# Creditor Rights, Access to Finance, and Stock Price Crash Risk

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# Creditor Rights, Access to Finance, and Stock Price Crash Risk

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**Abstract**

We examine the relation between creditor rights and stock price crash risk by exploiting the staggered enactment of anti-recharacterization laws that gives lenders greater access to the collateral and thus strengthens creditor rights. We find robust evidence that stock price crash risk subsides following the adoption of the laws. The evidence suggests that the strengthened creditor rights after the law enactment increase the borrowing firm's debt capacity and reduce the amounts of assets subjugated to corporate managers, thereby restricting managers' incentives and abilities to hoard bad news. We also find that the negative relation is highly concentrated in firms with higher degree of bank finance dependence and financial constraints, and in poorly governed firms.

*JEL Classification*: G14; G21; G30; G33; K22

*Keywords*: Crash risk, Anti-recharacterization laws, Bad news hoarding, Capital markets, Creditor rights, Principal-agent conflicts, Corporate governance, Financial constraints

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## 1. Introduction

Academic studies contend that stock price crash risk, i.e., the likelihood of a large-scale, unexpected plunge of stock price, is driven by bad news boarding or bad news formation. According to this bad-news-hoarding argument, negative information is stockpiled for an extended period of time until it is released all at once, leading to a steep stock price decline (Hutton, Marcus, and Tehranian, 2009; Jin and Myers, 2006; Kim, Li, and Zhang, 2011a, 2011b; Kim and Zhang, 2016; Kothari, Shu, and Wysocki, 2009). Under the rubric of bad-news-formation, managers tend to keep negative net-present-value (NPV) projects alive, accumulating poor performance. Once the negative NPV projects are abandoned, all accumulated bad news is released to the market, leading to stock price crashes (Chang, Chen, and Zolotoy, 2017; Deng, Gao, and Kim, 2020; Kim, Wang, and Zhang, 2016). Under this line of logics, managers' motives and abilities to hide bad information are important determinants of stock price crash risk. Empirical literature provides evidence in support of this bad-news-hoarding theory of stock price crash risk. Specifically, prior studies identify various firm-specific factors that influence bad news hoarding and thus crash risk. These factors include financial reporting opacity (Hutton et al., 2009) or readability (Kim, Wang, and Zhang, 2019a), accounting conservatism (Kim and Zhang, 2016), corporate tax avoidance (Kim et al., 2011b), corporate governance mechanisms such as institutional investor stability (Callen and Fang, 2013), analysts coverage (Kim, Lu, and Yu, 2019b) or ownership-control wedge (Hong, Kim, and Welker, 2017), and numerous managerial attributes (Andreou, Louca, and Petrou, 2017; Kim, Liao, and Liu, 2021; Kim et al., 2016) such as CEO age, martital status, and overconfidence.

While literature exploring the determinants of stock price crash risk is rich, the impact of creditors on the occurrences of extreme negative outliers in stock returns has mostly been overlooked (with the notable exception of Kim, Wang, and Wu (2019c), and Li, Liu, Pittman, and Yang (2021)), though creditors play a pivotal role in firms' decision making and subsequently shareholders' wealth. To fill this knowledge gap, we aim to provide systematic evidence on whether and how the strength of creditor rights at the economy level influences stock price crash risk at the firm level.

Establishing a causal link between creditor rights and stock price crash risk is a daunting task: a variety of unobserved factors may affect creditors rights as well as the firm's bad news hiding incentives, making the relation observed between the two possibly spurious. To address this endogeneity concern, we exploit exogenous variation in creditors' rights protection induced by the staggered adoption of anti-recharacterization laws that likely enhanced the borrowing firm's debt capacity. Under the US Bankruptcy Code of Chapter 11, the automatic stay clause harms the ability of creditors to repossess the pledged collateral unless the firm transfers the collateral to a bankruptcy-remote special purpose vehicle (SPV). Prior to the enactment of anti-recharacterization laws, the bankruptcy judge had the discretion to recharacterize the sale of assets to the special purpose vehicle (SPV) as a secured loan and include the SPV assets in the firm's (originator's) bankruptcy estate. Such recharacterization of collateral impairs creditor rights during bankruptcy and reduces firms' debt capacity. Also, it enhances the amounts of assets that are subjugated to the control of originators' management team in bankruptcy and leads to investment inefficiency (Fang, 2020). The enactment of anti-recharacterization laws, by strengthening creditors' rights to repossess collateral, enhance firms' borrowing capacity and alleviate borrowing firm's managerial discretion of assets in bankruptcy.

How does the enhanced creditors rights induced by the anti-recharacterization (AR) laws protect shareholder wealth as shown in constraining stock price crash? We advocate two explanations for potential negative impacts of AR laws on stock price crash risk. Firstly, managers are generally encouraged to conceal negative information, or refrain from disclosing income-reducing information, in an attempt to improve their access to the credit market. For instance, Jiang (2008) provides evidence that beating earnings benchmark can reduce a firm's cost of capital. Moreira and Pope (2007) also argue that firms facing recessions have a higher incentive to conceal negative information in order to hide from credit markets the loss that could be translated into higher costs of debt. Liu, Ning, and Davidson (2010) show evidence of significant income-increasing earnings management prior to bond offerings. With the introduction of the AR law, the availability of credit is substantially enhanced (Favara, Gao, and Giannetti, 2021; Li, Whited, and Wu, 2016), which mitigates managers' incentives to hoard bad information or incentivize them to provide a thorough disclosure of negative news.

Secondly, the adoption of AR laws can be negatively related to stock price crash risk if AR allows for a lower amount of assets included in the bankruptcy estate that are subject to the authority of borrowing firm's managers in times of bankruptcy. Before the enactment of AR laws, managers have more liberty to misuse the assets and overinvest in negative NPV projects (Fang, 2020). Research on stock price crash risk documents that overinvestment in negative NPV projects leads to accumulation of poor performance and corresponding bad information generated by the poorly selected projects; this, in turn, increases stock price crash risk and brings about crash occurrences, once such projects are abandoned (Chang et al., 2017; Deng et al., 2020; Kim et al., 2016). However, AR laws reduces the ability of borrowing firms to overinvest; this lowers stock price crash risk, for example, by decreasing the quantities of assets subjugated to the control of debtors' managers.

We empirically test the relation between the passage of AR laws and stock price crash risk using a large sample of U.S. public firms. Consistent with the view that AR laws can restrict managers' incentives, opportunities, and abilities to hoard bad news, we find evidence that enhanced creditors rights accompanied by the enactment of AR laws significantly mitigate borrowers' stock crash risk as demonstrated by a reduced magnitude of negative conditional skewness and "down-to-up volatility", all shown to capture left-tail risk in stock returns (Chen, Hong, and Stein, 2001).

Equally important, we validate our parallel trend assumption that is crucial to the difference-in-differences (DiD) analysis in a staggered regulatory setting; we validate this assumption by showing no noticeable pre-trend differences between treated and control groups before the enactment of AR laws. To address a potential endogeneity concern that the statutes are jointly influenced by the lobbying efforts from firms (most likely to be financial and securitization firms) to discard the possibility of recharacterization and their tendency to hoard bad information (which is likely correlated with the lobbying efforts), we examine the determinants of the timing of the AR laws using a proportional hazard model. We find no significant evidence that a firm's crash risk is related to the expected duration (or the probability) to pass the laws. As a robustness check, we restrict our samples to a propensity-score-matched (PSM) sample and use alternate definitions of sample periods as well as various measures of stock price crash risk.[1] We continue

[1] For example, we measure stock price crash risk using a binary variable that indicates crash if a firm experiences more than one price crash week in a fiscal year, and the number of crash weeks in a fiscal year.

to find robust evidence in support of the negative association between stock price crash risk and creditor rights in the post period after the enactment of AR laws.

