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Shareholder Litigation Rights and Stock Price Crash Risk

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Abstract

We study the impact of shareholder-initiated litigation risk on a firm's stock price crash risk. Our empirical analysis takes advantage of the staggered adoption of universal demand laws, which led to an exogenous decline in derivative litigation risk. We find that a decline in the threat of derivative litigation reduces crash risk and that information hoarding associated with earnings management is a channel through which litigation risk affects crash risk. The relationship is also moderated by how exposed firms are to the other primary form of shareholder litigation, namely securities class-action lawsuits.

Keywords: shareholder litigation risk; derivative lawsuits; universal demand; stock price crash risk; securities class action lawsuits; judge ideology

JEL Classification: G14; G30; G38; K22.

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1. Introduction

Corporate governance mechanisms help protect shareholder interests within a firm (Jensen, 1993). One important component of this is the legal protection afforded to shareholders, which reduces agency problems resulting from misalignments in the interests of shareholders and managers (La Porta et al., 1998; Cheng et al., 2010). Litigation rights provide a disciplinary tool for shareholders to influence the behavior of management. This right has been shown to reduce managerial misconduct (Romano, 1991), strengthen the position of the board of directors (Ferris et al., 2007), discourage excessive risk-taking (Ferris et al., 2007; Gormley and Matsa, 2016), and affect the cost of debt (Ni and Yin, 2018) and the value of corporate cash holdings (Nguyen et al., 2018).

Prior studies also find that litigation risk affects a firm's disclosure environment. Bourveau et al. (2018) find that the threat of derivative lawsuits can discourage corporate disclosure. As litigation risk and the associated costs are reduced, firms become more willing to disclose further information. Better corporate disclosure should also reduce the impetus for management to hoard bad news, subsequently leading to a decline in the number of negative stock price shocks that a firm experiences. Because bad news hoarding is generally considered the main cause of stock price crash risk (Jin and Myers, 2006; Hutton et al., 2009; Kim et al., 2011a, b), we expect that a reduction in derivative litigation risk results in a decline in stock price crash risk (hereafter, crash risk).

There is, however, evidence showing that when faced with litigation risk, managers voluntarily disclose bad news pre-emptively to deter lawsuits (Skinner, 1994; Kasznik and Lev, 1995; Johnson, et al., 2001). Therefore, a competing hypothesis is that a decrease in litigation risk reduces disclosure quality and ultimately increases crash risk. As a result, it is unclear which direction the association between litigation risk and crash risk will, on average, dominate.

We attempt to fill this gap in the literature by examining this issue empirically. To do so, we first consider two important issues. The first issue is the type of lawsuit being examined. There are two primary types of legal recourse available to shareholders: derivative lawsuits (hereafter, DLs) and direct-action lawsuits such as securities lawsuits. DLs are filed by shareholders on behalf of a corporation against corporate insiders (Kinney, 1994). They primarily deal with breaches of fiduciary duties by the firm's directors and/or officers. The settlement generally involves changes in the firm's corporate governance practices (Erickson, 2010; Ferris et al., 2007). Any monetary recovery, which is uncommon, goes to the corporate treasury, not to the shareholders. On the other hand, securities class action lawsuits are filed directly by shareholders as plaintiffs. They allege securities fraud and direct harm to the shareholders. These lawsuits tend to be cash-settled.

This paper focuses on DLs because they are the primary legal tool to force corporate governance change within a firm and therefore constitute an important corporate governance mechanism (Erickson, 2010; Ferris et al., 2007). Nevertheless, securities class action suits are also relevant. In fact, these two types of lawsuits sometimes behave as substitutes and sometimes as complements. Choi et al. (2017) find that DLs tend to usually follow security class action suits. For example, a firm that has committed securities fraud may also implicate directors and officers for breaching their fiduciary duties, thus spurring a need for internal corporate governance reforms. If one form of lawsuit is too difficult to file, shareholders may decide to pursue the alternative. As a result, to fully understand the effect of litigation risk on crashes, we need to consider alternative ways in which shareholders can pursue litigation.[1]

Endogeneity is the second important empirical issue that we consider and address substantially in this study. For instance, shareholder litigation risk and crash risk may be simultaneously affected by one or more omitted variable(s). Moreover, shareholders likely consider future crash risks when deciding whether to initiate a lawsuit. These possibilities add to the challenge of establishing a causal effect of litigation risk on crashes and require an identification strategy that utilizes an exogenous variation in the firm-level litigation risk. To address this issue, we follow prior studies and use the U.S. states' staggered adoption of universal demand (UD) laws, which significantly increased the difficulty of filing DLs against corporate insiders (Bourveau et al., 2018; Ni and Ying, 2018; Nguyen, et al., 2018). UD laws are based on a firm's state of incorporation and have been adopted by 23 U.S. states so far. UD laws effectively reduce the ability of shareholders to push through a derivative lawsuit as they first require that the shareholders make a demand to the firm's board of directors to take actions against the alleged wrongdoer(s). Boards usually reject shareholders' demands, often due to a conflict of interest because in many cases, individual directors are named in the lawsuit. The adoption of the UD laws, which took place in different states in different years, provides a plausibly exogenous shock to the threat of DLs faced by a firm's officers and directors. This allows us to establish the causal effect of litigation risk on a firm's crash risk.

Our sample consists of 38,471 firm-year observations of publicly traded companies incorporated in the U.S. from the year 1993 to 2014. We proxy firm-specific stock price crash risk using three measures common in the literature: the negative skewness coefficient of firm-

[1] There is also evidence to suggest that the threat of securities class action lawsuits tends to result in firms voluntarily disclosing more information as a pre-emptive measure to avoid this type of litigation (Dong and Zhang, 2019; Houston, et al., 2019).

specific returns (*NSKEW*), the down-to-up volatility of firm-specific returns (*DUVOL*), and a count (*COUNT*) of the difference in the number of days with negative, as opposed to positive, extreme firm-specific returns (Chen et al., 2001; Jin and Myers, 2006; Hutton et al., 2009).

We use a difference-in-differences (DiD) approach following prior research (Bourveau et al., 2018; Ni and Ying, 2018; Nguyen, et al., 2018). Our treatment group is comprised of firms incorporated in states that have adopted UD laws. Our results show a significant reduction in crash risk among firms incorporated in states that have adopted UD laws. These results hold even when we include a comprehensive set of firm-level control variables and year and firm fixed-effects. These results are also economically significant. The adoption of UD laws leads to a reduction in crash risk by at least 14% of the respective measure's standard deviation.

Next, we conduct several tests to address potential measurement error concerns. We check for pre-trend, i.e. whether crash risk starts to diminish even before a state adopts a UD law. We find no evidence of such a pre-existing trend. Crash risk starts to decline the years after a state adopts UD laws. We also conduct a placebo test to ensure that other unobservable state-level events do not drive our results. In this test, we randomize the state a firm is incorporated in, re-run our baseline regression and repeat the process 1,000 times. The results show no evidence that our baseline results can be generated by a random draw. We also conduct a test to ensure that our results are not driven by specific state-based corporate laws – specifically those considered in Karpoff and Wittry (2018) – that are known to affect the market for corporate control. These laws do not affect our baseline results.

We also examine the role that the risk of securities class action lawsuits can have in moderating the relationship between derivative litigation risk and crash risk. We use two proxies for the risk of firms facing securities lawsuits. The first is the intensity of actual lawsuits on a state-year basis, and the second is based on the work of Huang et al. (2019) who measure ex-ante securities class action risk using a measure of the political ideology of the federal circuit courts. Results from both approaches show that UD laws are more important among firms which have access to fewer legal remedies to protect shareholders.[2]

Finally, we examine whether the improvement in corporate opacity due to the adoption of UD laws is a channel by which stock price crash risk is reduced. Using structural equation modelling, we show that the substantive impact UD laws have on crash risk is through a change in a firm's earnings management. Moreover, we adopt Andreou et al. (2017)'s methodology to analyse breaks in a firm's positive earnings streak and find that, after UD laws, there are fewer stock price crashes that coincide with negative earnings announcements after a string of positive earnings news. This result suggests that there is a significant reduction in bad news hoarding to mask unfavourable earnings after the adoption of UD laws.

We conduct a battery of robustness tests on our main results. To address selection bias, we use the propensity score matching (PSM) approach. Further, to address the sensitivity of our results to the use of varying statistical specifications, we use firm fixed effects and state headquarters-by-year fixed effects as well as the generalized methods of moments estimator instead of panel OLS. Moreover, we conduct additional tests that control for state citizen ideology and state government ideology (Berry et al., 1998), corporate governance, the effect of the ninth circuit states, Delaware incorporation, corporate lobbying, and periods of financial stress. All these robustness tests continue to support our baseline results of the effect of UD laws have on crash risk.

[2] We also examine whether using Kim and Skinner's (2012) model for estimating a firm's ex-ante securities litigation risk moderates the impact that UD laws have on crash risk. While the results of applying their measure of firm ex-ante litigation risk reveals it is significant and positively associated with crash risk, it does not significantly influence the impact that UD laws have on crash risk. This latter result may be due to the fact that some of the variables used in the model to predict class action lawsuits also affect derivative lawsuits. For this reason, we rely on actual, rather than predicted, class action lawsuits for cleaner inference.

