT* provide evidence on the parallel trend assumption, while the coefficients of *YEAR(0)* × *TREAT*, *YEAR(1)* × *TREAT*, *YEAR(2)* × *TREAT*, *YEAR(3)* × *TREAT*, and *YEAR(4+)* × *TREAT* shed light on how persistent the treatment effect is.

Column 1 of table 3, panel C, reports the results of estimating equation (4). The coefficients of *YEAR(−3)* × *TREAT*, *YEAR(−2)* × *TREAT*, and *YEAR(−1)* × *TREAT* are all insignificant, suggesting that the difference in the likelihoods of warnings issuance between the treatment and control firms in year −3 (year −2, year −1) is not distinguishable from that for the default years—all years prior to year −3. These results support the parallel trend assumption. The coefficients of *YEAR(0)* × *TREAT*, *YEAR(1)* × *TREAT*, *YEAR(2)* × *TREAT*, *YEAR(3)* × *TREAT*, and *YEAR(4+)* × *TREAT* are all negative, and those of *YEAR(0)* × *TREAT*, *YEAR(2)* × *TREAT*, and *YEAR(4+)* × *TREAT* are significant. We find similar results when removing *YEAR(−3), YEAR(−2), YEAR(−1)*, and their interaction terms with *TREAT*

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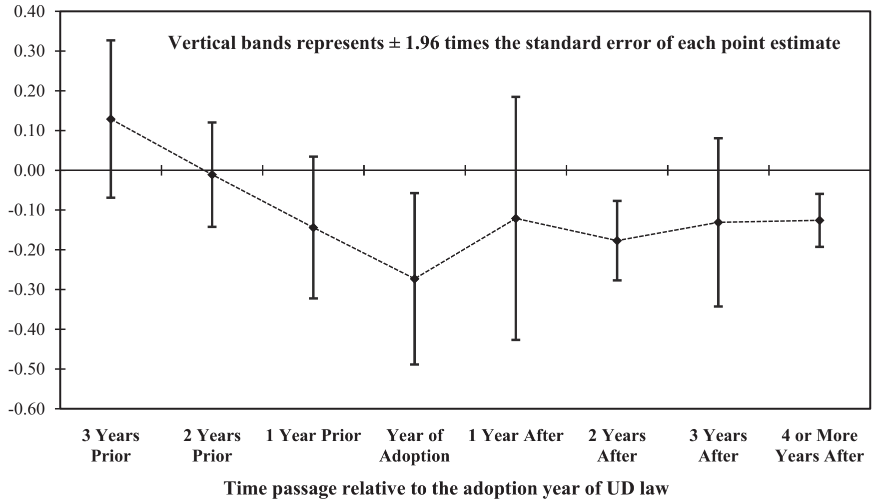

FIG. 1.—Difference-in-differences estimates for years before, during, and after the UD laws adoption. This figure plots the estimated coefficients in column 1 of table 3, panel B, and their 5% confidence intervals. The point estimate reflects how the difference in the likelihoods of warnings between the treatment and control firms in year −3 (−2, −1, 0, 1, 2, 3, and 4+) is different from that in the years prior to year −3, where year −3 is the third year prior to the UD laws adoption year and the other years are defined similarly.

(column 2 of panel C). These results suggest that the treatment effect documented in table 3, panel B is fairly persistent. Figure 1 provides a visual presentation of the coefficients in column 1 and their 5% confidence intervals (see figure 3 of Autor [2003]).

Next, we examine how our finding varies with ex ante derivative lawsuit risk, which we estimate following Bourveau, Lou, and Wang [2018].[22] Specifically, we estimate the following model:

$$\begin{aligned} Sued_t = {} & \alpha + \beta_1 Derivative\ lawsuit\ industry_t + \beta_2 Log(assets)_{t-1} \\ & + \beta_3 Sales\ growth_{t-1} + +\beta_4 Return_{t-1} + \beta_5 Return\ volatility_{t-1} \\ & + \beta_6 Return\ skewness_{t-1} + \beta_7 Turnover_{t-1} + \varepsilon. \qquad (5) \end{aligned}$$

Sued is an indicator variable for whether a firm is sued in a derivative lawsuit in year *t*. *Derivative lawsuit industry* is an indicator variable equal to one if there is a derivative lawsuit in the firm's industry and zero otherwise. *Log(assets)* is the natural logarithm of total assets. *Sales growth* is the sales growth rate. *Returns* is the market-adjusted 12-month stock return. *Return volatility* and *Return skewness* are the standard deviation and skewness of the monthly return during the year, respectively. *Turnover* is the 12-month trad-

[22] Bourveau et al.'s [2018] approach is modified from a model used by Kim and Skinner [2012].

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TABLE 4

The Effect of UD Laws on Earnings Warnings for Bad-News Quarters: Subsamples Based on Ex Ante Derivative Lawsuit Risk

Dependent variable:	*WARNING*			
	Ex Ante Derivative Lawsuit Risk			
	Low		High	
	1		2	
	Coefficient	*t*-statistic	Coefficient	*t*-statistic
TREAT × *POST*	−0.062	−0.79	−0.380***	−2.90
TREAT	0.082	1.23	0.150	1.15
POST	0.121	1.23	0.260*	1.87
p-value of diff. in *TREAT* × *POST*	0.001			
Control variables	Yes		Yes	
Industry-year fixed effects	Yes		Yes	
Headquarters state fixed effects	Yes		Yes	
Observations (firm-quarters)	312		311	
Adj. *R*-squared	0.692		0.590	

This table reports the OLS regression results for the effect of adopting UD laws on the likelihood of issuing earning warnings (*WARNING*) in bad-news quarters based on subsamples with high versus low ex ante derivative lawsuit risk (*DRisk*). The sample period is from 1995 to 2010. For the matching of firms with large negative earnings surprise, we first exclude firm-quarters without large negative earnings surprise. Next, for each firm-quarter in the state that adopts UD laws during the sample period (treatment firm), we match one firm-quarter in the same quarter and industry from a state that does not have UD laws during the sample period (control firm) based on propensity score matching. We partition the matched sample into high versus low (above vs. below the sample median) *DRisk* subsamples based on *DRisk* measured at the year prior to the UD laws adoption year. *DRisk* is estimated using Bourveau, Lou, and Wang's [2018] approach. Both regressions include industry-year (Fama-French 48 industries) and headquarters state fixed effects. Standard errors are clustered at the incorporation state level. All continuous variables are winsorized at the 1% level. Variable definitions are provided in the appendix. *** and * denote significance at the 1% and 10% levels based on two-sided tests, respectively.