Next, we explore the mechanisms through which stronger creditor rights lead to lower stock price crash risk in the future. Firstly, to shed light on the attenuation effect deriving from the improved credit availability, we show that the AR laws are more effective in lowering future stock price crash risk in firms with greater dependence on external bank credit, as shown by a larger fraction of bank loans, and in those with higher financial constraints, as shown by a higher HP financial constraint index developed by Hadlock and Pierce (2010).[2] Additionally, Almeida and Campello (2007) argue that a firm's asset tangibility increases the value that can be captured by creditors in default states and relieves a firm's financial constraints.  Mann (2018) show that patents are a growing source of pledged collateral to raise significant debt financing. We find that the negative impact on crash risk of strengthened credit rights in the post-AR law period is more prominent for firms with less patents or less tangible assets, suggesting that firms with more patents to be pledged or with high asset tangibility do experience their credit constraints being lowered in general

Finally, we test whether the negative impact on crash risk of strengthened creditor right in the post-AR law period arises from the AR laws limiting borrowing firms' misappropriation of assets in times of bankruptcy. To this end, we explore the differential impact of AR laws on firms with strong versus weak corporate governance. Yu (2008) advocate that analysts serve as external monitors to managers and Chen, Harford, and Li (2007) and Almazan, Hartzell, and Starks (2005) find that institutional shareholders can use their ownership rights to hold managers accountable for actions that do not promote shareholders' best interests. Hence, we separate samples based on

[2] For more detail about the construction of the HP index, see discussions in section 6.

analyst coverage and institutional ownership and show evidence that the negative association between strengthened creditor rights in the post AR-law period and stock price crash risk is more pronounced in firms with low analyst coverage and institutional ownership.

The paper adds to the existing literature in the following ways. First, to our knowledge, our study is the first to examine on the impact of creditor rights on stock price crash risk. The lack of relevant on the issue is surprising, given that creditors are one of the most influential stakeholders behind major corporate decisions, including investment, capital structure, and information disclosure. Among prior studies on crash risk, Kim et al. (2019c) and Dang et al. (2022) exploit a shock to bank competition – the interstate branching deregulation – that shifts banks' monitoring and screening behavior and show that bank competition reduces a firm's crash risk. Li et al. (2021) exploit the merger between lenders and shareholders of the same firm as an exogenous shock to dual ownership and show that dual holders have strong information advantage and monitoring incentives, thereby reducing firms' expected crash risk. Unlike most of prior studies that emphasize the monitoring role of creditors, our study focuses on enhanced creditor rights that lead to an increased credit capacity of the borrowing firm (Favara et al., 2021; Li et al., 2016) and a reduced control of assets by the borrowing firms' management team in bankruptcy (Fang, 2020). We provide evidence that firms' crash risk subsides following the adoption of AR laws that protect creditors against the automatic stay clauses. And such attenuation effect is stronger in firms that are more credit constrained and poorly governed.

Secondly, the findings of this study contribute to the recent line of inquiry that examines the impact of the staggered adoption of AR statutes. This literature shows that the passage of AR laws increases firms' access to debt financing, an important channel that translates into higher financial leverage (Li et al., 2016), less corporate leasing (Chu, 2020), stronger bargaining power over customers (Billett, Freeman, and Gao, 2021), greater efficiency in bankruptcy process (Fang, 2020), mitigation of fluctuations from uncertainty shocks (Favara et al., 2021), and higher investments in social initiatives (Attig, 2021). In this study, we find that the adoption of AR laws restricts firms' incentives and abilities to hoard bad news, which in turn leads to lower stock price crash risk. We extend this line of work by showing the AR laws can exert an important influence on shareholder value by disincentivizing managers' opportunistic bad-news-hiding behavior.

The paper proceeds as follows. Section 2 briefly reviews related literature and develops the hypotheses. Section 3 describes data and research design. Section 4 shows the main results of our empirical analyses, together with methods to address concerns about potential endogeneity. Section 5 shows whether the adoption of AR laws affects managerial bad news hoarding, which is the major cause of crashes. Section 6 presents the cross-sectional analyses. Section 7 concludes.

## 2. Relevant literature and hypothesis development

### *2.1. AR laws and relevant literature*

According to the U.S. Bankruptcy Code of Chapter 11, collateral underlying secured lending is subject to automatic stay, which prevent secured lenders from seizing the pledged collateral that is part of the bankruptcy estate.[3] Automatic stay can be evaded if the originating firm transfers the collateral to a bankruptcy-remote SPV. This is because SPV assures lenders that obligations will be fulfilled even if the originating firm files for bankruptcy. However, the bankruptcy-remoteness of a SPV cannot guarantee the avoidance of automatic stay if the bankruptcy court rules that the transfer of the collateral to SPV is a secured loan rather than a true

[3] Section 541(a) of the Bankruptcy Code states that upon the filing of bankruptcy petition, an estate is created, which consists of "all legal or equitable interests of the debtor in property as of the commencement of the case". The property may be included in the debtors' estate even if debtors do not have a possessory interest in that property (Pearce, 2011).

sale, a ruling known as recharacterization. The recharacterization of collateral and the automatic stay harm creditors' rights and reduce issuing firms' debt capacity (Favara et al., 2021; Li et al., 2016) though it favors business continuation, Furthermore, when sales to SPVs are recharacterized as secured loans, assets transferred to SPV become the property of the bankruptcy estate, which increases the amounts that assets managers of the borrowing firm can control (Fang, 2020). The ARL laws are thus enacted to enhance creditor protection. The AR laws discard the possibility of recharacterization by bankruptcy judges, and thus strengthens creditors' rights to repossess collateral pledged in SPV.

Recent academic studies show that the laws, by improving the collateral value of the issuing firms, have a real impact on firms' investment and financial policies. Li et al. (2016) first document that there is an upward shift in financial leverage after the adoption of AR laws while Chu (2020) argues that the ease of collateral repossession makes corporate leasing less attractive. Favara et al. (2021) examine the impact of the AR law on firms' precautionary behavior and show that the passage of the AR laws leads borrowing firms to gain better access to credit markets and shields firms from fluctuations in uncertainty. Predicated on the similar arguments for the AR laws, Attig (2021) find that a greater access to credit increase corporate social responsibility initiatives while Billett et al. (2021) contend that a relaxed debt constraint improves firms' bargaining position with powerful customers and allow them to cut down on trade credit.

***2.2. Hypotheses development***

Firms have incentives to hoard bad performance or unfavorable information (or negative news) in order to obtain favorable terms from creditors. Liu et al. (2010) find that firms tend to engage in income-increasing earnings management prior to bond offerings. Similarly, Jiang (2008) show that beating earnings benchmark can lower a firm's cost of capital. Moreira and Pope (2007) also argue that firms are motivated to conceal negative information from credit markets since negative information could lead to higher costs of debt. Prior research on stock price crash risk has consistently shown that managerial bad news hoarding is the major driving force for future stock price crashes (among others, Hutton et al., 2009; Jin and Myers, 2006; Kim et al., 2019a; Kim et al., 2011a, 2011b; Kim et al., 2019b; Kim et al., 2021; Kim et al., 2016; Kim and Zhang, 2016; Kim, Li, and Li, 2014; Kothari et al., 2009). The adoption of AR laws, however, has significantly reinforced creditors' rights to repossess collateral, which in turn enhances a firm's borrowing capacity (Chu, 2020; Favara et al., 2021; Li et al., 2016). With better access to the credit market, borrowing firms' incentives to hoard bad information is alleviated. Therefore, we argue that the introduction of AR laws can lower stock price crash risk as the implementation of AR laws improves borrowing capacity for a firm, and thereby curb the firm's incentives to hoard bad news from the credit market.