This paper makes several contributions to the literature. First, by exploiting plausible exogenous variations in derivative litigation risk, we show that it directly affects a firm's stock price crash risk. While litigation risk has been shown to affect corporate behavior (Ferris et al., 2007; Gormley and Matsa, 2016), the cost of debt (Ni and Yin, 2018), the cost of bank loans (Chu, 2017) and the value of corporate cash holdings (Nguyen, et al., 2018), we show that it can also have a direct impact on a firm's market value. We, therefore, further emphasize the significant role that litigation risk has in shaping the corporate environment.

We also contribute to the derivative lawsuit litigation literature by showing the ramification that changes in corporate disclosure activities (Bourveau et al., 2018) caused by UD laws have on the stock market. In addition, we shed light on the interlinkages between different forms of shareholder litigation. Specifically, we find that the impact of UD laws on crash risk is moderated by how exposed firms are to the other main form of shareholder litigation, namely securities class action lawsuits.

Furthermore, we contribute to the stock price crash risk literature by discovering another important cause of such crashes. Prior research has shown that crash risk is linked to several corporate governance factors, including corporate tax avoidance (Kim et al., 2011a), executive personal compensation sensitivity (Kim et al., 2011b), managerial career concerns (Andreou et al., 2016), CEO overconfidence (Kim et al., 2016), accounting conservatism (Kim and Zhang, 2016), auditor-client relationships (Callen and Fang, 2017), and 10-K report readability (Kim et al., 2019). We show that a key mechanism of corporate governance, through shareholder litigation (La Porta et al., 1998; Cheng et al., 2010), is also directly linked to crash risk due to its impact on a firm’s disclosure regime.

The remainder of this paper is organized as follows. Section 2 outlines the theoretical framework and hypotheses development. Section 3 describes the data and methodology used to assess the relation between stock price crash risk and universal demand laws. Section 4 presents our empirical analysis, while section 5 concludes the paper and presents avenues for future research.

2. Hypotheses Development

Derivative lawsuits are an important mechanism that shareholders can use to force directors and officers to fulfil their fiduciary duties. In particular, Erickson (2010) notes that as the evidential hurdles required to file securities class action lawsuits have risen since the 1995 Private Securities Litigation Reform, many shareholders opted for DLs instead. In derivative lawsuits, as the name implies, the shareholders who launch the lawsuit are the derivative plaintiff, with the firm itself being the plaintiff. The defendants tend to be the managers and/or directors who have allegedly failed to fulfil their fiduciary duties. Therefore, while a successful DL may lead to individual managers having to compensate the firm for damages, it is more common that settlements include corporate governance reforms as a means to mitigate future breaches of fiduciary duties (Erickson, 2010). This can include a range of reforms that limit management's ability to entrench themselves and strengthen board oversight. Ferris et al. (2007), for example, show that the number of independent directors rises following the settlement of DLs.

Even in situations where initiating DLs may be easier than launching securities class action suits, there are impediments to filing a DL. Importantly, before a DL can be filed, shareholders must demand that the firm's board consider the allegations being made. This demand requirement is based on the premise that directors must manage the affairs of firms and, therefore, they are best placed to determine whether there is value in proceeding with derivative lawsuits. However, it is common for the directors to be the actual targets of the lawsuit. This, consequently, reduces the chances that the board will support the filing.

To address such potential conflicts of interest, shareholders can present a 'demand futility exception' to the court by arguing that the majority of directors cannot impartially evaluate the lawsuit. However, the upshot of this is that it incentivizes plaintiffs to focus more on establishing a futility exception to file the lawsuit and effectively bypass the demand requirement (Swanson, 1993). As a response to a rise in lawsuits using this futility argument to substantiate their filings, many states progressively adopted UD laws between 1989 and 2005 that effectively reinstated the need for shareholders to make a demand on the board, and that the lawsuit would only proceed if the board supports it. Appel (2019) shows that derivative lawsuit filings have declined by approximately a third in the states that have adopted UD laws.

As a result of the staggered nature in which states adopted UD laws, a variation in derivative lawsuit risk exists not only between states, depending on whether they have adopted UD laws or not, but also within a state before and after it adopts a UD law. As these UD laws are not a result of firm-specific action, they provide a plausibly exogenous shock to shareholder litigation risk. Therefore, these events have been extensively used in difference-in-differences analysis (Bourveau et al., 2018; Ni and Yin, 2018; Nguyen et al, 2018).

The impact of UD laws on corporate disclosure is of specific interest to our conjectures. Jin and Myers (2006) provide a theoretical argument that the hoarding of bad news is associated with stock price crash risk. While it is advantageous for managers to temporarily withhold bad news, if a sufficiently long pile of bad news continues to build up, managers have little option but to release it, usually all at once. This leads to a corresponding sharp drop in the stock price, resulting in long left tails in stock return distributions. Multiple studies have since provided empirical evidence of this (Jin and Myers, 2006; Hutton et al., 2009; Kim et al., 2011a, b; Callen and Fang, 2017; Kim et al., 2019).

For this study, it is important to understand the effect of UD laws on corporate disclosure. Prior studies offer mixed results. On the one hand, Boone et al. (2019) show that management forecast accuracy declines when derivative lawsuit risk is reduced. Moreover, Appel (2019) argues that the decline in this risk leads to a greater adoption of provisions that entrench management, reducing the pressure for managers to release information. These findings suggest that a lower litigation risk may lead to greater corporate opaqueness, and consequently, a greater opportunity for management to hoard bad news. On the other hand, Bourveau et al. (2018) find that the introduction of UD laws improves corporate disclosure because managers' private costs of better disclosure decline with a lower risk of being sued. They find that firms issue more earnings forecasts, offer richer disclosure in corporate filings and increase disclosures of both positive and negative news. If this is the case, we would expect crash risk to decline in states that have adopted UD laws due to a reduced threat of derivative lawsuit litigation and subsequent improvement in corporate disclosure. Because this relationship can go in either direction, we present our first set of testable hypotheses, $H1_a$ and $H1_b$, as follows:

$H1_a$: The adoption of UD laws leads to a reduction in stock price crash risk.

$H1_b$: The adoption of UD laws leads to a rise in stock price crash risk.

The second hypothesis focuses on the impact that the other primary form of shareholder litigation, namely securities class action lawsuits, can have on the relation between UD laws and crash risk. Even if the derivative litigation avenue becomes more challenging to pursue through a state's adoption of a UD law, in many cases shareholders can litigate through securities class action lawsuits. While securities class action lawsuits are not a perfect substitute to derivative lawsuits, Choi et al. (2017) note that parallel lawsuits are common. Securities fraud, for instance, can implicate directors and/or managers in breaching their fiduciary duties, which offers a ground for derivative lawsuit as well. However, Huang et al. (2019) show that how supportive the courts are of securities class action lawsuits has a significant impact on the likelihood of firms facing this type of litigation. This leads us to conjecture that if the courts are less likely to entertain securities class actions, then the adoption of UD laws will likely have a stronger impact in reducing crash risk as both major avenues of shareholder litigation are now restricted to complainants. This leads to our second hypothesis:

H2: The impact of UD laws on stock price crash risk will be stronger if the risk of securities class action lawsuits is less.

Our final hypothesis focuses on the channel through which UD laws have an impact on crash risk. As Bourveau et al. (2018) show that corporate disclosure improves following the adoption of UD laws, we expect to find a link between a firm's improvement in corporate transparency and crash risk that is directly tied to a state's adoption of a UD law. In particular, Hutton et al. (2009) show that stock price crash risk is positively related to the firm's earnings management, which encourages bad news hoarding. We, therefore, postulate that if UD laws encourage firms to engage less in earnings management, then we should find that a change in firm-level earnings management following a state's adoption of UD law will lead to a reduction in crash risk:

H3: Earnings management is a channel through which a state's adoption of UD law affects stock price crash risk.

3. Data and Methodology

3.1 Sample selection

Our sample period begins in January 1993 and ends in January 2015. Following Bourveau et al. (2018), Ni and Yin (2018), and Nguyen et al (2018), we use a firm's state of incorporation to identify whether it is subject to a UD law. Our sample starts in 1993 as prior to this date we cannot access electronic filings (via U.S. Securities and Exchange Commission (SEC) Edgar) to reliably determine a firm's historical state of incorporation.[3]

To construct our stock price crash risk measures, we collect daily return data for the common stocks of all U.S. publicly listed firms covered by the CRSP database and to which there is firm-level accounting information available in Compustat. I/B/E/S is used to acquire analyst coverage data and institutional ownership data is accessed via Thomson Reuters 13F filings. Following Appel (2019) and Bourveau et al. (2018), we remove firms that change their state of incorporation during our sample period as firms may pre-select, or even migrate to, states that match their preferred legal environment. This leads to our final sample consisting of 38,471 firm-year observations from 4,738 unique firms.

3.2 Methodology

We utilize the three most common measures of stock price crash risk. All three measures are based on firm-specific returns, estimated from the residuals of the market model following Chen, Hong and Stein (2001). The market model we utilize is given below in equation (1) and is estimated on a rolling fiscal year basis:

$$r_{i,t} = a_i + \beta_{1,i} r_{m,t-2} + \beta_{2,i} r_{m,t-1} + \beta_{3,i} r_{m,t} + \beta_{4,i} r_{m,t+1} + \beta_{5,i} r_{m,t+2} + \varepsilon_{i,t} \qquad (1)$$

where $r_{i,t}$ is the daily stock return for firm i, a_i is the intercept, $r_{m,t}$ is the return of CRSP value-weighted market index and $\varepsilon_{i,t}$ is the error term. Following the literature, we also include two-day lead and lag returns of the market index to allow for non-synchronous trading (Dimson, 1979). Firm-specific daily returns ($D_{i,t}$), for firm i, are subsequently estimated using the residuals derived from equation (1). Following the crash risk literature, we log transform the regression residuals to address skewness:

[3] We determine a firm's state of incorporation based on Edgar electronic filings and from Bill McDonald's website (https://sraf.nd.edu/data/).

$$D_{i,t} = \ln\left(1 + \varepsilon_{i,t}\right) \quad (2)$$

Using the firm-specific daily returns, our first measure of stock price crash risk, following Chen et al. (2001), is the negative skewness (*NSKEW*). *NSKEW* is calculated as follows:

$$NSKEW_i = -\left[n(n-1)^{\frac{3}{2}} \sum D_{i,t}^3\right] / \left[(n-1)(n-2)\left(\sum D_{i,t}^2\right)\right]^{\frac{3}{2}} \quad (3)$$

where *n* is the number of observations during the fiscal year *t*.