ing volume scaled by the number of shares outstanding at the beginning of the year. We estimate equation (5) using the data from the sample period 2000–2010 and use the estimated coefficients to estimate the ex ante derivative lawsuit risk for the full sample period (1995–2010).[23]

We partition the bad-news quarter sample based on the median of the ex ante derivative lawsuit risk, measured in the year prior to UD laws adoption, into high and low groups, and estimate equation (2) separately for each group. Table 4 reports the results of this analysis. For the low-risk group (column 1), the estimated coefficient on *TREAT* × *POST* is insignificant (−0.062, *t*-statistic = −0.79). In contrast, the coefficient is negative and significant (−0.380, *t*-statistic = −2.90) for the high-risk group (column 2), and it is larger in magnitude than that in table 3, panel B. The difference in the coefficients between the two groups is also significant (*p*-value = 0.001).[24] These results suggest that the effect of UD laws adoption on man-

[23] We use the sample period 2000–2010 to fit the model because, based on the frequency of lawsuits recorded for each year, the derivative lawsuit data in Audit Analytics is likely incomplete in the years prior to 2000.

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TABLE 5

The Effect of UD Laws on the Issuance of Good-News Alerts for Good-News Quarters

Dependent variable:	*GNEWS_ALERT*					
	Full Sample		Ex Ante Derivative Lawsuit Risk			
			Low		High	
	1		2		3	
	Coefficient	*t*-stat	Coefficient	*t*-stat	Coefficient	*t*-stat
TREAT × *POST*	−0.007	−0.12	−0.029	−0.52	0.013	0.09
TREAT	−0.014	−0.32	0.031	0.57	−0.038	−0.27
POST	0.139*	1.79	0.225	0.97	−0.563	−1.51
SIZE	0.001	0.04	0.017	0.94	−0.012	−0.35
BTM	0.006	0.15	0.002	0.08	0.059	0.80
INST OWN	0.105*	1.80	0.088	0.89	0.009	0.07
ABRET	0.039	0.95	0.016	0.69	−0.037	−0.62
ROA	0.087	1.18	−0.048	−0.29	0.388	0.66
LOSS	−0.025	−1.40	−0.026	−0.67	−0.092	−1.30
EARN_SURPRISE	−0.040	−0.12	0.865	0.75	−1.028	−0.65
EARN_VOL	−0.048	−0.23	−0.390	−0.66	0.274	0.37
NUM_SEG	0.002	0.58	−0.006	−0.82	0.005	0.79
M&A	0.092	1.02	−0.032	−0.55	0.108	1.15
LITRISK	0.027	0.88	0.115	0.93	0.067	0.76
p-value of Diff. in *TREAT* × *POST*				0.671		
Control variables	Yes		Yes		Yes	
Industry-year fixed effects	Yes		Yes		Yes	
Headquarters state fixed effects	Yes		Yes		Yes	
Observations (firm-quarters)	568		207		206	
Adj. *R*-squared	0.310		0.731		0.494	

This table reports the OLS regression results for the effect of adopting UD laws on the likelihood of issuing good-news alerts (*GNEWS_ALERT*) for good-news quarters using the propensity score matched sample, and how the effect varies with ex ante derivative lawsuit risk (*DRisk*). The sample period is from 1995 to 2010. For the matching of firms with large positive earnings surprise, we first exclude firm-quarters without large positive earnings surprise. Next, for each firm-quarter in the state that adopts UD laws during the sample period (treatment firm), we match one firm-quarter in the same quarter and industry from a state that does not have UD laws during the sample period (control firm) using propensity score matching. Column 1 reports results based on the full matched sample. In columns 2 and 3, we partition the matched sample into high versus low (above vs. below the sample median) *DRisk* subsamples based on *DRisk* measured in the year prior to the UD laws adoption year. *DRisk* is estimated using Bourveau, Lou, and Wang's [2018] approach. All regressions include industry-year (Fama-French 48 industries) and headquarters state fixed effects. Standard errors are clustered at the incorporation state level. All continuous variables are winsorized at the 1% level. Variable definitions are in the appendix. * denotes statistical significance at 10% level based on a two-sided test.

agers' incentives to issue earnings warnings is stronger for firms with higher ex ante derivative lawsuit risk, providing additional evidence that the adoption affects managers' issuance of earnings warnings through changing the litigation reduction benefit of warnings.

Table 5 repeats the analyses in table 3, panel B, and table 4 for the good-news quarter sample using *GNEWS_ALERT* as the dependent variable. Col-

[24] In this table as well as table 5, we use a seemingly unrelated regressions (SUR) system to compare coefficient estimates across two subsamples.

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umn 1 reports results of estimating equation (3) for the full sample. We find that the coefficient on *TREAT* × *POST* is insignificant (−0.007, *t*-statistic = −0.12), providing no conclusive evidence that adopting UD laws changes managers' issuance of good-news alerts prior to large positive earnings surprises.[25] Columns 2 and 3 report results of sample partition based on ex ante derivative lawsuit risk. In both the high and low risk groups, we find an insignificant coefficient of *TREAT* × *POST*. Because lawsuits are usually triggered by large negative earnings surprises but not by large positive surprises, this result from testing good-news alerts increases our confidence that the decrease in earnings warnings we document is attributable to the adoption of UD laws reducing litigation risk, rather than to other confounding factors that may also have changed around the adoption of UD laws.