Secondly, the enactment of AR laws decreases the amounts of assets that are subjugated to the control of originator's management team in bankruptcy state (Fang, 2020). In a world with agency conflicts, managers are inclined to overinvest as they can derive perquisites from those investments (Jensen, 1986; Stulz, 1990). Overinvestment in negative NPV projects fosters managers' reluctance to releases negative information regarding the poorly selected projects. It thereby leads to accumulation of poor performances and bad information, and eventually triggers crash occurrences once the poor performances materialize at their maturity and the corresponding bad news are brought to light (e.g., Chang et al., 2017; Deng et al., 2020; Kim et al., 2016). With the enactment of AR laws, the propensity of borrowing firms to overinvest decreases since managers have less amounts of assets at their discretion to make overinvestments or exploit private benefits. The adoption of AR laws thereby reduces stock price crash risk by increasing firms' debt capacity and/or decreasing the quantity of assets subjugated to the control of debtors' managers. To provide systematic evidence on this unexplored issue, we propose and test the hypothesis below, stated in alternative form:

> **Hypothesis:** *The enhanced creditor rights following the enactment of anti-recharacterization laws reduce a borrowing firm's incentive to hoard bad news, and thereby lowers the stock price crash risk.*

## 3. Data and methodology

### *3.1. Sample selection*

We source the accounting and finance information for U.S. public firms from the *Compustat* and stock return data from the Centre for Research in Security Prices (*CRSP*) database over the period of 1994 to 2008. Our sample period starts three years prior to the first passage of the state AR laws in 1997 in Texas, and ends three years after the last state in 2005 in Nevada.[4] Following prior studies (e.g., Chang et al., 2017; Hutton et al., 2009; Kim et al., 2016), we exclude financial firms (SIC codes between 6000 and 6999), firms with year-end share prices below $1, those with fewer than 26 weeks of stock return data in the fiscal year, firm-year observations with negative total assets and book values of equity, and those lacking financial data to compute relevant variables in our analysis. Following Favara et al. (2021), we restrict our sample of firms to have available information on the state of incorporation (Compustat item *INCORP*). These selection criteria yield an initial sample of 43,915 firm-year observations (6,737 unique firms). Our treatment firms include firms incorporated in Texas, Louisiana, Alabama, or Delaware that have passed the AR laws in our sample period.

[4] Our results are robust to alternative sample with a five-year window around the adoptions of AR laws (Ersahin, 2020; Favara et al., 2021).

### 3.2. Measuring stock price crash risk

We follow Hutton et al. (2009) and calculate firm-specific weekly returns by estimating the following:

$$r_{j,\tau} = \alpha_j + \beta_{1,j} r_{m,\tau-1} + \beta_{2,j} r_{i,\tau-1} + \beta_{3,j} r_{m,\tau} + \beta_{4,j} r_{i,\tau} + \beta_{5,j} r_{m,\tau+1} + \beta_{6,j} r_{i,\tau+1} + \varepsilon_{j,\tau} \quad (1)$$

where $r_{j,\tau}$ is the weekly return on stock $j$ in week $\tau$, $r_{m,\tau}$ is the return on CRSP value-weighted market index, and $r_{i,\tau}$ is the Fama and French value-weighted industry index in week $\tau$. The lead and lag terms of the market and industry returns are included to consider nonsynchronous trading (Dimson, 1979). We use weekly returns to mitigate concerns about thinly traded stocks and estimate weekly returns from Wednesday to Wednesday to avoid any contaminating effects from weekends and Mondays (Wang, Li, and Erickson, 1997). The firm-specific weekly return ($W_{j,\tau}$) is calculated as the log value of one plus the residual return from Eq. (1), i .e., $W_{j,\tau} = (1 + \varepsilon_{j,\tau})$.

Following Chen et al. (2001), our primary measure of stock price crash risk is negative conditional skewness (*NCSKEW*). It is defined as the negative of the third moment of each stock's firm-specific weekly returns divided by the standard deviation raised to the third power. For firm $j$ in fiscal year $t$, this measure is computed as follows:

$$NCSKEW_{j,t} = -[n(n-1)^{3/2} \sum W_{j,\tau}^3] / \left[(n-1)(n-2)(\sum W_{j,\tau}^2)^{3/2}\right] \quad (2)$$

where $n$ is the number of observations of weekly returns in fiscal year $t$. Firms with high *NCSKEW* are more likely to experience a stock price crash.

Our second measure of firm-specific crash risk is "down-to-up volatility" (*DUVOL*), which is calculated as follows:

$$DUVOL_{j,t} = log\left\{(n_u-1) \sum_{Down} W_{j,\tau}^2 / (n_d-1) \sum_{Up} W_{j,\tau}^2\right\} \quad (3)$$

where $n_u$ and $n_d$ are the number of up and down weeks over the fiscal year *t*, respectively. For each stock *j* over fiscal year *t*, we partition all firm-specific weekly returns into down (up) weeks when the weekly returns are below (above) the annual mean. We then calculate the standard deviation of firm-specific weekly returns for each group separately. *DUVOL* is the log ratio of the standard deviation in the down weeks to the standard deviation in the up weeks. A stock with a higher value of *DUVOL* is likely to be more crash prone. Compared to *NCSKEW*, this alternative measure of crash risk may be less influenced by a handful of extreme returns as it does not involve the third moments (Chen et al., 2001).

### *3.3. Identification and empirical model*

We design a difference-in-differences (DiD) test to examine the effect of the passage of AR laws on firm-specific stock price crash risk. Specifically, we estimate the following model:

$$Crash\ Risk_{i,t+1} = \beta_0 + \beta_1 ARL_{j,t} + X_{i,t}\gamma + \ Year_t \ + State_j \ + \varepsilon_{i,t}, \qquad (4)$$

In the above, our outcome variable $Crash_{it+1}$ is one of our firm-specific crash risk measures in year $t+1$, i.e., *NCSKEW* or *DUVOL*. Our key variable of interest is $ARL_{jt}$, which is defined as a dummy variable that is equal to one if firm *i* is incorporated in state *j* with an AR law passed at *t* or earlier, and zero otherwise. Specifically, $ARL_{jt}$ is equal to one for firms incorporated in Texas or Louisiana after 1997, for firms incorporated in Alabama after 2001, in Delaware after 2002, in South Dakota after 2003, in Virginia after 2004, and in Nevada after 2005. Given the staggered nature of the law, the control sample comprises not only firms incorporated in states that never adopt the laws, but also firms in states that would eventually introduce the laws but had not yet introduced them in year *t*. Our hypothesis is supported if we observe that $\beta_1$ is significantly negative, suggesting that the introduction of AR laws can effectively curb managers' incentives to hoard bad information and thus reduce the firm's crash risk.

$X_{it}$ stands for a host of control variables that are considered to be correlated with stock price crash risk according to prior literature (e.g., Chen et al., 2001; Hutton et al., 2009; Jin and Myers, 2006). For example, Detrended stock trading volume ($DTURN_t$), defined as the difference between the average monthly share turnover in fiscal year $t$ and that in fiscal year $t$–1, captures the heterogeneity of investor opinions. Also, Chen et al. (2001) find that past stock return mean and volatility are more inclined to crash in the future for stocks with a higher degree of divergence in investor opinions. We thus include stock return volatility ($SIGMA_t$) and past stock returns ($RET_t$). We also add firm-specific controls such as firm size ($SIZE_t$), market-to-book ratio ($MB_t$), and financial leverage ($LEV_t$), and return on assets ($ROA_t$). To account for potential persistence of the third moment of stock returns, we add past stock price crash risk ($NCSKEW_t$). Opacity ($ACCM_t$) is defined as the absolute value of discretionary accruals; the discretionary accruals are the residuals estimated from the modified Jones (1991) model. We account for the effects of accrual management since financial reporting opacity is positively associated with future stock price crash risk (Hutton et al., 2009). Appendix A provides the definitions of all variables used in the paper. To mitigate the effect of outliers, we winsorize all continuous variables at the 1% and 99% percentiles of their distributions.

Table 1 presents the summary statistics for the key variables used in our baseline regressions. The sample consists of 43,915 firm-year observations covering the period of 1994 to 2008. The mean values (standard deviation) of future stock price crash risk measures, $NCSKEW_{t+1}$ and $DUVOL_{t+1}$ are −0.070 (0.812) and −0.052 (0.386), respectively, which are very similar to those reported by prior studies, e.g., Kim et al. (2011b) and Kim et al. (2019a).

[Insert Table 1 about here]

## 4. Empirical results

This section reports our main findings and explains how concerns about potential endogeneity are addressed. In addition, we demonstrate that our baseline results are robust to alternate measures.