Our second proxy of crash risk is down-to-up volatility (*DUVOL*). To estimate this measure, we first separate the daily returns for each firm (*i*) into up-day (return is above the annual mean for firm *i*) and down-day (return is below the annual mean for firm *i*) groups for each fiscal year. Next, we calculate the standard deviation for each group. *DUVOL* is then defined as the natural logarithm of the ratio of the standard deviation in the down-day group to the standard deviation in the up-day group.

$$DUVOL_i = \log\left(\left[(n_u - 1) \sum_d D_{i,t}^2\right] / \left[(n_d - 1)\right] \sum_u D_{i,t}^2\right) \quad (4)$$

where n_u is the number of observations in the up-group, and n_d is the number of observations in the down-group. The third measure we use is the difference in the number of times firm-specific daily returns are above, to those that are below, *k* standard deviations of the mean, where *k* is set to generate frequencies of 0.01% in the log-normal distribution (Jin and Myers, 2006).

To examine the relation between derivative litigation risk and future stock price crash risk, we employ a difference-in-differences (DiD) model. Our treatment (control) group includes all firms that are (are not) incorporated in a state that has passed a UD law in the year that it is adopted. More specifically, we employ the following baseline regression in our study:

$$CRASH\ RISK_{i,t+1} = \beta_0 + \beta_1 UDL_{i,t} + \delta X_{i,t} + Year_t + Firm_i + \varepsilon_{i,t} \quad (5)$$

where *i* and *t* represent firm and year, respectively. Our dependent variable, $CRASH\ RISK_{t+1}$, represents either of our three proxies for crash risk ($NSKEW_{t+1}$, $DUVOL_{t+1}$ or $COUNT_{t+1}$). Our independent variable of interest is the indicator variable UDL_t. It is constructed by assigning a value of one to all firms incorporated in a state that has adopted a universal demand law, and zero otherwise.

In Equation (5), $X_{i,t}$ is a vector of firm, industry and state controls that the prior literature has established as determinants of crash risk. Following Chen et al. (2001), Jin and Myers (2006) and Hutton et al. (2009), we control for prior period crash risk using $NSKEW_t$; $KURTOSIS_t$ to capture the kurtosis of firm-specific daily returns; $RETURN_t$ to represent firm-specific daily returns; $SIGMA_t$ to measure the standard deviation of firm-specific daily returns; the logarithm of total assets ($SIZE_t$); the market to book ratio (MB_t); $LEVERAGE_t$, measured as total long-term debt divided by total assets; ROA_t, measured as income before extraordinary items divided by total assets; and share turnover ($TURNOVER_t$).[4]

In addition, as Hutton et al. (2009) note that firm-level earnings management is associated with bad news hoarding and, therefore, crash risk, we control for firm opacity using $ACCRUALS_t$, which is estimated from the modified Jones model (Dechow et al., 1995). Furthermore, we control for the number of analysts following a firm ($ANALYST_t$) as Chen et al. (2001) show that analysts improve the information environment surrounding a firm and therefore impact future crash risk. Also, we control for product market competition using an industry Herfindahl-Hirschman index (HHI), as Giroud and Mueller (2010) indicate it can act

[4] To account for the possibility that using a lag of the dependent variable as part of the control set may bias the coefficient estimates (see Nickell, 1981), when $NSKEW_{t+1}$ is the dependent variable we exclude its lag as a control variable.

as an external governance mechanism. For similar reasons and following the lead of Callen and Fang (2013, 2017), we control for institutional ownership (INS_t) and auditor tenure ($TENURE_t$).

To control for state-level factors that may influence crash risk, we account for macro-economic conditions within the state by including per capita income (PCI_t), gross state product (GSP_t), state-level unemployment ($UEMP_t$), and growth in gross state product (ΔGSP_t). We obtain data for these variables from the University of Kentucky Center for Poverty Research (UKCPR).[5] Also, as Callen and Fang (2015) show that religious adherence of the population can affect crash risk, we include the number of religious adherents relative to the state's population ($RELIGION_t$). The data used to construct this religious adherence variable is acquired from the Association of Religion data archives.[6] A description of how all the variables are calculated is provided in Appendix I.

Our baseline regression model also includes firm fixed effects to account for time-invariant omitted firm-specific factors, and year fixed effects to control for unobservable time-variant effects. As our variable of interest, UDL_t, varies by state, we cluster our standard errors by the state of incorporation of each firm.

4. Empirical results

4.1 Main results

Panel A of Table 1 provides a univariate analysis of our crash risk proxies before and after a state adopts a UD law. We calculate the three-year average of the crash risk measures for both the pre- and post-UD law adoption periods and report them. The third column in the panel shows the difference in the values before and after the event. These statistics show a significant reduction in all three crash risk measures following the adoption of UD laws. $NSKEW_t$,

[5] UKCPR National Welfare Data, 1980-2017. URL: http://ukcpr.org/resources/national-welfare-data

[6] See http://www.thearda.com/Archive/ChCounty.asp

$DUVOL_t$ and *$COUNT_t$* reduce by 0.162, 0.043 and 0.130, respectively. Panel B presents the descriptive statistics for all the variables used in our baseline regressions at either the firm-year or state-year level. The mean for our variable of interest, *UDL_t*, indicates that 13.6% of our observations are from firms incorporated in states that have adopted UD laws.

[Insert Table 1 about here]

In Table 2, we present our baseline results from regressing Equation (5). In columns (1) to (3), the crash risk measures are regressed solely against our variable of interest, *UDL_t*, with the inclusion of firm and year fixed effects. The regression models in columns (4) to (6) also include our complete set of control variables. Supporting $H1_a$, the coefficients of *UDL_t* are negative and significant at the 1% significance level when regressed against *$NSKEW_{t+1}$* and *$DUVOL_{t+1}$*, and at the 5% level for *$COUNT_{t+1}$*. To appreciate the economic significance that the adoption of UD laws has on crash risk, we examine the proportional reduction in crash risk that occurs when a state adopts a UD law relative to the dispersion of each crash risk measure. The results from columns (4) to (6) reveal that crash risk is reduced by 16.0%, 20.1% and 14.4% of a standard deviation when using the *$NSKEW_{t+1}$*, *$DUVOL_{t+1}$* and *$COUNT_{t+1}$* measures, respectively.[7] This further reinforces the results obtained from our univariate analysis presented in Panel A of Table 1 when examining three-year crash risk averages, pre- and post-UD law adoption.

[Insert Table 2 about here]

We next present trend analysis for our variable of interest, *UDL_t*. If *UDL_t* is capturing a significant change in a firm's future crash risk that specifically relates to a state adopting a

[7] These figures are based on dividing the *UDL_t* coefficients in each regression by the standard deviation of the relevant crash risk measure reported in Table 1. We measure economic significance using the standard deviation of the crash risk variables instead of the means as they are all close to the value of zero, implying even small numerical changes to these values will lead to large percentage changes.

UD law, then we should find that if we introduce a lagged UD law indicator variable into our regression it should theoretically produce an insignificant coefficient result. If this is not the case, then it puts the validity of our baseline results into question as there would be evidence of a pre-treatment trend in crash risk just prior to the adoption of the UD laws. In other words, the DiD parallel trends assumption would be invalid. To conduct this test, we replace UDL_t in Equation (5) with a set of new indicator variables. UDL_0 is equal to the value of one during the year of a state's UD law adoption and UDL_{t-1} and UDL_{t-2} is equal to one for the year before adoption and two years before adoption, respectively. Otherwise the values are zero. We also include forward-looking UDL variables (UDL_{t+1} and $UDL_{\geq t+2}$). UDL_{t+1} is equal to one the year after UD law adoption, while $UDL_{\geq t+2}$ is equal to one for two years succeeding the adoption and thereafter. Otherwise, the values are again zero. Including these lead variables allows us to analyze the post-adoption dynamic effects.

The results presented in Table 3 show that the lagged variables' coefficients are statistically insignificant, both individually and jointly, indicating that there is no evidence of a pre-treatment trend in crash risk before the adoption of UD laws.[8] The regression results also show that the impact of a state adopting a UD law on a firm's crash risk is felt the year after it is adopted, as the coefficient estimates of UDL_0 are significant in the regressions to at least the 5% level. In addition, the impact of adopting UD laws continues to have an impact on crash risk in all subsequent years, with the exception being the coefficient for UDL_{t+1} in the *COUNT* regression.