5.3 ROBUSTNESS TESTS

5.3.1. Sample Representativeness and Larger Samples of Bad-News Quarters. Our treatment sample contains 469 treatment firm-quarters. It is relatively small for two reasons. First, following prior research on litigation and disclosure (e.g., Kasznik and Lev [1995], Field, Lowry, and Shu [2005]), we focus on firm-quarters with large negative earnings surprises (i.e., consensus analyst forecast – actual earnings per share > 1% of stock price) to provide a more powerful test of our hypothesis. However, a drawback of this design is the smaller sample of firm-quarters with large negative earnings surprises. Second, our treatment states—states that adopted UD laws over 1995–2010—do not include Delaware or Nevada, the two most popular incorporation states.[26]

The small treatment sample raises the concern that our finding may not be generalizable. To explore this issue, we compare major firm characteristics of the treatment firm-quarters in the matched sample with the following samples over the same period: (i) all bad-news firm-quarters in the treatment states, (ii) all bad-news firm-quarters in the control states (states that do not have UD laws during the sample period), (iii) all bad-news firm-quarters in the states that do not change the UD laws adoption status during the sample period, and (iv) all bad-news firm-quarters in Compustat, namely, (i) + (iii). The comparisons are reported in table 6. We focus on comparing all the control variables in the warnings regression. We find that the treatment firm-quarters in the matched sample are representative of bad-news quarters in the treatment states—for all of the 11 variables, the difference in the means between the two groups (columns 1 and 2) is insignificant.

[25] For the effects of control variables, we find the likelihood of issuing good-news alerts prior to large positive earnings surprises increases with institutional ownership (*INST_OWN*).

[26] We note that this sample limitation applies to all studies using the UD laws setting (e.g., Bourveau et al. [2018], Boone et al. [2019]).

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TABLE 6

Analysis of Sample Representativeness

	Treatment Bad-News Firm-Quarters in the Matched Sample	All Bad-News Firm-Quarters in Treatment States	All Bad-News Firm-Quarters in Control States	All Bad-News Firm-Quarters in States Not Changing the Adoption Status	All Bad-News Firm-Quarters in Compustat	Difference			
	(1)	(2)	(3)	(4)	(5)	(1) − (2)	(1) − (3)	(1) − (4)	(1) − (5)
SIZE	5.00	4.95	5.04	5.02	5.02	0.05	−0.04	−0.03	−0.02
BTM	0.87	0.90	0.82	0.84	0.84	−0.03	0.05	0.04	0.03
INSTITUTION OWN	0.48	0.48	0.46	0.46	0.46	0.01	0.03**	0.03**	0.03**
ABRET	−0.07	−0.06	−0.07	−0.07	−0.07	0.00	0.00	0.00	0.00
ROA	−0.04	−0.04	−0.07	−0.07	−0.06	0.00	0.03***	0.03***	0.03***
LOSS	0.76	0.75	0.79	0.79	0.78	0.01	−0.03	−0.02	−0.02
EARN_SURPRISE	−0.03	−0.04	−0.06	−0.06	−0.06	0.01	0.03***	0.03***	0.03***
EARN_VOL	0.04	0.04	0.07	0.06	0.06	0.00	−0.03***	−0.02***	−0.02***
NUM_SEG	4.49	4.53	4.49	4.49	4.49	−0.05	0.00	0.00	0.00
M&A	0.04	0.04	0.03	0.03	0.03	0.00	0.01	0.01	0.01
LITRISK	0.43	0.42	0.45	0.44	0.44	0.01	−0.02	−0.01	−0.01
N	469	561	9,049	9,742	10,303				

The table compares the means of major firm characteristics for the following five samples: (i) treatment bad-news firm-quarters in the matched sample (column 1), (ii) all bad-news firm-quarters in the treatment states (column 2), (iii) all bad-news firm-quarters in the control states, namely, states that do not have the UD laws in the sample period (column 3), (iv) all bad-news firm-quarters in the states that do not change the UD laws adoption status during the sample period (column 4), and (v) all bad-news firm-quarters in Compustat (column 5). The reported numbers in the first five columns are the means of firm characteristics. The last four columns compare sample (i) with the other four samples. Variable definitions are provided in the appendix. *** and ** denote statistical significance at the 1% and 5% levels based on two-sided tests, respectively.

When comparing the treatment firm-quarters to (ii) all bad-news firm-quarters in the control states (columns 1 and 3 of table 6), we find that 4 out of the 11 variables (*INSITTUTION OWN*, *ROA*, *EARN_SURPRISE*, and *EARN_VOL*) have significantly different means and the differences for the other 7 variables are insignificant. Specifically, relative to bad-news firms in the control states, our sample firms have similar size, growth opportunities, stock performance, loss rate, number of segments, likelihood of mergers and acquisitions, and ex ante class action litigation risk, but higher institutional ownership, higher profitability, more positive earnings surprises, and less volatile earnings. Results are similar from the comparisons with (iii) all bad-news firm-quarters in the states that do not change the UD laws adoption status during the sample period (columns 1 and 4); and (iv) all bad-news firm-quarters in Compustat (columns 1 and 5). Overall, these results indicate that the treatment firm-quarters are similar to all bad-news firm-quarters in the control states and in the Compustat universe in many key dimensions, but not completely comparable.

To address the generalizability issue, we examine whether our finding also holds in two larger treatment samples of bad-news quarters. The first broader sample contains firm-quarters in the treatment states with negative earnings surprises (i.e., consensus analyst forecast > actual earnings) and has 2,484 observations. The second sample contains firm-quarters in the treatment states identified as bad-news quarters based on stock returns (Roychowdhury and Sletten [2012]). More specifically, it contains 6,078 firm-quarters in the treatment states with market-adjusted returns beginning two days after the prior earnings announcement and ending one day after the current earnings announcement lower than -1%.[27] As reported in table 7, we find that our results hold in both broader treatment samples.[28] These results indicate that our finding is generalizable to much larger samples of bad-news firm-quarters.

5.3.2. Alternative Research Design. We also examine the robustness of our results to a generalized DID design similar to Bourveau, Lou, and Wang, 2018], which does not rely on matching. Specifically, we estimate the following OLS model for the full bad-news sample:

$$\begin{aligned} WARNING = \alpha + \beta UD\ LAW + Controls + Year\ FE + Industry\ FE \\ + Headquarters\ State\ FE \\ + Incorporation\ State\ FE + \varepsilon, \end{aligned} \tag{6}$$

where *UD LAW* is an indicator variable that equals to one for all firm-quarters incorporated in a state that has UD laws in that quarter and zero

[27] The results are robust to using alternative cutoffs (e.g., -2% and -5%) and using a different return measurement window—from 30 days after the prior earnings announcement to one day after the current earnings announcement.