### *4.1. Baseline regression results*

Table 2 presents the results for our estimation of Eq. (4). We use negative conditional skewness (*NCSKEW*) as the proxy for crash risk in Columns 1 and 3 and down-to-up volatility (*DUVOL*) as the proxy in Columns 2 and 4. All outcome variables are measured in year *t+1*. All specifications include a set of controls for time-varying firm characteristics that might determine stock price crash risk. All of the *t*-statistics are based on standard errors that are clustered at the state level (Petersen, 2009). [5] Our baseline results are robust to the use of standard errors clustered at the firm or year level (demonstrated in Panel A of Table 5).

In Columns 1 and 2, we include state and year fixed effects in the regression. The coefficients on the key variable of our interest *ARL* are negative and statistically significant at the 1% level. The magnitude is economically significant as well: Given that the unconditional mean (standard deviation) of *NCSKEW* is -0.070 (0.812), the AR laws reduce negative conditional skewness by 0.053, which is translated into 75.7% (6.5%) reduction of the absolute value of the unconditional mean (standard deviation) of *NCSKEW*. For down-to-up volatility, the passage of AR laws reduces *DUVOL* by 0.024, which corresponds to 46.2% (6.2%) reduction of the absolute

[5] Note that Ersahin (2020) also cluster standard errors at the state of location level. Clustering standard errors by firm relies on a strict assumption that standard errors are uncorrelated across firms (Cameron and Miller, 2015; Ullah and Giles, 2016), therefore we cluster standard errors at a more aggregate level.

value of the unconditional mean (standard deviation) of *DUVOL,* which is -0.052 and 0.386, respectively. In Columns 3 and 4, we further control for firm fixed effects to account for the impact of time-invariant firm-level heterogeneity. We continue to find that *ARL* exert a negative and significant effect on both *NCSKEW* and *DUVOL*, showing that the results cannot be explained by omitted firm-specific factors such as industry structures or corporate cultures. In all, the results are in support of our hypothesis that the passage of AR laws could mitigate managers' incentives to hoard bad news and thereby lead to lower stock price crash risk.

[Insert Table 2 about here]

### ***4.2. Parallel trend condition***

The aforementioned interpretation is warranted only if the parallel trend assumption is satisfied, i.e., there is common trend between the treated and control samples before the enactment of the laws. If firms with lower propensity to hoard bad information are more motivated to lobby for the passage of the laws, then one could observe non-parallel trend in crash risk even before the introduction of the laws. To evaluate whether the parallel trend assumption holds, we investigate multi-period dynamic effects of AR laws. In particular, we substitute the dummy variable *ARL* in Eq. (4) with binary variables indicating the years before or after the enactment of the laws. We show our results for the dynamic effects in Table 3. We study the differences in crash risk in 2 and 4 years before the introduction of the AR laws. *Years 2+ before ARL* (*Years 4+ before ARL*) is a binary variable equal to one for firms incorporated in states that will pass the laws in 2 (4) years or more. *Years 2+ after ARL* (*Years 4+ after ARL*) is a binary variable equal to one for firms incorporated in states that have passed the laws 2 (4) years ago or more. *Years 1-2 before ARL* (*Years 1-4 before ARL*) is a binary variable equal to one for firms incorporated in states that will pass the laws in 1 to 2 years (1 to 4 years). Analogously, *Years 1-2 after ARL* (*Years 1-4 after ARL*) is a binary variable equal to one for firms incorporated in states that have already passed the laws 1 to 2 years (1 to 4 years) ago. We investigate the parallel trend assumption for both *NCSKEW* and *DUVOL*. In Table 3, the estimated coefficients on *Years 2+ before ARL and Years 1-2 before ARL* in Columns 1 (for *NCSKEW*) and 2 (for *DUVOL*), and the estimated coefficients on *Years 4+ before ARL and Years 1-4 before ARL* in Columns 3 (for *NCSKEW*) and 4 (for *DUVOL*) are all statistically insignificant. This shows that there are no significant pre-trend differences in crash risk between treated and control samples before the introduction of the laws, which is consistent with the parallel trend assumption. Notably, the estimated coefficients on *Years 2+ after ARL and Years 1-2 after ARL* in Columns 1 and 2, and those on *Years 4+ after ARL and Years 1-4 after ARL* in Columns 3 and 4 are all negative and statistically significant, indicating that the effect of the laws materializes only after they are passed.

[Insert Table 3 about here]

### *4.3 Propensity-score matched samples*

Our last attempt to mitigate potential endogeneity is to re-estimate the DiD regression in Eq. (4) using a propensity-score matched sample. So far, we offer evidence that there is negative relationship between the passage of AR laws and stock price crash risk. However, firms incorporated in states that adopt the AR laws (*ARL*-firms or treatment firms) can be systematically different from firms incorporated in states that do not adopt the AR laws (non-*ARL* firms control firms) along some observable firm characteristics, and such characteristics could potentially affect the firm's stock price crash risk. We hence conduct propensity-score matching (PSM). To this end, we first estimate the propensity scores using a probit regression model with *ARL* as the dependent variable and all our control variables in our baseline model, as well as industry, state, and year fixed effects as the independent variables. Next, we match each treatment firm with a control firm based on the propensity score (the predicted probability) from the first-stage probit regression. In doing so, we apply the one-to-one nearest neighbor-matching technique without replacement. Panel A of Table 4 presents the diagnostic tests of our propensity-score matches. The results of *t*-test (*T-STAT*) show no significant difference in major firm characteristics between the treatment and the matched control samples after the PSM, indicating that PSM has been effective in eliminating the differences between the treatment firms and the control firm. Panel B of Table 4 re-estimate Eq. (4) using the propensity-score matched samples. Again, the coefficients are negative and statistically significant for both *NCSKEW* and *DUVOL*, suggesting that *ARL*-firms have lower stock price crash risk than non-*ARL* firms. And importantly, the differences in crash risk observed between the treated and PSM control samples cannot be ascribed to the firm-specific heterogeneity such as financial leverage (*LEV*), which is repeatedly shown to be an important outcome of passing the AR laws (e.g., Li et al., 2016), accrual management (*ACCM*) or profitability (*ROA*). The PSM results thereby lend further support to the full sample results of the negative relation between the adoption of AR laws and firm-specific stock price crash risk.

[Insert Table 4 about here]

### *4.4 Robustness checks*

In this section, we check whether our results are robust to the use of alternative samples and standard error clustering schemes. The results are presented in Table 5. To check whether our results are unduly influenced by the 2008 financial crisis, we re-estimate our DiD regression in Eq. (4) after excluding observation in the post-2007 period. As shown in Columns (1) and (2) of Table 5, Panel A, we find that our results remain unaltered, suggesting that our main results are robust to the inclusion of observations during the 2008 financial crisis. Also, we re-estimate Eq. (4) using firm or year level clustering to adjust standard errors. As shown in Columns (3) and (4) for firm-level clustering and Columns (5) and (6) for year-level clustering, our results are robust to the use of alternative clustering methods.

Next, we repeat the baseline analysis using alternative measures of stock price crash risk. First, following Hutton et al. (2009) and Kim et al. (2016), we use an alternative measure of crash risk, *CRASH*, which is an indicator variable that takes the value of 1 when a firm experiences at least one crash week during the fiscal year, and zero otherwise. Crash weeks are considered as those weeks during which the firm-specific weekly returns as defined in Eq. (1) are 3.09 standard deviations *below* the mean firm-specific weekly returns over the fiscal year. 3.09 is chosen to generate a frequency of 0.1% in the normal distribution. Second, as in Callen and Fang (2015) and Hong et al. (2017), we employ another measure of crash risk: the number of crash weeks minus the number of jump weeks over the fiscal year ($COUNT$). We define jump weeks in the opposite way to crash weeks, namely those weeks during which the firm-specific weekly returns are 3.09 standard deviations *above* the mean firm-specific weekly returns over the fiscal year. Again, 3.09 is chosen to generate a frequency of 0.1% in the normal distribution. The effects of AR laws on alternative measures of crash are shown in Panel B of Table 5. The coefficients on *ARL* continue to be negative and statistically significant. It shows that the negative relationship between the adoption of AR laws and crash risk is robust to using alternative measures of crash risk.