[Insert Table 3 about here]

[8] We do note that while the coefficient estimate for UDL_{t-1} in the third column of Table 3 (i.e., the $COUNT_{t+1}$ model) is insignificant, it is sizable. We, therefore, conduct a couple of robustness checks to test for the joint significance of the lagged coefficients to ensure the parallel trends assumption remains intact. Specifically, we conduct Wald restriction tests of setting the lagged UDL coefficients to zero and also Likelihood Ratio tests of excluding the lagged variables from the regressions. Neither of these tests show that there is joint significance in the lagged variables, supporting our conclusion of the absence of a pre-adoption trend.

Next, we conduct a placebo test to address the possibility that our prior results are driven by unobserved shocks that coincide with the states' adoption of UD laws. Specifically, we randomly assign a firm's location to another state. If our results are driven by unobserved shocks that simultaneously occurred with the adoption of UD laws, we should find that when firms are incorrectly assigned to a state and we re-run our baseline regressions, the variable UDL_t should continue to be significant. To strengthen the test, we repeat this experiment 1,000 times and record the coefficient and t-statistic values for UDL_t from each iteration.

In Figure 1, we present three histograms of the t-statistics for UDL_t generated from the placebo tests conducted for each of the three crash risk measures. For comparison, we include a vertical line in each histogram to represent the estimated t-statistic obtained in our baseline results (from columns (4) to (6) in Table 2 using the true state of incorporation data). The large t-statistics obtained from using the actual data are in contrast to the placebo test t-statistics that are rarely significant.[9] This suggests that our findings are unlikely to be influenced by other unobserved shocks occurring at the same time as states adopt UD laws. For completeness, in Appendix II, we report the number of positive and the number of negative coefficients that are generated from the placebo runs, along with the number of those coefficients that are also significant at the 5% level. These statistics demonstrate an evenly distributed number of positive and negative coefficient values for UDL_t, reinforcing the view that placebo effects are highly unlikely to drive our baseline results.

[Insert Figure 1 about here]

As an additional test, in Table 4, we directly account for the possibility that a number of state-based anti-takeover provisions introduced during our sample period are interfering with our baseline results. These laws can entrench management and weaken the market for corporate

[9] Only 3 out of 1,000 placebo test observations are larger than the true t-statistic for $NSKEW_{t+1}$, 5 for $DUVOL_{t+1}$, and 55 for $COUNT_{t+1}$.

control. Changes in corporate governance practices caused by these laws can, subsequently, also change litigation risk. To deal with this issue, we account for the confounding law changes that Karpoff and Wittry (2018) discuss that have undergone a change in any of the states during our sample period. For each law, we determine the date it was adopted in each state and create an indicator variable equal to one if the law has been adopted and zero otherwise. Specifically, there are three laws that some states enacted during our sample period, business combination laws (*BCL*), directors' duties laws (*DDL*), and poison pill laws (*PPL*).[10]

[Insert Table 4 about here]

Table 4 reports regression results when each confounding law is added individually to the regression and also when we include all three confounding laws together. Throughout the twelve regressions, UDL_t remains negative and significant at the 1% level when regressed against $NSKEW_{t+1}$ and $DUVOL_{t+1}$, while it is significant at the 5% level when regressed against $COUNT_{t+1}$.

4.2 Accounting for securities litigation risk

While it is not appropriate for all cases, parallel lawsuits are not uncommon (Choi et al., 2017), indicating the plaintiffs' ability to switch from one form of shareholder litigation to another. Securities fraud tends to also implicate directors and/or managers in breaching their fiduciary duties, paving the way for derivative litigation. This can imply that even if a UD law is adopted in a state, the effect it has in reducing a firm's exposure to overall shareholder litigation risk may be offset by the ease with which it is possible for plaintiffs to instead launch a securities class action lawsuit.

[10] The two other laws discussed in Karpoff and Wittry (2017) are control share acquisition laws and fair price laws. However, all states that have adopted these laws did so prior to the commencement of our sample period.

To address this issue, we first examine the prevalence of securities class action lawsuits across different states. State courts may be more or less willing to entertain securities class action lawsuits, which can affect the degree to which firms headquartered in that state are exposed to litigation of this nature, potentially moderating the impact that UD laws have on crash risk. We first collect securities class action data from the Audit Analytics database to calculate the proportion of firms that face securities class action litigation on a state-year basis. We then create an indicator variable, $HIGH\ SCA_t$, that is equal to one if a state has a higher proportion than the median of any state for that year, and zero otherwise. We include this indicator variable in the baseline regression model as well as an interaction term with UDL_t. The results reported in Panel A of Table 5 reveal that the coefficient of the interaction term is positive and significant at the 10%, 5% and 1% levels when $NSKEW_{t+1}$, $DUVOL_{+1}$ and $COUNT_{t+1}$ are the dependent variables, respectively. Taken together, these results indicate that the impact of UD laws on crash risk is dampened for firms which are headquartered in states where securities class action is prevalent.

Analysis by Huang et al. (2019) shows that significant variations in judge ideology exist across the federal circuit courts and over time. This results from the circuit court judges being appointed by the US president, whose choice of appointments are typically influenced by the president's political affiliation. Huang et al. (2019) demonstrate that firms located in more liberal-leaning circuits, which tend to be more supportive of shareholders, are 33.5% more likely to be sued in securities class action lawsuits than those in conservative-leaning circuits. This implies that the risk of firms facing shareholder litigation, as a whole, may not diminish as much in liberal leaning circuits when UD laws are imposed relative to those located in conservative-leaning circuits. This will potentially moderate the impact that the adoption of UD laws has on crash risk.

To examine the impact that ex-ante securities litigation risk, proxied via circuit court judge ideology, has on moderating UD laws' impact in reducing crash risk, we first conduct a double-sort of our sample. Similar to Panel A of Table 1, we calculate the three-year averages for the three crash risk measures when we split our sample of firms into pre- and post- UD law periods. We then further split the two sub-samples into firms that are located in either conservative or liberal-leaning circuits. We determine whether a circuit is more conservative or liberal-leaning based on whether Huang et al.'s (2019) judge ideology value for a circuit is below or above the median value in any given year, respectively. Panel B of Table 5 reports the results from this double-sort and shows that the adoption of UD laws significantly reduces crash risk only in conservative-leaning circuits. There is a reduction in $NSKEW_t$ of 0.180, which is significant at the 10% level, once a state, that is associated with a conservative circuit court, adopts UD laws. Likewise, $DUVOL_t$ ($COUNT_{t+1}$) is reduced by 0.058 (0.315), which is significant at the 1% level.

In Panel C of Table 5, we show how circuit court judge ideology moderates the impact of UD laws in a multivariate setting. We create an indicator variable, $LIBJUDGE_t$ that equals one when a firm, in year *t*, is incorporated in a circuit that is considered liberal-leaning, and zero otherwise. This indicator variable is then added as an additional control and also as an interaction with UDL_t to our baseline model. The results indicate that a firm located in a liberal-leaning circuit significantly dampens UD laws' impact on crash risk. Comparing the interaction coefficient sizes with UDL_t suggests that liberal-leaning courts reduce the UD law effect by at least a third ($DUVOL_{t+1}$ being the least impacted). This supports the hypothesis that the impact of UD laws on crash risk will be dampened wherever alternative avenues to shareholder litigation exist.

[Insert Table 5 about here]

Overall, the above results support H2 as they indicate that where securities class action lawsuits are less likely to be entertained by the courts, the impact of a state adopting a UD law leads to a more pronounced effect in reducing crash risk.

4.3. Channel Analysis

Finally, given that all our preceding analysis rests on the premise that the adoption of UD laws improves corporate disclosure, it is important to verify that this is true for our sample of firms. Following Bourveau et al. (2018), we examine three measures capturing the quantity of disclosure that firms release as well as two measures that capture the type/ quality of disclosure. Our measures of disclosure quantity are the frequency of management earnings forecasts, the length of the management discussion and analysis (MD&A) section in 10-K filings, and the frequency of voluntary 8-K filings. Our first measure of disclosure quality is the difference between the number of times a firm releases poor management earnings forecasts compared to good management earnings forecasts, where forecasts are considered to be either good or bad based on whether they are higher or lower than analyst consensus estimates made before these forecasts. Our second measure of disclosure quality captures the precision of management earnings forecasts by measuring the width of the forecast errors (the averaged difference between the upper- and lower-end estimates for each year, scaled by price).

Table 6 presents regression results showing that a firm's corporate disclosure, as captured by the above five measures, significantly improves following the states' adoption of UD laws. Specifically, there is evidence at the 1% significance level that the frequency of management forecasts increases, while the width of management forecasts reduces ($p < 0.05$), and the length of MD&As plus frequency of 8-K fillings both also increase ($p < 0.10$). Moreover, the relative amount of bad to good news forecasts significantly rises, at the 1% level, indicating that firms are less likely hoarding bad news after the adoption of UD laws.

[Insert Table 6 about here]

Table 7 provides a test of our third hypothesis. We postulate that as UD laws improve earnings transparency, crash risk will diminish. We test this in two ways. The first method is based on Hutton et al. (2009) who demonstrate that stock price crash risk is directly related to the firm's financial reporting opacity. In particular, they focus on earnings management and argue that firms that engage less in earnings management are less likely to be hoarding bad news and therefore have reduced crash risk. Therefore, we expect the adoption of UD laws to encourage better disclosure practices which would reduce firms need to engage in earnings management and, consequently, reduce stock price crash risk. To test this, we utilize structural equation modelling (SEM) to consider two paths that the adoption of UD laws can have on crash risk. The first path is a direct route of UD laws impacting crash risk. The second path is a mediated route where UD laws affect the level of earnings management (as captured by our *ACCRUALS* measure) within the firm, which then subsequently affects stock price crash risk. The results in Panel A of Table 7 show that once the indirect route is accounted for within the SEM, the direct route is no longer significant, implying that earnings management is the principal channel through which UD laws affect crash risk.