[28] When the bad-news quarter is identified with stock return (column 2, table 6), we replace the control variable *EARN_SURPRISE* with the return. The results are similar when *EARN_SURPRISE* is used, whereas the sample size becomes smaller (6,410 observations).

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TABLE 7
Results Based on Larger Samples

Dependent variable:	*WARNING*			
	Negative Earnings Surprise Sample		Bad-News Sample Based on Stock Return	
	1		2	
	Coefficient	*t*-statistic	Coefficient	*t*-statistic
TREAT × *POST*	−0.022***	−2.07	−0.016***	−3.16
TREAT	−0.004	−0.31	−0.002	−0.34
POST	0.007	0.48	−0.009*	−1.83
Control variables	Yes		Yes	
Industry-year fixed effects	Yes		Yes	
Headquarters state fixed effects	Yes		Yes	
Observations (firm-quarters)	4,968		12,156	
Adj. *R*-squared	0.198		0.114	

This table reports the OLS regression results for the effect of adopting UD laws on the likelihood of issuing earning warnings (*WARNING*) in bad-news quarters using two alternative samples that are much larger than the main sample. In column 1, bad-news quarters are identified as firm-quarters for which the actual earnings are lower than the consensus analyst forecasts during the 30 days after the prior-quarter earnings announcement (i.e., firm-quarters with negative earnings surprise). In column 2, a bad-news quarter is defined as a firm-quarter with the market-adjusted return from two days after the earnings announcement date of the previous quarter to one day after the earnings announcement date of the current quarter being lower than −1%. For the regression in column 2, the control variable *EARN_SURPRISE* is replaced with the return used to measure the earnings news. Both regressions include industry-year (Fama-French 48 industries) and headquarters state fixed effects. Standard errors are clustered at the incorporation state level. All continuous variables are winsorized at the 1% level. Variable definitions provided are in the appendix. *** and * denote significance at the 1% and 10% levels based on two-sided tests, respectively.

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otherwise. Specifically, for firms incorporated in the treatment states, *UD LAW* takes the value one for all quarters after UD laws adoption and zero otherwise. For firms incorporated in the other states (states that do not change the adoption status during the sample period), it equals to one if the state always has UD laws and zero if the state never has UD laws. *Controls* refers to control variables in equation (2). Because the application of UD laws is based on a firm's incorporation state, we include year and incorporation state fixed effects in equation (6) so that the estimated coefficient on *UD LAW* has a DID interpretation at the state level (Bertrand and Mullainathan [2003], Bourveau, Lou, and Wang [2018]).[29]

Column 1 of table 8 reports the results of this analysis. Because this test is based on the full bad-news sample, the sample size is much larger than that of the matched sample (10,303 vs. 938). We find a negative and significant coefficient on *UD LAW* (−0.078, *t*-statistic = −3.92), indicating that firms are less likely to issue warnings after the adoption of UD laws. The magnitude of the treatment effect is comparable to that estimated based on the matched sample in panel B of table 3 (−0.078 vs. −0.131). Thus, our main finding in table 3, panel B, is robust to this alternative approach of implementing a DID estimation.[30]

5.3.3. Alternative Matching Methods. Our main analysis is based on a PSM approach with replacement, which helps identify a control firm-quarter with an as close as possible propensity score for each treatment firm-quarter. However, the downside of matching with replacement is that the same firm-quarter could be the control firm-quarter for multiple treatment firm-quarters.[31] To address this issue, we report a robustness test based on PSM without replacement in column 2 of table 8. The magnitude of the treatment effect is very similar to that in panel B of table 3 (−0.125 vs. −0.131).

Although PSM allows for matching on multiple variables, it has been argued that the results may be sensitive to the matching procedure (e.g., DeFond, Erkens, and Zhang [2017]). Thus, we examine the robustness of our results to an alternative matching approach: matching on industry membership (Fama-French 48 industries) and market capitalization. Specifically,

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[29] Bourveau et al. [2018] use a similar model but with incorporation state fixed effects replaced with firm fixed effects. We cannot implement firm fixed effects because after restricting the sample to firm-quarters with large negative earnings surprises, there is little within-firm variation for firms incorporated in the treatment states. In section 6, when we reconcile our finding with Bourveau et al.'s [2018], we use exactly the same design as theirs.

[30] We do not use this approach in our main analysis because the number of observations for treatment firms is relatively low. There are only 158 (1.53%) pre-adoption and 403 (3.91%) postadoption observations in the treatment states. By using a matching approach, we are able to obtain a more balanced sample. The matching approach also makes the treatment and control firms more comparable.

[31] In our main PSM sample, 26 control firm-quarters are matched to two treatment firm-quarters, and one control firm-quarter is matched to three treatment firm-quarters.

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TABLE 8
Robustness Tests

	Generalized DID Design 1	PSM without Replacement 2	Industry -Size Matching 3	HQ State-Year Fixed Effects 4	Alternative Sample Period (1998~2010) 5	Alternative Warning Measurement Window 6	Timeliness Measure 7
UD LAW	−0.078***						
	(−3.92)						
TREAT × *POST*		−0.125***	−0.145***	−0.067*	−0.226***	−0.096**	−0.106*
		(−3.39)	(−3.07)	(−1.98)	(−2.94)	(−2.15)	(−1.88)
TREAT		0.071*	0.150***	0.016	0.167**	0.047	0.071
		(1.76)	(4.20)	(0.66)	(2.23)	(0.93)	(1.18)
POST		0.130***	0.119***	0.169***	0.156**	0.132***	0.080
		(2.54)	(3.23)	(2.85)	(2.33)	(3.35)	(1.08)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HQ state FE	Yes	Yes	Yes	No	Yes	Yes	Yes
HQ state-year FE	No	No	No	Yes	No	No	No
Incorporation state FE	Yes	No	No	No	No	No	No
Observations (firm-quarters)	10,303	926	942	938	765	938	938
Adj. *R*-squared	0.207	0.335	0.372	0.581	0.398	0.302	0.342