[Insert Table 5 about here]

## 5. Underlying mechanisms

### *5.1 Is the timing of AR laws random?*

One potential concern of using the enactment of AR laws as a natural experiment setting for identification is that the passage of the laws is heavily lobbied by financial entities, such as investment banks and credit rating agencies (Kettering, 2009). Even though the concern might be alleviated in our study since the state-level laws are reasonably exogenous to stock price crash risk among *nonfinancial* firms that are headquartered in the state, we directly investigate whether the timing of the AR law enactment is associated with state-level factors and stock price crash risk.

We estimate the Weibull (Weibull, 1951) proportional hazard model which examines the "time-to-event" outcome, where the "failure event" is the adoption of AR laws in a U.S. state. The hazard rate function takes the form:

$$h\left(t, X_{t,\beta}\right) = h_0(t)\exp(X'\beta) \ , \tag{5}$$

where $X_t$ is a vector of time-varying covariates; $\beta$ is a vector of unknown parameters to be estimated; and the baseline hazard rate, $h_0(t)$, is $pt^{p-1}$ with shape parameter $p$ that will be estimated from the data. All explanatory variables included are measured at the state level. States are dropped from the sample once they pass the AR laws. We have one observation for each state in each year up to and including the year of adoption, which yields 666 observations. Table 9 reports the coefficients $\beta^*$ scaled by the Weibull shape parameter, which indicate the percentage change in the time to adopt the AR laws for a one-unit change in the covariates.

In Column (1) and (3) of Table 6, we include only the average stock price crash risk for each state as the independent variable of interest. We further incorporate a set of macro-level variables in Column (2) and (4), including GDP level and GDP growth, population, political preferences, and unemployment rate. The insignificant estimation results across all columns suggest that neither state-level stock price crash risk nor these state-level variables predict the AR law adoption timing. Overall, the findings are consistent with the view that the timing of such state-level law is plausibly exogenous to stock price crash risk, which validates our identification strategy.

[Insert Table 6 about here]

### *5.2 Managerial bad news hoarding*

In this section, we explore whether the adoption of AR laws affects future stock price crash risk by altering managers' propensity to hoard bad news. Previous research suggests that managerial bad news hoarding is the primary cause for future stock price crashes (e.g., Hutton et al., 2009; Jin and Myers, 2006; Kim et al., 2011a, 2011b; Kim and Zhang, 2016; Kothari et al., 2009). According to this bad-news-hoarding theory, negative information is accumulated for an extended period of time; when it is released all at once, there will be a large-scale decline in stock price. Hence, it is important to demonstrate whether the passage of AR laws, by easing the access to external capital markets and decreasing the amounts of assets subjugated to debtors' managerial control, reduces managers' incentives or ability to hide bad information.

To test the bad-new-hoarding mechanism, we measure the degree of bad news hoarding/revelation using: (i) stock market response to negative earnings surprise; and (ii) residual short interest. For the first measure, we intend to capture the amount of news impounded in stock prices prior to the earnings announcement. One can gauge the content of bad information contained in stock prices by evaluating the sensitivity of accumulated abnormal stock returns (*CAR*) over the window of (-10, 1) to the quarterly negative earnings surprise (*N_SUE*). Abnormal stock returns are defined as returns in excess of CRSP value-weighted market return. Earnings surprise (*SUE*) is the forecast errors scaled by the fiscal-year-end stock price with earnings forecast drawn from the median value of I/B/E/S quarterly consensus forecasts. If managers have incentives to reveal bad information, we expect that stock prices or returns impound most of the bad earnings news and react strongly to negative earnings surprises. We then examine whether the adoption of AR laws temper or enhance the stock market responses to *N_SUE* by interacting *ARL* with *N_SUE*.

Panel A of Table 7 displays our estimates. In Column 1, we show our results using state, industry and year fixed effects to account for the impact of industry-specific heterogeneity, geographical characteristics and macroeconomic trends. In Column 2, aside from state and industry fixed effects, we include year×quarter fixed effects to further control for the impact of seasonality. In Column 3, we force our identification of coefficients within the same firm by using firm fixed effects, and we also control for year×quarter fixed effects. In all specifications, the coefficients on $N\_SUE_t \times ARL_t$ are positive and significant at the 1% level, suggesting that the passage of AR laws fosters managerial revelation of negative information as it leads to more adverse earnings news being impounded in stock prices.

Secondly, we follow Bao, Kim, Mian, and Su (2019) to measure the managers' tendency to withhold bad news by computing residual short interest. Bao et al. (2019) propose that residual short interest is a good proxy for the degree of managers' private bad information and show that residual short interest is a reliable predictor of future stock price crashes. The reasoning is that managers and short sellers share the same negative private information (since managers tend to leak private information to short sellers) and therefore managers of firms with higher short interest would possess more negative private information, which is not yet reflected in stock prices. To ensure that short interest purely capture the shorting demand due to bets on the stock price decline, we follow Bao et al. (2019) and use the residual short interest which is purged of factors not relevant to negative private information such as the supply of loanable shares (proxied by institutional ownership), increasing use of hedge-based short strategies (proxied by the presence of convertible securities), and market-wide factors (proxied by a time-trend variable). Specifically, the residual short interest (*ResSI*) is defined as the residuals from the regression of quarterly raw short interest on institutional ownership, convertible bonds indicator, and calendar quarterly fixed effects. The calculation of residual short interest is described in detail in Appendix A.

The estimated effects of the passage of AR laws on the amounts of managers' private bad information as captured by *ResSI* are shown in Panel B of Table 7. In Columns 1 and 2, we include (calendar) quarter fixed effects and state-level fixed effects. The coefficients on *ARL* are negative and significant at the 5% level. The results continue to hold when we use firm fixed effects (in lieu of state fixed effect) in Columns 3 and 4. Collectively, the above results show that the introduction of AR laws can reduce the extent of managers' private bad information, and can therefore result in lower stock price crash risk.

[Insert Table 7 about here]

## 6. Cross-sectional analyses

Our main conjecture with respect to the effect of the adoption of AR laws is that it can strengthen creditor rights and facilitate access to external capital markets. Managers are incentivized to withhold negative information that could be translated into higher costs of capital. The improved ability to borrow may therefore reduce the extent to which managers tend to withhold negative information in order to seek better access to the capital market. Therefore, the adoption of AR laws is more likely to decrease stock price crash risk in firms with larger dependence on bank debt or financially constrained (i.e., those with limited access to capital markets).

We operationalize the mediating effect of credit dependence by using a firm's bank loan ratio, defined as the amount of cumulative bank loan scaled by the total assets. We identify firms as being more (less) dependent on bank capital if they have above-median (below-median) bank loan ratio. We create an indicator variable *Loan* that equals one for firms with above-median bank loan ratio and zero otherwise. As shown in Column 1 of Table 8, we find that the coefficients on *ARL* × *Loan* are negative and significant for both *NCSKEW* and *DUVOL*. This finding is in line with the view that the AR laws are more effective in reducing future stock price crash risk in firms that have greater dependence on external bank credit.

Secondly, we use the HP (Hadlock-Pierce) index developed by Hadlock and Pierce (2010) to capture the mediating impact of financial constraints.[6] We define *HPINDEX* as an indicator variable that takes the value of one if the firm has above-median HP index, and zero otherwise. In Column 2 of Table 8, the coefficients on *ARL* × *HPINDEX* for both measure of crash risk are negative and significant. The result shows that the enactment of AR laws, by easing access to external financial market, can reduce firm-specific crash risk, and such effect is more pronounced for firms that are considered as financially constrained.

[Insert Table 8 about here]

[6] Hadlock and Pierce (2010) show that firm size and age play a prominent role in shaping a firm's financial constraint status.