[Insert Table 7 about here]

In Panel B of Table 7, we take a different approach to consider the same issue. We examine firm-specific stock price crashes caused by earnings announcements that break the past years' strings of successive earnings increases. Prior studies argue that firms with consecutive strings of increasing earnings enjoy higher price premiums and a break in the string causes a substantial decline in the price premium (DeAngelo et al., 1996; Barth et al., 1999; Shanthikumar, 2012). In addition, Myers, et al. (2007) argue that strings of successive earnings increases are driven by earnings management, and in extreme cases, financial accounting fraud.

If UD laws do reduce the propensity to hoard bad news, then we should find a decline in the likelihood of firms experiencing stock price crashes resulting from a break in the string of reported consecutive earnings increases.

In line with Andreou et al. (2017), we create three crash risk string indicator variables. The first is *CRASH BREAK STRING 1* which is equal to one if a firm experiences a stock price crash and earnings have also declined in the current year but increased in the previous year. Otherwise the variable takes the value of zero. *CRASH BREAK STRING 2* and *CRASH BREAK STRING 3* are calculated similarly but require two and three consecutive prior years of earnings increases, respectively.[11] We then use these three measures as dependent variables in logit regressions where we employ our standard set of independent variables. These results show that for each of the three measures, the probability of firms experiencing a crash break string is reduced if the state has adopted UD laws. The average marginal effect from a state adopting a UD law is to reduce *CRASH BREAK STRING 1*, *2* and *3* by 2.90%, 4.42% and 5.02%, respectively.

4.4. Robustness checks.

In this section, we conduct a battery of robustness tests to address selection bias and to test the sensitivity of our results to variations in statistical and sampling design. First, we attempt to address selection bias using the propensity score matching (PSM) approach. Following Ni and Yin (2018), we construct a series of cohort firms for each UD law adoption event by retaining all firms with sufficient data over a seven-year window centred on the event date. We match each treatment firm (i.e. those firms incorporated in a state adopting a UD law in year t+1) within each cohort with a control firm (i.e. firms not incorporated in a state

[11] For these three crash risk string variables, the stock price is considered to have crashed if its firm-specific weekly return is more than 3.09 standard deviations below the mean firm-specific return (see Andreou et al., 2017) for either the same or subsequent year that the firm experiences negative earnings following a string of positive earnings increases.

adopting a UD law in year t+1). Propensity scores, derived in the year prior to a given state adopting a UD law, are based on $SIZE_t$, $LEVERAGE_t$, MTB_t and $ACCRUALS_t$. We repeat this process for each UD law cohort and then stack them together to form our final propensity score matched sample. Panel A of Table 8 reports balance test results, while Panel B shows regression results for the total six-year period surrounding the year treatment firms experience their state adopting UD laws. Despite this analysis using a far more constrained sample, the coefficients for UDL_t remain significant to at least the 5% level.

[Insert Table 8 about here]

Next, we test the sensitivity of our baseline results to different model specifications and sampling conditions and report results in Table 9.

[Insert Table 9 about here]

In columns (1) to (3) in Panel A of Table 9, we report results when utilizing firm fixed effects and state headquarters-by-year fixed effects, and in columns (4) to (6), we show the results when we exclude firms incorporated in states that have adopted UD laws prior to 1995. Doing this ensures the availability of at least two years of observations for each firm before the adoption of UD laws (i.e., capturing both pre- and post-UD law periods for each state) in our sample. As it is common in the crash risk literature to include the lag of the dependent variable in the $NSKEW_{t+1}$ regressions, column (7) shows the results where we include it and utilize the Arellano and Bond (1991) general methods of moments estimator instead of panel OLS, given that OLS estimates may result in bias coefficient results (Nickell, 1981).

Panel B of Table 9 also shows that the baseline results hold when we extend our control variables to include several additional factors that have been shown to be impacted by the adoption of UD laws. This includes corporate cash holdings (Nguyen et al., 2018); executive compensationand corporate governance practices (Foroughi, et al., 2020). We also include two state-year variables to account for the impact that a state's political ideology may have on the propensity that a firm faces litigation. These are state citizen ideology and state government ideology, as first applied by Berry et al. (1998).

Finally, we attempt to test the sensitivity of our baseline results to the use of different samples designed to address the effect of the state of incorporation, corporate lobbying, and the financial crises. We report the results of these tests in Table 10.

[Insert Table 10 about here]

Panel A of Table 10 shows that our baseline results hold when we exclude the ninth circuit court states to deal with the possible confounding effect of the ninth circuit ruling in July 2nd 1999 that resulted in a more stringent interpretation of the pleading standards necessary to launch securities class action lawsuits (Crane and Koch, 2018). This implies that for these states launching a securities class action lawsuit as a possible alternative, where appropriate, to derivative litigation, may be more difficult than in other states. Also, given the bankruptcy protections offered to firms incorporated in Delaware, we show that our results hold if we exclude firms from that state. In Panel B of Table 10, we further show that our results hold when considering the possible impact that corporate lobbying may have in undermining our exogeneity assumption of states adopting UD laws. We follow Nguyen et al. (2018) and Appel (2019) in recognizing that the adoption of UD laws in Pennsylvania was a result of Supreme Court action in the case of Cuker v. Mikalauskas (1997) rather than due to legislative decision-making that could be prompted by corporate lobbying. We set UDL_t to equal one only for firms that are located in Pennsylvania after it adopted the UD law, and zero otherwise. In doing so, we find that our results still hold. Lastly, in Panel C of Table 10, we also show that the baseline results hold when we exclude the years associated with the Asian financial crisis (1997, 1998), the tech bubble (2000, 2001) and the global financial crisis (2007, 2008).

5. Conclusion

Using the staggered adoption of universal demand laws as a plausibly exogenous shock to the risk of derivative litigation that firms face, we examine the impact of the reduction in shareholder's litigation rights on stock price crash risk. As recent research indicates that with a reduction in derivative lawsuit risk, corporate disclosures tend to improve (Bourveau et al., 2018), we hypothesize that this can imply that firms are less likely to hoard bad news and therefore will experience a decline in stock price crash risk. Our results support this view and show that firms incorporated in states which have adopted universal demand laws are less likely to face stock price crash risk.

In addition, to account for the net effect of a change in shareholder litigation risk, we consider the impact that a firm's exposure to securities class action lawsuits has on the relationship. Huang et al. (2019) find that there is variation in the amount of securities class action suits based on which circuit court firms are associated with. Using this variation across circuits, we find that the impact UD laws have in reducing crash risk is more pronounced in firms located in conservative-leaning circuit court jurisdictions. We also find evidence to support the view that earnings management is a channel by which UD laws affect stock price crash risk.

Overall, our results highlight the significant impact that shareholder litigation risk has on a firm's stock price crash risk and that this relationship is moderated by how favorable the courts are to entertaining securities class action lawsuits. This latter point suggests that, when examining shareholder litigation risk, future research should be mindful to consider how supportive the courts are likely to be of shareholder litigation.

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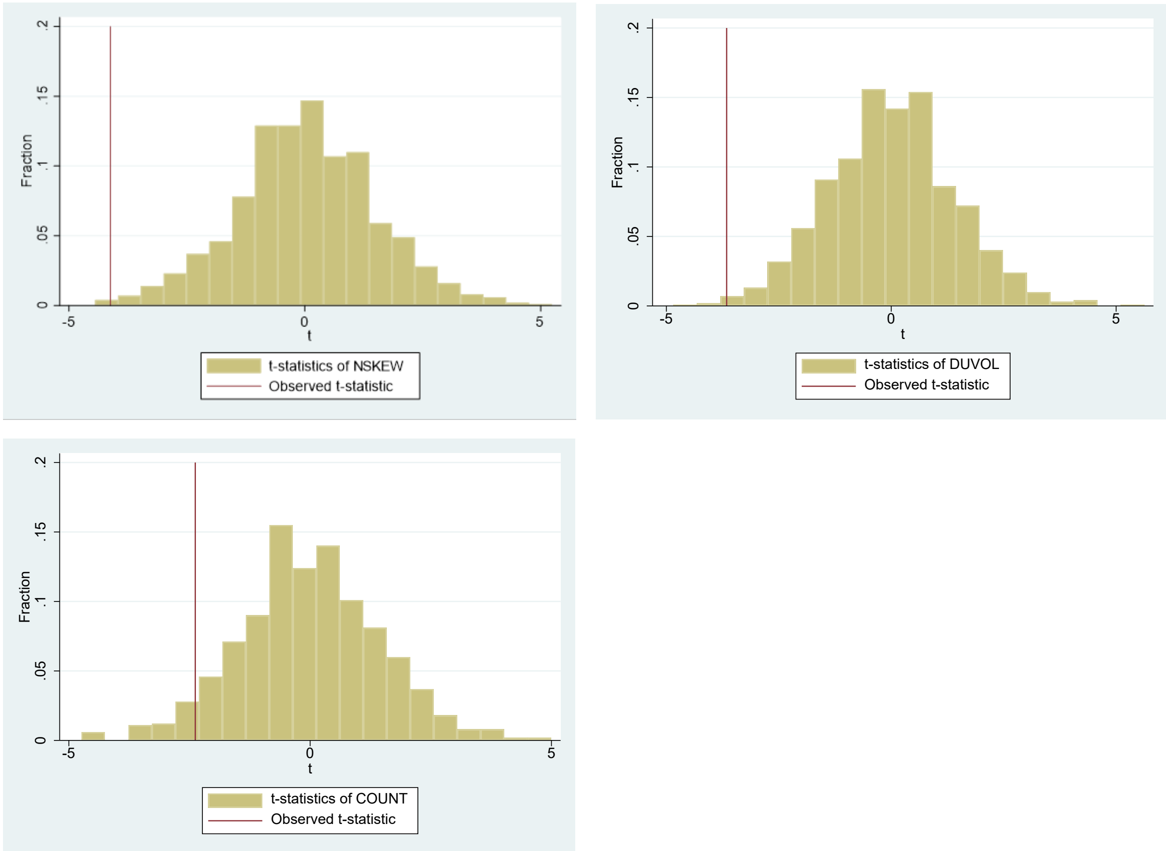
Figure 1. Histograms of t-statistics from placebo tests.