This table reports robustness tests for the effect of adopting UD laws on the likelihood of issuing earnings warnings (*WARNING*). The sample period is from 1995 to 2010 except for column 6, which reports the results based on an alternative sample period 1998~2010. Column 1 reports the analysis using a generalized DID design and the full sample of firms with large negative earnings surprise. *UD LAW* is an indicator variable that equals to one for firm-quarters in states that have passed UD laws. It equals to zero for all firms incorporated in states that have not adopted UD laws and firm-quarters before the adoption for firms incorporated in states that have adopted UD laws, and one otherwise. Column 2 reports the results of estimating equation (2) using a sample based on PSM without replacement. Column 3 reports the results of estimating equation (2) using the industry and size matched sample. Column 4 reports the results of estimating equation (2) by replacing headquarters state fixed effects with headquarters state-year fixed effects. Column 5 reports the results of estimating equation (2) using the sample period 1998~2010. Column 6 reports the results of estimating equation (2) using an alternative window to measure warnings: the period between the fiscal quarter end and the earnings announcement date (preannouncement period). Column 7 reports the results of estimating equation (2) using the analyst-based measure (*TIMELINESS*) from Donelson et al. [2012] as the dependent variable. Standard errors are clustered at the incorporation state level. All continuous variables are winsorized at the 1% level. Variable definitions are provided in the appendix. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

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for each bad-news quarter in the treatment sample, we find a matched bad-news quarter in the same quarter and industry with closest market capitalization from a state that does not have UD laws during our sample period. We repeat the main analysis using the sample based on this alternative matching method and report the results in column 3 of table 8. Consistent with the results in table 3, panel B, we continue to find that the likelihood of issuing earnings warnings decreases after the adoption of UD laws.

5.3.4. Addressing the Influence of Headquarters States. By construction, our treatment and control firms are incorporated in different states. One potential concern is that if these firms are also headquartered in different states, our results may be driven by the difference in the changes of economic environments between treatment and control firms. To mitigate this concern, we replace the headquarters state fixed effects in equation (2) with headquarters state by year fixed effects. The results reported in column 4 of table 8 indicate that our inference is the same.

5.3.5. Alternative Sample Period and Measurement Window. Chuk, Matsumoto, and Miller [2013] show that the coverage of management earnings forecasts by First Call was incomplete in the early years. To ensure that our results are not affected, we repeat the main test by excluding years 1995–1997 from the sample period. Using this shorter sample period reduces the number of treatment states from 13 to 6 (see table 1). However, the results reported in column 5 of table 6 indicate that our finding is robust to this shorter sample period. The estimated coefficient of *TREAT* × *POST* continues to be significantly negative and the magnitude is comparable to that in table 3, panel B (−0.226 vs. −0.131).

In our main analysis, earnings warnings are measured over the period from 30 days after the prior quarter earnings announcement to the current quarter earnings announcement (e.g., Kasznik and Lev [1995]). To examine the sensitivity of our results to the measurement window, we examine two alternative windows: (i) the period from the fiscal quarter end to the earnings announcement date (preannouncement period) and (ii) the period from 30 days before the fiscal quarter end to the earnings announcement date. The results based on these two alternative measurement windows are fairly consistent with those in table 3, panel B. To conserve table space, we report only results for the first alternative measurement window in column 6 of table 8. The estimated coefficient of *TREAT* × *POST* continues to be significantly negative and the magnitude is comparable to that in table 3, panel B (−0.096 vs. −0.131).

5.3.6. Analyst Forecast-Based Disclosure Measure. Donelson et al. [2012] argue that in addition to earnings warnings, earnings news can be revealed to the market through other channels, such as analyst conference calls, presentations, and webcasts. They construct a new measure of the timeliness of all earnings disclosures based on the evolution of analysts' consensus earnings forecasts. We follow their approach to provide additional evidence on

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our hypothesis. Specifically, we calculate Donelson et al.'s [2012] timeliness measure (*TIMELINESS*) as the average proportion of total earnings news revealed up to a given day during the measurement window from 30 days after the prior-quarter announcement to the current quarter announcement. *TIMELINESS* captures the average daily proportion of total earnings news revealed to the market during the measurement period. Using this measure, Donelson et al. [2012] show that earlier revelation of bad earnings news lowers the likelihood of litigation.

Column 7 of table 8 reports results of estimating equation (2) using *TIMELINESS* as the dependent variable. The estimated coefficient on *TREAT* × *POST* is negative and significant (−0.106, *t*-statistic = −1.88), indicating a decrease in the timeliness of bad-news earnings disclosures by 10.6 percentage points after the adoption of UD laws. This decrease is economically large compared to the mean of this variable in the preadoption period for treatment firms (41.8%). This result confirms our finding in table 3, panel B, and further suggests that managers' incentive to preempt large negative earnings surprises decreases when expected litigation risk decreases.

6. *Reconciliation with Bourveau, Lou, and Wang [2018]*

Our results differ from Bourveau, Lou, and Wang's [2018] finding that firms issue more management earnings forecasts after the adoption of UD laws. However, this difference is not surprising because the two studies examine managerial earnings forecasts of different horizons. Bourveau, Lou, and Wang [2018] test the average effect for all forecasts including both annual and quarterly forecasts. They do not examine annual and quarterly forecasts separately; nor do they examine earnings warnings. As discussed in the introduction, we conjecture that forecast horizon plays a critical role in determining the effect of litigation risk on managers' disclosure incentives: Higher litigation risk will induce managers to issue fewer regular, long-horizon forecasts to lower the chance of being sued for issuing ex post overly optimistic forecasts, but more short-horizon forecasts like earnings warnings to avoid being sued for withholding bad news (Skinner [1994]).