The enactment of AR laws increases a firm's debt capacity, which in turn alleviates managers' incentives for bad news hoarding to pursue low costs of external financing. In this subsection, instead of relying on an index measure for a firm's financial constraint status, we evaluate a firm's availability to external credit markets  based on the firm's asset pledgeability. Almeida and Campello (2007) argue that a firm's asset tangibility enhances the value that can be captured by creditors in default states and eases a firm's financial constraints. Moreover, Mann (2018) asserts that patents are an important source of pledged collateral to raise significant debt financing. Hence, firms with more patents to be pledged are likely to experience less constrained access to credit. As before, we therefore predict that the effect of *ARL* is more pronounced for firms that are considered to be more constrained financially, for example, for firms with low asset tangibility or less pledgeable patent collateral, and vice versa. Therefore, the effect of *ARL* is expected to be strong (weaker) when firms have low (high) availability of pledgeable collateral and low asset tangibility.

We define firms of low asset tangibility as firms having below-median tangibility ratio (*LOW_TANGIBLE*); the tangibility ratio is defined as tangible assets scaled by total assets. We use the patent counts as well as patent citations to capture a firm's number of patents. We extract information about patents issued to US firms from Kogan, Papanikolaou, Seru, and Stoffman (2017). We then define two variables: (i) *CPAT* (the natural logarithm of one plus total number of patents filed); and (ii) *CWPAT* (citation-weighted patent counts). The weight is computed as one plus the number of citations scaled by the average number of cites to patents issued in the year *t*. Firms with *CPAT* and *CWPAT* below the sample median are classified as *LOW_CPAT* and *LOW_CWPAT*, respectively.

We demonstrate the mediating effects of collateral pledgeability by interacting *ARL* with *LOW_TANGIBLE*, *LOW_CPAT* and *LOW_ CWPAT*. The results are presented in Table 9. We find that the coefficients on *ARL* × *LOW_TANGIBLE, ARL* × *LOW_ CPAT* and *ARL* × *LOW_ CWPAT* are negative and significant for both measures of crash risk. This finding indicates that the impact of AR laws is stronger for firms with less pledgeable collateral or lower asset tangibility. Combined together, we provide strong evidence that that the negative impact of AR laws on crash risk is more prominent when firms are more financially constrained (e.g., Chu, 2020); this evidence is consistent with the notion that AR laws reduce crash risk by improving firm's debt capacity or enhancing firms' ability to gain access to the external credit market.

[Insert Table 9 about here]

Lastly, we examine whether the constraining effects of AR laws arise from the AR laws limiting borrowing firms' control of assets in times of bankruptcy. To this end, we explore the differential impact of AR laws on firms with strong corporate governance versus those with weak governance. Chen et al. (2007) and Almazan et al. (2005) advocate that institutional shareholders can use their ownership rights to hold managers accountable for actions that do not serve shareholders' best interests, e.g., concealing bad information from investors as a way to increase their job security (Kothari et al., 2009).  At the same time, Yu (2008) argue that analysts are deemed as external monitors of management and have been actively involved in discoveries of earnings manipulation or corporate fraud. Thus, the effects of AR laws should be less prominent for firms that are more actively monitored by analysts or institutional shareholders. We hence divide our samples according to the levels of analysts coverage and institutional ownership of the firm. We define firms as poorly governed if firms have institutional ownership (*LOW_INSOWN*) or analysts coverage (*LOW_COVER*) below the respective sample median.

In Table 10, we present the cross-sectional regression results. As shown in Table 10, we find that the coefficients on *ARL* × *LOW_INSOWN* are negative and significant for both measures of crash risk. This finding suggests that the impact of AR laws is more pronounced for firms with lower institutional shareholding. Furthermore, the coefficients on *ARL* × *LOW_COVER* are also negative and significant, indicating that the impact of AR laws is more pronounced for firms with lower analyst coverage. Taken together, it shows that the impact of AR laws on constraining management teams' control of assets is more pronounced for firms with weaker corporate governance.

[Insert Table 10 about here]

## 7. Conclusions

This study investigates the impact of the AR law enactment on stock price crash risk at the firm level. The introduction of AR laws enhances creditors rights to repossess collateral and thereby increases a firm's debt capacity. Moreover, the laws reduce the amounts of assets that are subjugated to the control of a firm's management team in bankruptcy. We show that when a firm's credit capacity is improved and managers' control of assets are mitigated because of the laws, firms experience less stock price crash risk. We further examine the underlying mechanism through which the AR laws influence bad news hoarding (and thus crash risk), and provide evidence that the degree of bad news hoarding subsides following the introduction of the laws. The negative relation between the staggered adoption of AR laws and firm-specific stock price crash risk is robust to using various crash risk measures. We find no noticeable parallel trends in crash risk between the treated and control groups before the adoption of the laws and no evidence that a firm's crash risk determines the lobbying efforts or the timing of the passage of the laws.

Finally, we show that the negative relation between the AR law enactment and crash risk is stronger for firms that are more dependent on bank credit, for those with higher financial constraints, and for those with less pledgeable assets. The above evidence lends support to the notion that AR laws reduce crash risk by increasing a firm's access to external capital market. Further, the effect of AR laws is more prominent when firms are poorly governed, consistent with the view that AR laws operates via reducing the amounts of assets that are under the control of borrowing firms' management teams in bankruptcy.

Evidence provided in this study highlights that protection of creditors rights have a non-trivial impact on the protection of shareholder wealth as it can reduce the occurrences of experiencing stock price crash risk. In this way, we demonstrate an alternative angle to explore creditors and shareholders' conflicts, and offer a different perspective from those adopted in prior literature (e.g., Chava, Livdan, and Purnanandam, 2009; Chava, Wang, and Zou, 2019; Chu, 2018). Further, we contribute to existing literature in accounting and finance that focuses on the determinants of managerial bad news hoarding decisions (e.g., Ali, Li, and Zhang, 2019; Bao et al., 2019; Skinner, 1994) and that searches for determinants of firm-level crash risk (e.g., Kim et al., 2019a; Kim et al., 2011b). Our finding that enhanced creditor rights lead to lower crash risk emphasizes that the interrelation among creditors' rights, borrowers' corporate governance and shareholders welfare could be a promising avenue for future studies.

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## Table 1. Descriptive Statistics

This table reports the descriptive statistics for variables used in the baseline empirical analyses. The sample consists of 43,915 firm-years observations for 6,737 public U.S. firms over the period of 1994-2008. All variables are winsorized at the 1% and 99% levels. Variable definitions are listed in Appendix A.

| Variable | N | Mean | Std. Dev. | $25^{th}$ | Median | $75^{th}$ |
|---|---|---|---|---|---|---|
| $NCSKEW_{t+1}$ | 43,915 | -0.070 | 0.812 | -0.511 | -0.100 | 0.315 |
| $DUVOL_{t+1}$ | 43,915 | -0.052 | 0.386 | -0.305 | -0.062 | 0.184 |
| $LAW_t$ | 43,915 | 0.084 | 0.277 | 0.000 | 0.000 | 0.000 |
| $DTURN_t$ | 43,915 | 0.063 | 0.917 | -0.213 | 0.016 | 0.298 |
| $SIGMA_t$ | 43,915 | 0.065 | 0.033 | 0.040 | 0.058 | 0.082 |
| $RET_t$ | 43,915 | -0.261 | 0.282 | -0.332 | -0.165 | -0.080 |
| $SIZE_t$ | 43,915 | 5.498 | 1.938 | 4.073 | 5.384 | 6.782 |
| $MB_t$ | 43,915 | 3.116 | 3.404 | 1.257 | 2.065 | 3.544 |
| $LEV_t$ | 43,915 | 0.197 | 0.183 | 0.017 | 0.165 | 0.322 |
| $ROA_t$ | 43,915 | -0.015 | 0.195 | -0.021 | 0.036 | 0.078 |
| $NCSKEW_t$ | 43,915 | -0.069 | 0.745 | -0.502 | -0.098 | 0.306 |
| $ACCM_t$ | 43,915 | 0.082 | 0.102 | 0.021 | 0.049 | 0.100 |