The histogram on the top left plots the distribution of the UDL_t coefficient t-statistics generated from running 1,000 iterations of our baseline regression (Equation 5) where $NSKEW_{t+1}$ is the dependent variable. For each iteration, firms are randomly assigned to a state other than the one they are incorporated in. The vertical line represents the t-statistic generated from using the actual, non-randomly assigned, data. The histogram on the top right shows the results where the dependent variable is $DUVOL_{t+1}$. The bottom histogram shows the results where the dependent variable is $COUNT_{t+1}$.

Table 1. Summary Statistics.

Panel A. Comparative Statistics	**Pre UDL**	**Post UDL**	***Diff***	***t*-Stat**	***P*-Value**
3-year average $NSKEW_t$	-0.047	-0.208	-0.162	2.898	0.005
3-year average $DUVOL_t$	-0.059	-0.102	-0.043	2.977	0.004
3-year average $COUNT_t$	-0.063	-0.194	-0.130	1.948	0.054
Panel B. Descriptive Statistics	**Mean**	**σ**	**P25**	**Median**	**P75**
Dependent variables					
$NSKEW_{t+1}$	0.015	1.374	-0.604	-0.140	0.384
$DUVOL_{t+1}$	-0.054	0.303	-0.244	-0.072	0.111
$COUNT_{t+1}$	-0.132	1.253	-1.000	0.000	1.000
Variable of interest					
UDL_t	0.136	0.343	0.000	0.000	0.000
Firm-level controls					
$NSKEW_t$	0.020	1.297	-0.582	-0.144	0.355
$KURTOSIS_t$	7.546	10.979	1.789	3.586	8.114
$SIGMA_t$	0.030	0.017	0.017	0.026	0.038
$RETURN_t$	-0.058	0.072	-0.071	-0.033	-0.015
$TURNOVER_t$	0.022	0.971	-0.291	0.008	0.310
$SIZE_t$	6.362	1.839	5.026	6.232	7.603
MTB_t	3.005	4.253	1.297	2.082	3.547
$LEVERAGE_t$	0.218	0.205	0.021	0.187	0.343
ROA_t	0.108	0.195	0.067	0.131	0.200
$ANALYST_t$	2.082	0.810	1.386	2.079	2.708
$TENURE_t$	10.349	8.084	4.000	8.000	14.000
HHI_t	0.126	0.087	0.069	0.099	0.157
INS_t	0.665	0.283	0.448	0.704	0.934
$ACCRUALS_t$	0.125	0.132	0.046	0.085	0.154
State-level controls					
PCI_t	3.642	0.695	3.041	3.785	4.183
GSP_t	2.012	3.155	0.432	0.576	2.382
$UEMP_t$	5.237	1.767	4.000	4.700	6.400
ΔGSP_t	0.051	0.029	0.027	0.048	0.068
$RELIGION_t$	0.448	0.070	0.411	0.420	0.448

Panel A compares the three-year average crash risk measures ($NSKEW_t$, $DUVOL_t$ and $COUNT_t$) of firms before and after the state they are incorporated in adopts UD laws. Panel B reports descriptive statistics for all firm-year observations of the variables used in the empirical analysis. The sample consists of 38,471 firm-year observations from 1993 to 2014. All variables are defined in Appendix I. Continuous variables are winsorized at the one percent level. The t-statistics and p-values are from difference in means tests.

Table 2. Baseline regression analysis.

	$NSKEW_{t+1}$ (1)	$DUVOL_{t+1}$ (2)	$COUNT_{t+1}$ (3)	$NSKEW_{t+1}$ (4)	$DUVOL_{t+1}$ (5)	$COUNT_{t+1}$ (6)
UDL_t	-0.147***	-0.043***	-0.150**	-0.220***	-0.061***	-0.181**
	(0.03)	(0.01)	(0.06)	(0.05)	(0.02)	(0.08)
$NSKEW_t$					-0.025***	-0.069***
					(0.00)	(0.01)
$KURTOSIS_t$				-0.005***	0.001***	0.002***
				(0.00)	(0.00)	(0.00)
$SIGMA_t$				-10.423***	-0.979*	2.826
				(1.98)	(0.56)	(2.29)
$RETURN_t$				-0.014	0.382***	2.248***
				(0.32)	(0.08)	(0.29)
$TURNOVER_t$				0.062***	0.014***	0.050***
				(0.00)	(0.00)	(0.00)
$SIZE_t$				0.161***	0.041***	0.126***
				(0.01)	(0.00)	(0.01)
MTB_t				0.020***	0.005***	0.015***
				(0.00)	(0.00)	(0.00)
$LEVERAGE_t$				-0.059	-0.041***	-0.178**
				(0.04)	(0.01)	(0.07)
ROA_t				0.572***	0.144***	0.507***
				(0.05)	(0.01)	(0.04)
$ANALYST_t$				0.169***	0.049***	0.186***
				(0.02)	(0.01)	(0.02)
$TENURE_t$				-0.002	-0.000	0.000
				(0.00)	(0.00)	(0.00)
HHI_t				0.448**	0.087**	0.268
				(0.19)	(0.04)	(0.17)
INS_t				0.103***	0.022***	0.008
				(0.03)	(0.01)	(0.04)
$ACCRUALS_t$				0.175***	0.033***	0.091***
				(0.05)	(0.01)	(0.03)
$RELIGION_t$				-1.035	-0.146	-0.273
				(0.65)	(0.16)	(0.67)
PCI_t				0.172***	0.052***	0.106*
				(0.06)	(0.01)	(0.06)
GSP_t				0.016**	0.003	0.001
				(0.01)	(0.00)	(0.01)
$UEMP_t$				0.013	0.001	-0.010
				(0.02)	(0.00)	(0.02)
ΔGSP_t				-0.249	-0.056	-0.114
				(0.49)	(0.11)	(0.62)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	38,471	38,471	38,471	38,471	38,471	38,471
Adjusted R^2	0.027	0.045	0.026	0.055	0.089	0.052

This table reports panel OLS regression results showing the impact that UD laws have on crash risk based on Equation (5). The sample consists of firm-year observations for the period 1993 to 2014. The first three columns are fixed effects models whereas the remaining three columns also include control variables. Standard errors are in parentheses and are clustered by the state of incorporation. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 3. Parallel trend analysis.

	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$
	(1)	(2)	(3)
UDL_{t-2}	-0.051	-0.010	-0.121
	(0.06)	(0.02)	(0.16)
UDL_{t-1}	-0.019	-0.013	-0.305
	(0.06)	(0.02)	(0.20)
UDL_{t0}	-0.231**	-0.068**	-0.298**
	(0.09)	(0.03)	(0.13)
UDL_{t+1}	-0.191***	-0.054***	-0.170
	(0.05)	(0.02)	(0.12)
$UDL_{\geq t+2}$	-0.207***	-0.063***	-0.280**
	(0.06)	(0.02)	(0.14)
Firm controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
No. of Obs.	37,093	37,093	37,093
Adjusted R^2	0.069	0.102	0.072
Wald F-stats (Coeffs. on $UDL_{t-2} = UDL_{t-1} = 0$)	0.80	0.49	0.20

This table reports results for when UDL_t in Equation (5) is replaced by a set of indicator variables to capture pre- and post-UD law adoption effects. UDL_0, UDL_{t-1}, and UDL_{t-2}, are equal to the value of one during the year of a state's UD adoption, the year before adoption and two years before adoption, respectively, and otherwise zero. UDL_{t+1} is equal to one the year after UD law adoption while $UDL_{\geq t+2}$ is equal to the value of one from two years and onwards following the adoption, and zero otherwise. Standard errors are provided in parentheses and are clustered by the state of incorporation. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 4. Confounding law effects.

	$NSKEW_{t+1}$				$DUVOL_{t+1}$				$COUNT_{t+1}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
UDL_t	-0.241***	-0.217***	-0.220***	-0.242***	-0.065***	-0.060***	-0.061***	-0.066***	-0.193**	-0.179**	-0.182**	-0.194**
	(0.05)	(0.05)	(0.05)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.08)	(0.08)	(0.08)	(0.08)
BCL_t	0.120***			0.145***	0.025			0.030*	0.069			0.080
	(0.04)			(0.05)	(0.02)			(0.02)	(0.07)			(0.08)
DDL_t		-0.167***		-0.187***		-0.037**		-0.038**		-0.112		-0.095
		(0.05)		(0.06)		(0.02)		(0.02)		(0.08)		(0.08)
PPL_t			-0.020	0.030			-0.013	-0.003			-0.085	-0.060
			(0.07)	(0.05)			(0.02)	(0.02)			(0.10)	(0.11)
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	38,471	38,471	38,471	38,471	38,471	38,471	38,471	38,471	38,471	38,471	38,471	38,471
Adjusted R^2	0.055	0.055	0.055	0.055	0.089	0.089	0.089	0.089	0.052	0.052	0.052	0.052

This table reports results when indicator variables are included in Equation (5) to account for the timing of states adopting specific state-based anti-takeover provisions. These provisions are business combination laws (BCL_t), director's duties laws (DDL_t), and poison pill laws (PPL_t). Each indicator variable is equal to one when the particular law is adopted within a state, and zero otherwise. Standard errors are provided in parentheses and are clustered by the state of incorporation. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 5. Impact from security class actions and firm ex-ante litigation risk.