To test our conjecture and to reconcile our findings with Bourveau, Lou, and Wang [2018], we first replicate their results using their design. The results are reported in column 1 of table 9, corresponding to column 3 of Bourveau, Lou, and Wang's [2018] table 3. The dependent variable is the natural logarithm of one plus the frequency of management earnings forecasts issued during a fiscal year, as defined in Bourveau, Lou, and Wang [2018]. The treatment variable is the indicator variable *UD LAW*, as defined in equation (6). The sample period (1998–2007), control variables, and the fixed effects structure are also the same as those used by Bourveau, Lou, and Wang [2018]. Our sample size is 31,424, very close to Bourveau et al.'s (30,873). Our estimate of the coefficient on *UD LAW* is 0.136 (*t*-statistic = 8.84), which is also very close to Bourveau, Lou, and Wang's [2018] esti-

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TABLE 9

Reconciliation with Bourveau, Lou, and Wang [2018]: The Role of Forecast Horizon

Dependent variable:				Quarterly Forecasts			Annual and Quarterly Short-Horizon Forecasts
	All Forecasts	Annual Forecasts	Quarterly Forecasts	Long – Horizon	Medium – Horizon	Short –Horizon	
	1	2	3	4	5	6	7
UD LAW	0.136***	0.101***	0.034*	0.079***	−0.012**	−0.072***	−0.056***
	(8.84)	(5.51)	(2.25)	(5.32)	(−2.77)	(−6.74)	(−3.99)
LMV	0.161***	0.100***	0.104***	0.080***	0.008***	0.059***	0.063***
	(20.48)	(19.07)	(15.41)	(8.80)	(6.31)	(10.72)	(11.28)
BTM	0.015*	0.018**	0.003	0.015**	−0.002*	−0.026***	−0.027***
	(1.73)	(2.37)	(0.50)	(2.62)	(−1.95)	(−7.56)	(−6.80)
ROA	−0.002	−0.002	−0.007	−0.010	−0.004	0.014	0.019
	(−0.08)	(−0.14)	(−0.31)	(−0.60)	(−0.90)	(0.91)	(1.24)
LOSS	−0.089***	−0.062***	−0.051***	−0.041***	−0.002	−0.025***	−0.028***
	(−14.32)	(−10.60)	(−6.98)	(−5.10)	(−0.54)	(−5.22)	(−5.88)
EARN_VOL	−0.291***	−0.157**	−0.220***	−0.203***	−0.026	−0.009	−0.006
	(−3.85)	(−2.25)	(−4.30)	(−4.58)	(−1.57)	(−0.44)	(−0.25)
ABRET	−0.023***	−0.003	−0.024***	−0.021***	−0.001	−0.019***	−0.019***
	(−2.73)	(−0.62)	(−4.92)	(−3.36)	(−0.73)	(−5.11)	(−4.75)
INST_OWN	0.241***	0.177***	0.128***	0.136***	0.013**	0.001	−0.005
	(5.13)	(5.80)	(3.93)	(3.64)	(2.22)	(0.06)	(−0.29)

(*Continued*)

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TABLE 9—*Continued*

Dependent variable:				Quarterly Forecasts			Annual and Quarterly Short-Horizon Forecasts
	All Forecasts 1	Annual Forecasts 2	Quarterly Forecasts 3	Long – Horizon 4	Medium – Horizon 5	Short –Horizon 6	7
LITIGATION RISK	−0.029**	−0.040***	−0.009	−0.012	−0.000	0.015	0.011
	(−2.71)	(−5.53)	(−0.93)	(−1.25)	(−0.11)	(1.67)	(1.16)
HQ state-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (firm-years)	31,424	31,424	31,424	31,424	31,424	31,424	31,424
Adj. R-squared	0.699	0.667	0.640	0.623	0.325	0.519	0.523

This table reports the analyses that reconcile our findings with Bourveau, Lou, and Wang's [2018]. The sample period is 1998~2007. Column 1 replicates Bourveau, Lou, and Wang's [2018] results in column 3 of their table 3. The dependent variable is the natural logarithm of one plus the frequency of earnings forecasts (both annual and quarterly forecasts) issued in a given year. In columns 2 and 3, we separately examine the frequency of annual forecasts and the frequency of quarterly forecasts, respectively. In columns 4 to 6, we separately examine the frequencies of three types of quarterly forecasts based on the timing of their issuance: (i) long-horizon forecasts: those issued prior to the earnings announcement date for the prior quarter; (ii) medium-horizon forecasts: those issued within 30 days after the earnings announcement date for the prior quarter; and (iii) short-horizon forecasts: those issued after 30 days after the earnings announcement date for the prior quarter and before the earnings announcement date for the current quarter. In column 7, the dependent variable includes all annual and quarterly short-horizon forecasts, where annual short-horizon forecasts are annual forecasts issued between 30 days after the earnings announcement date of the third quarter and the annual earnings announcement date. *UD LAW* is an indicator variable that equals to one for firm-years in states that have passed UD laws. It equals to zero for all firms incorporated in states that have not adopted UD laws and for firm-years before the adoption for firms incorporated in states that have adopted UD laws and one otherwise. All regressions include headquarters state-year, industry-year, and firm fixed effects. Standard errors are clustered at the incorporation state level. The numbers in parentheses are t-statistics for the estimated coefficients. All continuous variables are winsorized at the 1% level. Variable definitions are provided in the appendix. ***, **, and * denote significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

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mate (0.121, t-statistic = 4.97). Then, in columns 2 and 3 of table 9, we separately examine the frequency of annual forecasts versus quarterly forecasts, and find that the estimated coefficient on *UD LAW* is much larger for annual forecasts than for quarterly forecasts (0.101 vs. 0.034).[32] This result suggests that the positive effect of UD laws adoption on the frequency of management earnings forecasts, as Bourveau, Lou, and Wang [2018] document, is much stronger for forecasts of longer horizon.

We next classify all quarterly forecasts, including those issued after the fiscal quarter end, into three groups based on the timing of their issuance: (i) those issued before the prior-quarter earnings announcement (long-horizon), (ii) those issued within 30 days after the prior-quarter announcement (medium-horizon), and (iii) those issued more than 30 days after the prior-quarter announcement and before the current-quarter announcement (short-horizon). Note that the short-horizon forecasts in the third group include earnings preannouncements and the measurement timing is the same as that of earnings warnings in the previous analyses.[33] We separately use the frequency of forecasts in each group (after log transformation) as the dependent variable and reestimate Bourveau, Lou, and Wang's [2018] model.

Column 4 of table 9 reports the results for long-horizon forecasts. We find a positive and significant coefficient on *UD LAW* (0.079, t-statistic = 5.32), and the coefficient is larger than that in column 3 for all quarterly forecasts (0.079 vs. 0.034). For medium-horizon forecasts (column 5), the estimated coefficient on *UD LAW* changes to be significantly negative (−0.012, t-statistic = −2.77). For short-horizon forecasts (column 6), we find an even more negative coefficient on *UD LAW* (−0.072, t-statistic = −6.74). In column 7, we show that the results for short-horizon forecasts in column 6 is robust to further including short-horizon annual forecasts, defined as annual forecasts issued between 30 days after the third-quarter announcement and the annual announcement. We use the total number of annual and quarterly short-horizon forecasts as the dependent variable and find that the estimated coefficient on *UD LAW* continues to be negative and significant (−0.056, t-statistic = −3.99).