## Table 2. Creditor rights and stock price crash risk: Baseline results

This table presents the regression results of the effect of anti-recharacterization laws on firm-level stock price crash risk. The dependent variable, crash risk, is proxied by negative conditional skewness (*NCSKEW*) and down-to-up volatility (*DUVOL*) in year $t$+1. *ARL* is an indicator variable that equals one for firms incorporated in Texas or Louisiana after 1997, in Alabama after 2001, in Delaware after 2002, in South Dakota after 2003, in Virginia after 2004, and in Nevada after 2005. Other variable definitions are presented in Appendix A. The numbers reported in parentheses are $t$-statistics based on standard errors clustered at the state level. $^{***}$, $^{**}$, and $^{*}$ denote statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ |
| $ARL_t$ | -0.053*** | -0.024*** | -0.063*** | -0.036*** |
| | (-3.87) | (-3.17) | (-2.94) | (-3.67) |
| $DTURN_t$ | 0.016*** | 0.008*** | 0.019*** | 0.009*** |
| | (4.72) | (4.90) | (4.54) | (4.30) |
| $SIGMA_t$ | 4.477*** | 1.946*** | 1.115 | 0.682* |
| | (6.64) | (6.58) | (1.38) | (1.84) |
| $RET_t$ | 0.532*** | 0.239*** | 0.240*** | 0.132*** |
| | (8.53) | (8.53) | (3.65) | (4.37) |
| $SIZE_t$ | 0.057*** | 0.027*** | 0.166*** | 0.080*** |
| | (15.18) | (15.83) | (13.35) | (14.30) |
| $MB_t$ | 0.016*** | 0.008*** | 0.026*** | 0.013*** |
| | (9.66) | (8.86) | (13.71) | (15.36) |
| $LEV_t$ | -0.236*** | -0.105*** | -0.365*** | -0.186*** |
| | (-7.23) | (-7.67) | (-6.30) | (-6.43) |
| $ROA_t$ | 0.196*** | 0.120*** | 0.273*** | 0.147*** |
| | (8.70) | (12.60) | (6.61) | (8.71) |
| $NCSKEW_t$ | 0.013* | 0.006 | -0.132*** | -0.061*** |
| | (1.76) | (1.66) | (-13.12) | (-13.10) |
| $ACCM_t$ | 0.123 | 0.053 | 0.085 | 0.037 |
| | (1.58) | (1.57) | (1.08) | (1.07) |
| *Constant* | -0.633*** | -0.312*** | -0.996*** | -0.518*** |
| | (-14.57) | (-15.34) | (-15.76) | (-18.20) |
| Year FE | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | No | No |
| Firm FE | No | No | Yes | Yes |
| No. of obs. | 43,915 | 43,915 | 43,915 | 43,915 |
| Adj. $R^2$ | 0.041 | 0.047 | 0.095 | 0.094 |

## Table 3. Pre-treatment Trends Analysis

This table presents the dynamic estimation results of the effect of anti-recharacterization (AR) laws on stock price crash risk. In Columns 1 and 2, we replace the AR laws indicator (*ARL*) with four indicators in the baseline model, corresponding to four time periods around each adoption of AR laws: more than 2 years before law adoption, the 2 years preceding law adoption, the 2 years following law adoption, and more than 2 years after law adoption. In Columns 3 and 4, the four time periods are more than 4 years before/4 years preceding/4 years following/more than 4 years after law adoption. The year of the law adoption is taken as the reference year. See Appendix A for variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ |
| *Years 2+ before ARL* | -0.005 | -0.007 | | |
| | (-0.09) | (-0.30) | | |
| *Years 1-2 before ARL* | 0.001 | -0.006 | | |
| | (0.03) | (-0.37) | | |
| *Years 1-2 after ARL* | -0.078** | -0.047** | | |
| | (-2.47) | (-2.62) | | |
| *Years 2+ after ARL* | -0.055* | -0.036** | | |
| | (-1.98) | (-2.35) | | |
| *Years 4+ before ARL* | | | -0.042 | -0.031 |
| | | | (-0.53) | (-0.92) |
| *Years 1-4 before ARL* | | | 0.007 | -0.001 |
| | | | (0.24) | (-0.08) |
| *Years 1-4 after ARL* | | | -0.056** | -0.040*** |
| | | | (-2.31) | (-2.74) |
| *Years 4+ after ARL* | | | -0.073** | -0.038** |
| | | | (-2.38) | (-2.35) |
| Controls | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| No. of obs. | 43915 | 43915 | 43915 | 43915 |
| Adj. $R^2$ | 0.087 | 0.088 | 0.043 | 0.049 |

## Table 4. Propensity Score Matching Analysis

This table reports the results of the one-to-one propensity score matching (PSM). We match control and treatment firms on all control variables in our baseline model, as well as industry, state, and year without replacement. Panel A reports presents diagnostic statistics for the difference in firm characteristics between the treatment and control groups. Panel B reports the regression results based on the propensity-score-matched sample. See Appendix A for other variable definitions. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

*Panel A. Diagnostics stat-difference in means of variables*

| | Treatment group | | Control group | | |
|---|---|---|---|---|---|
| Variables | N | Mean | N | Mean | T-STAT |
| *DTURN* | 3671 | 0.138 | 3671 | 0.116 | 1.03 |
| *SIGMA* | 3671 | 0.062 | 3671 | 0.062 | 0.4 |
| *RET* | 3671 | -0.239 | 3671 | -0.239 | -0.02 |
| *SIZE* | 3671 | 6.196 | 3671 | 6.192 | 0.07 |
| *MB* | 3671 | 2.535 | 3671 | 2.560 | -0.42 |
| *LEV* | 3671 | 0.234 | 3671 | 0.233 | 0.11 |
| *ROA* | 3671 | 0.011 | 3671 | 0.008 | 1.04 |
| *NCSKEW* | 3671 | -0.024 | 3671 | -0.032 | 0.48 |
| *ACCM* | 3671 | 0.076 | 3671 | 0.076 | 0.27 |

*Panel B. Regression with the propensity-score-matched samples*

| | (1) | (2) |
|---|---|---|
| | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ |
| $LAW_t$ | -0.073*** | -0.042*** |
| | (-3.44) | (-4.23) |
| Controls | Yes | Yes |
| Year FE | Yes | Yes |
| State FE | Yes | Yes |
| No. of obs. | 7342 | 7342 |
| Adj. $R^2$ | 0.030 | 0.037 |

## Table 5. Robustness Checks

This table presents the regression results of robustness tests. Panel A presents the results for alternative sample periods and standard error clustering schemes. Panel B presents the logit regression results in which crash risk is proxied by an indicator variable (*CRASH*) that takes one if a firm experiences more than one price crash week in a fiscal year, and the number of crash weeks in a fiscal year (*COUNT*). We present the marginal effects in square brackets. To save space, all the control variables are suppressed. See Appendix A for other variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

*Panel A. Alternative sample period and standard error clustering schemes*

| | Excluding the post-2007 period | | Clustering standard errors by firm | | Clustering standard errors by year | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ |
| $ARL_t$ | -0.053*** | -0.023*** | -0.053** | -0.024** | -0.053*** | -0.024** |
| | (-3.78) | (-2.98) | (-2.41) | (-2.21) | (-3.53) | (-2.66) |
| State FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| No. of obs. | 41,697 | 41,697 | 43,915 | 43,915 | 43,915 | 43,915 |
| Adj. $R^2$ | 0.042 | 0.048 | 0.041 | 0.047 | 0.041 | 0.047 |

*Panel B. Alternative measures of crash risk*

| | (1) | (2) |
|---|---|---|
| | $CRASH_{t+1}$ | $COUNT_{t+1}$ |
| $ARL_t$ | -0.200*** | -0.044** |
| | (-2.91) | (-2.35) |
| Marginal Effect | [-0.028] | |
| Year FE | Yes | Yes |
| State FE | Yes | Yes |
| No. of obs. | 79,228 | 79,153 |
| Adj./Pseudo $R^2$ | 0.029 | 0.029 |