Panel A. State-based security class action intensity				
		$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$
		(1)	(2)	(3)
UDL_t		-0.212***	-0.061***	-0.226***
		(0.04)	(0.01)	(0.06)
UDL x $HIGH\ SCA_t$		0.073*	0.022**	0.106***
		(0.04)	(0.01)	(0.04)
$HIGH\ SCA_t$		-0.016	-0.004	-0.027
		(0.02)	(0.00)	(0.02)
Firm and state controls		Yes	Yes	Yes
Firm and year FE		Yes	Yes	Yes
No. of Obs.		38,471	38,471	38,471
Adjusted R^2		0.069	0.102	0.058
Panel B. Double sort by circuit court judge ideology and UDL_t				
		PRE UDL	POST UDL	*Diff*
3-year average $NSKEW_t$	Conservative	0.029	-0.151	-0.180*
	Liberal	-0.158	-0.187	-0.029
3-year average $DUVOL_t$	Conservative	-0.044	-0.102	-0.058***
	Liberal	-0.074	-0.106	-0.032
3-year average $COUNT_t$	Conservative	-0.014	-0.329	-0.315***
	Liberal	-0.158	-0.307	-0.149
Panel C. Circuit Court Judge Ideology				
		$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$
		(1)	(2)	(3)
UDL_t		-0.266***	-0.067***	-0.177*
		(0.05)	(0.02)	(0.09)
UDL_t x $LIBJUDGE_t$		0.124*	0.023*	0.122**
		(0.07)	(0.01)	(0.05)
$LIBJUDGE_t$		0.018	-0.002	-0.004
		(0.04)	(0.01)	(0.03)
Firm and state Controls		Yes	Yes	Yes
Firm and year FE		Yes	Yes	Yes
No. of Obs.		35,563	35,563	35,563
Adjusted R^2		0.067	0.093	0.053

In Panel A, $HIGH\ SCA_t$, is equal to one if the state has a higher proportion than the median of securities class action suits within any state for that year, and otherwise zero. In Panel B, firms are split based on whether they are located in the jurisdiction of either a conservative ($LIBJUDGE_t = 0$) or liberal ($LIBJUDGE_t =1$) leaning federal circuit court, before and after the state they are in adopts UD laws. We split circuit courts by the sample median of their judge ideology values obtained from Huang et al. (2019) for the period 1996-2014. The t-statistics are from difference in means tests. In Panel C, OLS results that controls for judge ideology are presented. Standard errors are provided in parentheses and are clustered by the state of incorporation. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 6. The Impact of UD laws on Disclosure Quantity and Quality.

	Frequency of MF_{t+1}	*Length $MD\&A_{t+1}$*	*Frequency of VOL_8K $Filings_{t+1}$*	*Bad minus Good News MF_{t+1}*	*Width of MF_{t+1}*
	(1)	(2)	(3)	(4)	(5)
UDL_t	0.142^{***}	0.052^{*}	0.054^{*}	0.082^{***}	-0.011^{**}
	(0.05)	(0.03)	(0.03)	(0.02)	(0.01)
$SIZE_t$	0.190^{***}	0.081^{***}	0.003	0.041^{***}	0.018^{***}
	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)
MTB_t	0.002^{***}	0.002^{***}	0.003^{***}	0.001^{**}	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$LEVERAGE_t$	-0.090^{**}	0.147^{***}	0.110^{***}	0.013	-0.044^{***}
	(0.04)	(0.02)	(0.02)	(0.02)	(0.01)
ROA_t	0.213^{***}	-0.063^{**}	-0.027	0.109^{***}	-0.403^{***}
	(0.04)	(0.03)	(0.02)	(0.01)	(0.12)
$EARN\ VOL_t$	-0.091^{***}	0.034^{***}	-0.007	-0.012^{***}	-0.012^{**}
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
INS_t	0.176^{***}	0.084^{***}	0.167^{***}	0.044^{***}	0.003
	(0.02)	(0.02)	(0.01)	(0.01)	(0.00)
$RETURN_t$	0.572^{***}	-0.397^{***}	0.052	0.251^{***}	-0.825^{***}
	(0.07)	(0.04)	(0.07)	(0.03)	(0.28)
$LOSS_t$	-0.133^{***}	0.048^{***}	0.046^{***}	-0.015^{***}	-0.024^{**}
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
No. Of Obs.	33,788	13,612	38,462	33,777	11,199
Adjusted R^2	0.619	0.840	0.788	0.190	0.726

This table examines the impact that the adoption of UD laws has on a number of disclosure metrics. These metrics and accompanying regression models are calculated similarly to that presented in Bourveau, et al. (2018) and include the frequency of management forecasts, (column 1); the word length of the MD&A section in 10-K filings (column 2); and the frequency of voluntary 8-K filings per year (column 3). We also calculate a measure to determine the difference between the number of times a firm releases good management earnings forecasts relative to bad management earnings forecasts, where forecasts are considered to be either good or bad based on whether they are higher or lower than analyst consensus estimates made prior to these forecasts (column 4); and the width of the forecast errors, calculated as the averaged difference between the upper- and lower-end estimates for each year, scaled by price (column 5). *$EARN\ VOL_t$* represents the standard deviation of the annual return on assets of a firm over the past 10 years while *$LOSS_t$* is an indicator variable equal to one if income before extraordinary items of year *t*-1 is negative, and zero otherwise. The sample of observations differ based on data availability. Columns (1), (4) and (5) cover the period from 1997-2014. Column (3) is for our full sample period and column (2) is based on 1993-2006 data sourced from Stephen Brown's website (https://www.stephenvbrown.com/publication/annual-mda-modifications). Standard errors are provided in parentheses and are clustered by the state of incorporation. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 7. Channel analysis through earnings management and bad news hoarding.

Panel A. Structural Equation Modeling

	$ACCRUALS_t$ (1)	$NSKEW_{t+1}$ (2)	$DUVOL_{t+1}$ (3)	$COUNT_{t+1}$ (4)
Indirect Effect				
UDL_t	-0.014***			
	(0.01)			
$ACCRUALS_t$		0.332***	0.051***	0.156***
		(0.06)	(0.01)	(0.04)
Direct Effect				
UDL_t		0.015	0.008	0.025
		(0.02)	(0.01)	(0.02)
Firm and State Controls	Yes	Yes	Yes	Yes
No. of Obs.	38,471	38,471	38,471	38,471

Panel B. Role of Bad News Hoarding

	Crash Break String 1 (1)	*Crash Break String 2* (2)	*Crash Break String 3* (3)
UDL_t	-0.361***	-0.372***	-0.391***
	(0.14)	(0.14)	(0.14)
Firm and State Controls	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes
No. of Obs.	33,615	33,559	33,519
Pseudo R^2	0.027	0.022	0.022

In Panel A we report results from utilizing structural equation modeling to model both a direct effect that UD laws can have on crash risk (i.e., UDL → CRASH RISK) and a mediated (indirect) effect via earnings management (i.e., UDL → ACCRUALS → CRASH RISK). Panel B reports panel logit results of the impact UD laws have on the likelihood of bad news hoarding. *Crash Break String 1* is an indicator variable equal to one if the firm's earnings increase in the previous year but decrease in the current year and the stock price experiences a crash. Otherwise the variable is equal to zero. *Crash Break String 2* and *Crash Break String 3* are measured similarly except there must be two and three consecutive years of prior earnings increases, respectively. Standard errors are provided in parentheses and are clustered by the state of incorporation. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 8. Robustness: Propensity score matched DiD regressions.

Panel A. Balance test.				
	Mean Treatment cohort (1)	Mean Control cohort (2)	*Diff* (3)	*P*-Value (4)
$SIZE_t$	5.802	5.720	0.082	0.585
$LEVERAGE_t$	0.207	0.192	0.015	0.388
MTB_t	2.579	2.616	-0.037	0.874
$ACCRUALS_t$	0.128	0.129	-0.001	0.968

Panel B. PSM sample regressions.			
	$NSKEW_{t+1}$ (1)	$DUVOL_{t+1}$ (2)	$COUNT_{t+1}$ (3)
UDL_t	-0.243**	-0.076***	-0.269***
	(0.10)	(0.02)	(0.10)
Firm controls	Yes	Yes	Yes
State controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
No. of Obs.	3,753	3,753	3,753
Adjusted R^2	0.077	0.133	0.109

Panel A reports t-tests for the differences in means between the treatment (those firms experiencing their state adopting UD laws) and control groups for the covariates utilized in a logistic regression to generate propensity scores that are then used to match each treatment firm with a control firm. Matching is based on the period before a firm experiences their state adopting a UD law. The caliper for matching is set to 0.05. Panel B shows results from running a regression on the sub-sampled data starting three years before UD law adoption to three years after. Standard errors are provided in parentheses and are clustered by the state of incorporation. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 9. Robustness: Alternative baseline regression models and additional controls.