Columns 4 to 6 show a clear pattern that when the forecast horizon is shorter, the effect of UD laws adoption on the forecast frequency becomes more negative. The effect is significantly positive for long-horizon forecasts issued before the prior quarter announcement, turns significantly negative for medium-horizon forecasts issued early in the current quarter after the prior quarter announcement, and then becomes even more nega-

[32] We find similar results when normalizing the dependent variables; the coefficients on *UD LAW* are 0.169 and 0.060 for annual and quarterly forecasts, respectively.

[33] The main difference is that the previous analyses focus only on warnings and good-news alerts in firm-quarters with large earnings surprises and test warnings and good-news alerts separately, whereas the test here examines all short-horizon forecasts in all firm-quarters together.

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TABLE 10

Reconciliation with Bourveau, Lou, and Wang [2018]: Good-News Versus Bad-News Forecasts

Dependent variable:	Bad-News Quarterly Forecasts			Good-News Quarterly Forecasts		
	Long -Horizon 1	Medium -Horizon 2	Short -Horizon 3	Long -Horizon 4	Medium -Horizon 5	Short -Horizon 6
UD LAW	0.001	−0.005	−0.057***	0.024**	−0.003*	−0.014
	(0.04)	(−1.24)	(−4.66)	(2.25)	(−1.91)	(−1.38)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
HQ state-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations (firm-years)	31,424	31,424	31,424	31,424	31,424	31,424
Adj. R-squared	0.547	0.256	0.401	0.448	0.233	0.385

This table reports the analyses that reconcile our findings with Bourveau, Lou, and Wang [2018] based on good-news versus bad-news quarterly forecasts. We reestimate columns 4 to 6 of table 9 separately for good-news versus bad-news forecasts. We define good-new versus bad-news forecasts based on whether the forecast is above or below the most recent consensus analyst forecast prior to the forecast date. The sample period is 1998~2007. Column 1 (4) is based on the frequency of long-horizon bad (good) quarterly forecasts issued prior to the earnings announcement date for the prior quarter. Column 2 (5) is based on the frequency of medium-horizon bad (good) quarterly forecasts issued within 30 days after the earnings announcement date for the prior quarter. Column 3 (6) is based on the frequency of short-horizon bad (good) quarterly forecasts issued after 30 days after the earnings announcement date for the prior quarter and before the earnings announcement date for the current quarter. *UD LAW* is an indicator variable that equals to one for firm-years in states that have passed UD laws. It equals to zero for all firms incorporated in states that have not adopted UD laws and for firm-years before the adoption for firms incorporated in states that have adopted UD laws and one otherwise. The control variables are the same as those in table 9. All regressions include headquarters state-year, industry-year, and firm fixed effects. Standard errors are clustered at the incorporation state level. The numbers in parentheses are t-statistics for the estimated coefficients. All continuous variables are winsorized at the 1% level. Variable definitions are provided in the appendix. ***, **, and * denote significance at the 1%, 5%, and 10% levels based on two-sided tests, respectively.

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tive for short-horizon forecasts issued late in the current quarter. These results demonstrate that our findings differ from Bourveau, Lou, and Wang's [2018] mainly because the two studies examine management earnings forecasts of different horizon. More importantly, the comparison highlights the critical role of forecast horizon in determining the effect of the adoption of UD laws on management earnings forecasts.

Column 6 of table 9 indicates that the frequency of short-horizon forecasts decreases after the adoption of UD laws. Our findings in section 5 show that after adoption, firms issue fewer earnings warnings when facing large negative earnings surprises, but do not change the issuance of good-news alerts when facing large positive earnings surprises. To provide further evidence regarding what type of forecasts drives the pattern in columns 4 to 6 of table 9, we reestimate each regression separately for good-news versus bad-news forecasts. We follow Anilowski, Feng, and Skinner [2007] and classify good news and bad news for each quantitative forecast using the consensus analyst forecast as the benchmark. Table 10 reports results of this analysis. We find that the negative effect of UD laws adoption in column 6 of table 9 is mainly driven by bad news forecasts (columns 3 and 6 of table 10), which is consistent with our findings based on firms with large negative/positive earnings surprises. In contrast, the positive effect in column 4 of table 9 is mainly driven by good-news forecasts (columns 1 and 4 of table 10).

Overall, the results in tables 9 and 10 indicate that the effect of managerial litigation risk on management earnings forecasts differs by forecast horizon and the nature of news. When the forecast horizon is long and the final earnings number is uncertain, managers issue forecasts to reduce information asymmetry and are mainly concerned about the risk of their forecasts being construed as misleading ex post. In this case, higher litigation risk leads to fewer forecasts, especially of good news. However, when the horizon is short, managers issue forecasts to preempt bad earnings surprises and update market expectation with their material private information (Heitzman, Wasley, and Zimmerman [2010]), and are primarily concerned about the risk of being sued for withholding bad news. Thus, higher litigation risk leads to more short-horizon bad-news forecasts.

7. *Conclusion*

This study examines the causal effect of managerial litigation risk on managers' disclosure of earnings warnings in the face of large earnings shortfalls. We explore the staggered adoption of UD laws, which makes it more difficult for shareholders to sue managers through derivative suits, as an exogenous decrease in managerial litigation risk. Using a DID design, we show that the adoption of UD laws leads to a decrease in managers' issuance of earnings warnings in the face of large earnings shortfalls, especially among firms whose managers face a higher ex ante derivative lawsuit risk prior to the adoption. In contrast, we do not find similar results for

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managers' tendency to alert investors of impending large positive earnings surprises. These results suggest that lower litigation risk leads managers to provide fewer earnings warnings because there is less litigation reduction benefit of such warnings (Skinner [1994]).

We further test our prediction that the effect of managerial litigation risk on management earnings forecasts depends on forecast horizon and reconcile our finding with that of Bourveau, Lou, and Wang [2018], who find that the adoption of UD laws leads to an increase in the frequency of management earnings forecasts. We first replicate Bourveau, Lou, and Wang's [2018] results, and then show that as the forecast horizon becomes shorter, the effect of the adoption on management forecasts turns from being positive to being negative. This finding illustrates the two opposing effects that shareholder litigation risk can exert on managerial disclosure (Healy and Palepu [2001]), and highlights the important role of forecast horizon in determining the net effect of such risk on disclosure.

APPENDIX: VARIABLE DEFINITIONS

Variable	Definition
Quarter-Level Analyses	
ABRET	Buy-and-hold return excessive of market return for quarter *q*.
TIMELINESS	The average proportion of total earnings news revealed up to a given day during the measurement window from 30 days after the earnings announcement date of the previous fiscal quarter to the current quarter's earnings announcement date, calculated following Donelson et al. [2012].
BTM	Ratio of book value of equity to market capitalization at the end of quarter *q*.
DRisk	Ex ante derivative lawsuit risk, measured at the year prior to UD laws adoption year. It is the fitted value from equation (2) of Bourveau, Lou, and Wang [2018].
EARN_SURPRISE	Actual quarterly earnings per share minus the consensus analyst forecast during the 30 days after the earnings announcement date of the previous fiscal quarter, scaled by the stock price at the beginning of the quarter.
EARN_VOL	Standard deviation of quarterly return on assets over the past 20 quarters with a minimum of 10 nonmissing observations.
GNEWS_ALERT	An indicator variable that equals to one if a firm issues a good-news forecast of quarterly earnings in the period between 30 days after the earnings announcement date of the previous quarter and the current quarter's earnings announcement date and zero otherwise, where good news is defined based on whether the forecast is below the most recent consensus analyst forecast prior to the forecast date.

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APPENDIX—*(Continued)*

Variable	Definition
INST OWN	Percentage of institutional ownership at the end of quarter q.
LITRISK	Ex ante class action litigation risk at year t, calculated using the coefficient estimates from model (3) of Kim and Skinner [2012].
Loss	An indicator variable that equals to one if income before extraordinary items for quarter q is negative and zero otherwise.
M&A	An indicator variable that equals to one for firms having merger & acquisitions during quarter q and zero otherwise.
NUM_SEG	Number of business segments and geographic segments for year t.
POST	An indicator variable coded as follows: For a treatment firm-quarter, it equals to one for the period after a firm's incorporation state adopted UD laws. For a control firm-quarter, the variable takes the same value as the matched treatment firm-quarter.
ROA	Income before extraordinary items divided by total asset for quarter q.
SIZE	The natural logarithm of market capitalization at the end of quarter q.
TREAT	An indicator variable that equals to one for firms incorporated in the states that adopted UD laws during the sample period and zero otherwise.
UD LAW	An indicator variable that equals to one for firm-quarters in states that have adopted UD laws. It equals to zero for all firms incorporated in states that have not adopted UD laws and also equals to zero for firm-quarters before the adoption for firms incorporated in states that have adopted UD laws and one otherwise.
YEAR(−3)	An indicator variable coded as follows: For a treatment firm-quarter, it equals to one for the third year prior to the UD laws adoption year and zero otherwise. For a control firm-quarter, the variable takes the same value as the matched treatment firm-quarter.
YEAR(−2)	An indicator variable coded as follows: For a treatment firm-quarter, it equals to one for the second year prior to the UD laws adoption year and zero otherwise. For a control firm-quarter, the variable takes the same value as the matched treatment firm-quarter.
YEAR(−1)	An indicator variable coded as follows: For a treatment firm-quarter, it equals to one for the first year prior to the UD laws adoption year and zero otherwise. For a control firm-quarter, the variable takes the same value as the matched treatment firm-quarter.
YEAR(0)	An indicator variable coded as follows: For a treatment firm-quarter, it equals to one for the UD laws adoption year and zero otherwise. For a control firm-quarter, the variable takes the same value as the matched treatment firm-quarter.
YEAR(1)	An indicator variable coded as follows: For a treatment firm-quarter, it equals to one for the first year after the UD laws adoption year and zero otherwise. For a control firm-quarter, the variable takes the same value as the matched treatment firm-quarter.

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APPENDIX—*(Continued)*

Variable	Definition
YEAR(2)	An indicator variable coded as follows: For a treatment firm-quarter, it equals to one for the second year after the UD laws adoption year and zero otherwise. For a control firm-quarter, the variable takes the same value as the matched treatment firm-quarter.
YEAR(3)	An indicator variable coded as follows: For a treatment firm-quarter, it equals to one for the third year after the UD laws adoption year and zero otherwise. For a control firm-quarter, the variable takes the same value as the matched treatment firm-quarter.
YEAR(4+)	An indicator variable coded as follows: For a treatment firm-quarter, it equals to one for the fourth year after the UD laws adoption year and all later years and zero otherwise. For a control firm-quarter, the variable takes the same value as the matched treatment firm-quarter.
WARNING	An indicator variable that equals to one if a firm issues a bad-news forecast of quarterly earnings in the period between 30 days after the earnings announcement date of the previous quarter and the current quarter's earnings announcement date and zero otherwise, where bad news is defined based on whether the forecast is above the median consensus analyst forecast prior to the forecast date.
Annual-Level Analyses	
ABRET	Buy-and-hold return excessive of market return for year $t-1$.
BTM	Ratio of book value of equity to market capitalization at the end of year $t-1$.
EARN_VOL	Standard deviation of annual return on assets over the past 10 years with a minimum of five nonmissing observations.
INST_OWN	Percentage of institutional ownership at the end of year $t-1$.
LITIGATION RISK	Ex ante class action litigation risk at year t, calculated using the coefficient estimates from model (3) of Kim and Skinner [2012].
LMV	The natural logarithm of market capitalization at the end of year $t-1$.
LOSS	An indicator variable that equals to one if income before extraordinary items for year $t-1$ is negative and zero otherwise.
UD LAW	An indicator variable that equals to one for firm-years in states that have passed UD laws. It equals to zero for all firms incorporated in states that have not adopted UD laws and for firm-years before the adoption for firms incorporated in states that have adopted UD laws and one otherwise.
ROA	Income before extraordinary items scaled by total assets at the end of year $t-1$.

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