Panel A. Alternative baseline models

	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$	$NSKEW_{t+1}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
UDL	-0.220***	-0.058***	-0.150**	-0.217***	-0.061***	-0.173**	-0.366**
	(0.06)	(0.02)	(0.07)	(0.06)	(0.02)	(0.08)	(0.18)
Firm & State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed FE	No	No	No	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	No	No	No	No
No. of Obs.	38,471	38,471	38,471	35,681	35,681	35,681	26,191
Adjusted R^2	0.055	0.089	0.051	0.0655	0.091	0.053	
Prob > chi2							0.000
Arellano-Bond test for AR(1) in first differences: Z							-30.151 [0.000]
Arellano-Bond test for AR(2) in first differences: Z							0.765 [0.445]
Sargan test of overidentifying restrictions: Prob > Chi2							0.000

Panel B. Additional controls

	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$
	(1)	(2)	(3)
UDL_t	-0.345***	-0.100***	-0.278**
	(0.09)	(0.02)	(0.13)
$CASH\_RATIO_t$	-0.079	-0.014	-0.123
	(0.20)	(0.05)	(0.13)
CEO_CHAIR_t	0.058**	0.010**	0.001
	(0.02)	(0.00)	(0.02)
E_t	0.004	-0.001	-0.014
	(0.01)	(0.00)	(0.02)
$SALARY_t$	-0.100	-0.015	-0.028
	(0.09)	(0.02)	(0.07)
$DELTA_t$	-0.004	-0.000	-0.002
	(0.01)	(0.00)	(0.01)
$CIT\_IDEOLOGY_t$	-0.001	0.001	0.000
	(0.00)	(0.00)	(0.00)
$GOV\_IDEOLOGY_t$	0.001	0.000	-0.001**
	(0.00)	(0.00)	(0.00)
Standard Firm & State Controls	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes
No. of Obs.	12,281	12,281	12,281
Adjusted R^2	0.055	0.079	0.048

In Panel A, Columns (1) to (3) reproduce our baseline regressions when using headquarter state-by-year fixed effects with firm fixed effects. Columns (4) to (6) restrict the sample to ensure firms in states that adopt UD laws have at least two years of data prior to the adoption (i.e., the states of Georgia & Michigan (1989), Florida (1990), Wisconsin (1991), Montana, Virginia, & Utah (1992) and New Hampshire & Missouri (1993) are dropped). Standard errors are provided in parentheses and are clustered by the state of incorporation. In column (7) we include the lag of the dependent variable ($NSKEW_{t+1}$) as a control variable and use the GMM Arellano and Bond (1991) estimator, instead of panel OLS, with two and three lags used as the instrumental variables. P-values for the regression diagnostics are in brackets. The sample is reduced due to the need to create a balanced panel. Panel B reproduces the baseline results with an extended control set. $CASH\_RATIO_t$ is cash holdings to total assets, CEO_CHAIR_t is an indicator variable equal to one if the CEO is also the chair of the board of directors and zero otherwise. E_t is Bebchuck et al's (2009) entrenchment index, $SALARY_t$ is the dollar value of the CEO base salary in a given fiscal year, and $DELTA_t$ is CEO pay-for-performance sensitivity as defined by Coles et al. (2006). $CIT\_IDEOLOGY_t$ and $GOV\_IDEOLOGY_t$ are state-year based measures to capture state citizen and state government ideology, as defined by Berry et al. (1998). Data for the first four variables are extracted from ISS (formerly Riskmetrics) while data for the last two variables are from https://rcfording.com/state-ideology-data/. Standard errors are provided in parentheses and are clustered by the state of incorporation. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 10. Additional robustness tests.

Panel A. State Exclusions.						
	Ninth circuit states excluded			Delaware Exclusion		
	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$
	(1)	(2)	(3)	(4)	(5)	(6)
UDL_t	-0.249***	-0.067***	-0.208**	-0.221***	-0.059***	-0.179**
	(0.06)	(0.02)	(0.08)	(0.05)	(0.02)	(0.07)
Firm & State controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm & Year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	35,531	35,531	35,531	14,557	14,557	14,557
Adjusted R^2	0.052	0.086	0.049	0.059	0.091	0.051
Panel B. Corporate Lobbying Analysis.						
	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$
	(1)	(2)	(3)	(4)	(5)	(6)
UDL_t	-0.152***	-0.046***	-0.085***	-0.178***	-0.053***	-0.102***
	(0.02)	(0.00)	(0.02)	(0.03)	(0.01)	(0.03)
Firm & State controls	No	No	No	Yes	Yes	Yes
Firm & Year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	38,471	38,471	38,471	38,471	38,471	38,471
Adjusted R^2	0.027	0.045	0.026	0.055	0.089	0.051
Panel C. Periods of financial stress removed.						
	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$	$NSKEW_{t+1}$	$DUVOL_{t+1}$	$COUNT_{t+1}$
	(1)	(2)	(3)	(4)	(5)	(6)
UDL_t	-0.154***	-0.046***	-0.143**	-0.214***	-0.064***	-0.176**
	(0.04)	(0.01)	(0.06)	(0.05)	(0.01)	(0.07)
Firm & State controls	No	No	No	Yes	Yes	Yes
Firm & Year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	25,715	25,715	25,715	25,715	25,715	25,715
Adjusted R2	0.017	0.026	0.018	0.039	0.064	0.039

In Panel A, Columns (1) and (2) report regression results from Equation (5) where all firms that are within the jurisdiction of the ninth federal circuit court are removed from the sample. Columns (3) and (4) show regression results where all firms that are incorporated within the state of Delaware are excluded. In Panel B, the variable UDL_t is set to one for firms incorporated in the state of Pennsylvania after it adopted UD laws, and zero otherwise. Panel C reports results from re-running the regressions when financial crisis periods are excluded (1997; 1998; 2000; 2001; 2007; 2008). Standard errors are provided in parentheses and are clustered by the state of incorporation. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Appendix I. Variables definitions

Definitions of the primary variables used in the baseline regressions are provided below. All variables are winsorized at the one percentile.

Variable Name	Description
Shareholder litigation rights	
UDL_t	Indicator variable set to one if a firm's state of incorporation has adopted a universal demand law, and zero otherwise.
Stock Price Crash Risk Variables	
$NSKEW_{t+1}$	Negative coefficient of skewness of daily firm-specific stock returns during the fiscal year t+1. See equation (3).
$DUVOL_{t+1}$	Natural log of down to up volatility of daily firm-specific stock returns measured over the fiscal year t+1. See equation (4).
$COUNT_{t+1}$	The difference in the number of times firm-specific daily returns are above, to those that are below, k standard deviations of the mean, where k is set to generate frequencies of 0.01% in the log-normal distribution.
Other Variables	
$ACCRUALS_t$	Three year rolling sum of absolute abnormal accruals as estimated from the modified Jones model (Dechow et al., 1995).
$ANALYST_t$	The logarithm of the number of analysts covering a firm in fiscal year.
GSP_t	Annual gross state product in $100 billions.
ΔGSP_t	3-year average growth rate in gross state product.
HHI_t	Industry concentration measured using the Herfindahl-Hirschman Index.
INS_t	Proportion of the firm owned by institutional investors.
$KURTOSIS_t$	Kurtosis of daily firm-specific stock returns over the fiscal year.
$LEVERAGE_t$	Total long-term debt to total assets at the end of fiscal year.
MTB_t	Market to book value of equity at end of fiscal year.
PCI_t	Annual per capita income ($'000s).
$RELIGION_t$	Proportion of religious adherents in a state relative to the population.
$RETURN_t$	Average daily return of firm-specific stock returns over the fiscal year.
ROA_t	Income before extraordinary items over total assets at end of fiscal year.
$SIGMA_t$	Standard deviation of firm-specific stock returns over the fiscal year.
$SIZE_t$	Natural logarithm of total assets at end of fiscal year.
$TENURE_t$	Tenure of a firm's auditor, measured in years.
$TURNOVER_t$	Average monthly share turnover over the fiscal year t minus average monthly share turnover over the previous fiscal year (t-1). Monthly share turnover is defined as monthly share trading volume scaled by total number of shares outstanding for the month.
$UEMP_t$	Annual state level unemployment rate.

Appendix II. Summary statistics of the placebo tests.

This appendix reports the distribution of the coefficients for UDL_t generated from 1,000 placebo regressions. The number of coefficients that are either negative or positive are reported along with the number of positive and negative coefficients that are also significant at the 5% level.

$NSKEW_{t+1}$			
	Observations	Mean	Median
Full sample coefficient distribution			
Positive coefficients	513	0.063	0.052
Negative coefficients	487	-0.065	-0.055
Significant coefficients at the 5% level			
Positive coefficients	91	0.133	0.133
Negative coefficients	86	-0.141	-0.133
$DUVOL_{t+1}$			
	Observations	Mean	Median
Full sample coefficient distribution			
Positive coefficients	496	0.014	0.012
Negative coefficients	504	-0.014	-0.012
Significant coefficients at the 5% level			
Positive coefficients	32	0.036	0.034
Negative coefficients	29	-0.036	-0.034
$COUNT_{t+1}$			
	Observations	Mean	Median
Full sample coefficient distribution			
Positive coefficients	491	0.062	0.054
Negative coefficients	509	-0.063	-0.052
Significant coefficients at the 5% level			
Positive coefficients	35	0.158	0.156
Negative coefficients	38	-0.158	-0.146