## Table 6. Timing of anti-recharacterization laws: The duration model

This table reports results from the Weibull proportional hazard model where the "failure event" is the adoption of anti-recharacterization (AR) laws in a U.S. state. The dependent variable is the log of the expected time to adopt AR laws. The sample period is 1994 to 2008. All explanatory variables included are measured at the state level. States are dropped from the sample once they pass the AR laws. See Appendix A for variable definitions. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

| Dependent variable: the log of the expected time to adopt AR laws | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *NCSKEW* | -0.002 | -0.001 | | |
| | (-0.62) | (-0.28) | | |
| *DUVOL* | | | -0.004 | -0.002 |
| | | | (-0.40) | (-0.16) |
| *GDP Growth* | | 0.031 | | 0.030 |
| | | (0.29) | | (0.29) |
| *ln(GDP)* | | -0.006 | | -0.006 |
| | | (-0.52) | | (-0.51) |
| *political balance* | | 0.014 | | 0.014 |
| | | (1.30) | | (1.26) |
| *unemployment rate* | | 0.001 | | 0.001 |
| | | (0.92) | | (0.95) |
| *ln(population)* | | 0.005 | | 0.005 |
| | | (0.41) | | (0.40) |
| No. of obs | 666 | 666 | 666 | 666 |

## Table 7. Bad news hoarding mechanisms

This table presents the regression results of the effect of anti-recharacterization laws on bad news revelation and residual short interest. Following DeFond and Zhang (2014), we use stock market response to negative earnings surprise (*N_SUE*) during the window (-10, 1), i.e., Stock CAR (-10, 1), to capture bad news revelation in the stock market. Following Bao et al. (2019), we define residual short interest (*ResSI*) as the residual estimated from the regression of quarterly raw short interest on institutional ownership, convertible bonds indicator and calendar quarterly fixed effects. Other variable definitions are presented in Appendix A. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

| Panel A. Bad news revelation | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Stock CAR (-10, 1) | Stock CAR (-10, 1) | Stock CAR (-10, 1) |
| $N\_SUE_t \times ARL_t$ | 0.003*** | 0.003*** | 0.003*** |
| | (3.06) | (2.94) | (2.80) |
| $N\_SUE_t$ | 0.005*** | 0.005*** | 0.004*** |
| | (17.13) | (16.70) | (12.92) |
| $ARL_t$ | 0.003 | 0.003 | 0.002 |
| | (0.98) | (0.99) | (0.73) |
| Year-quarter FE | No | Yes | Yes |
| Year FE | Yes | No | No |
| State FE | Yes | Yes | No |
| Industry FE | Yes | Yes | No |
| Firm FE | No | No | Yes |
| No. of obs. | 55,181 | 55,181 | 55,181 |
| Adj. $R^2$ | 0.035 | 0.042 | 0.025 |

| Panel B. Residual short interest | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | ResSI | ResSI | ResSI | ResSI |
| $ARL_t$ | -0.009*** | -0.008** | -0.005*** | -0.005*** |
| | (-3.73) | (-2.92) | (-12.57) | (-6.26) |
| $SIZE_t$ | | -0.004** | | -0.002 |
| | | (-2.53) | | (-0.92) |
| $MB_t$ | | 0.001* | | -0.001 |
| | | (2.24) | | (-1.92) |
| $LEV_t$ | | 0.021*** | | 0.030*** |
| | | (4.87) | | (5.48) |
| $ROA_t$ | | -0.020 | | -0.016*** |
| | | (-1.54) | | (-6.00) |
| $insown\_q_t$ | | 0.034** | | 0.036*** |
| | | (2.98) | | (3.92) |
| Calendar quarter FE | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | No | No |
| Firm FE | No | No | Yes | Yes |
| No. of obs. | 12,389 | 12,389 | 12,380 | 12,380 |
| Adj. $R^2$ | 0.078 | 0.139 | 0.517 | 0.540 |

## Table 8. Credit dependence and financial constraints

This table presents the results conditional on credit dependence. We set two dummy variables that are equal to one for firm-years that have above-median loan ratio and HP index, respectively (*Loan, HPINDEX*). See Appendix A for other variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. $^{***}$, $^{**}$, and $^{*}$ denote statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ |
| ARL | -0.005 | -0.009 | -0.035* | -0.015 |
| | (-0.22) | (-0.48) | (-1.87) | (-1.38) |
| Loan | 0.017 | 0.004 | | |
| | (1.36) | (0.67) | | |
| ARL × Loan | -0.056* | -0.019** | | |
| | (-1.74) | (-2.38) | | |
| HPINDEX | | | -0.026** | -0.003 |
| | | | (-2.61) | (-0.57) |
| ARL × HPINDEX | | | -0.035** | -0.025** |
| | | | (-2.14) | (-2.20) |
| Controls | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| No. of obs. | 14572 | 14572 | 43915 | 43915 |
| Adj. R2 | 0.021 | 0.014 | 0.041 | 0.016 |

## Table 9. Pledgeability and innovation

This table presents the results conditional on asset tangibility and patent collateral. In Columns 1 and 2, we set the dummy variable that is equal to one for firm-years that have below-median tangibility ratios (*LOW_TANGIBLE*), and zero otherwise. In Columns 3 to 6, We set two dummy variables that are equal to one for firm-years that have below-the-sample-median patents counts, and citation-weighted patents counts (*LOW_ CPAT*, and *LOW_CWPAT*), respectively, and equal to zero otherwise. See Appendix A for other variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ |
| ARL | -0.016 | -0.004 | -0.010 | -0.004 | -0.021 | -0.012 |
| | (-1.01) | (-0.45) | (-0.18) | (-0.18) | (-0.40) | (-0.54) |
| LOW_TANGIBLE | 0.040*** | 0.016*** | | | | |
| | (4.53) | (3.46) | | | | |
| ARL × LOW_TANGIBLE | -0.076*** | -0.040*** | | | | |
| | (-5.27) | (-4.80) | | | | |
| LOW_CPAT | | | -0.009 | -0.002 | | |
| | | | (-0.71) | (-0.38) | | |
| ALR × LOW_ CPAT | | | -0.058*** | -0.037*** | | |
| | | | (-3.73) | (-4.26) | | |
| LOW_CWPAT | | | | | 0.017 | 0.012** |
| | | | | | (1.38) | (2.24) |
| ARL × LOW_CWPAT | | | | | -0.035* | -0.020** |
| | | | | | (-1.77) | (-2.11) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes | Yes | Yes |
| No. of obs. | 43915 | 43915 | 13796 | 13796 | 13796 | 13796 |
| Adj. R2 | 0.041 | 0.047 | 0.039 | 0.048 | 0.039 | 0.048 |

## Table 10. Governance mechanisms

This table presents the results conditional on institutional ownership and analysts coverage. We set two dummy variables that are equal to one for below-median institutional ownership and analyst coverage, respectively (*LOW_INSOWN* and *LOW_COVER*), and zero otherwise. See Appendix A for other variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ | $NCSKEW_{t+1}$ | $DUVOL_{t+1}$ |
| ARL | -0.009 | -0.006 | -0.037** | -0.016* |
| | (-0.18) | (-0.37) | (-2.47) | (-1.94) |
| LOW_INSOWN | 0.069*** | 0.032*** | | |
| | (3.96) | (3.80) | | |
| ARL × LOW_INSOWN | -0.079** | -0.033** | | |
| | (-2.26) | (-2.16) | | |
| LOW_COVER | | | 0.039*** | 0.020*** |
| | | | (3.14) | (3.57) |
| ARL × LOW_COVER | | | -0.052*** | -0.024*** |
| | | | (-3.24) | (-3.12) |
| Controls | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| No. of obs. | 12193 | 12193 | 43915 | 43915 |
| Adj. R2 | 0.021 | 0.024 | 0.041 | 0.047 |

**Appendix A. Variable definition**

*NCSKEW* is the negative skewness of firm-specific weekly returns over the fiscal year.

*DUVOL* is the log of the ratio of the standard deviations of down-week to up-week firm-specific weekly returns.

*ARL* is an indicator variable that equals one equals one for firms incorporated in Texas or Louisiana after 1997, in Alabama after 2001, in Delaware after 2002, in South Dakota after 2003, in Virginia after 2004, and in Nevada after 2005.

*SIGMA* is defined as the standard deviation of firm-specific weekly returns over fiscal year *t*.

*RET* is calculated as the average firm-specific weekly returns over fiscal year *t.*

*SIZE* is calculated as the log of market value of equity at the end of fiscal year *t*.

*MB* is market-to-book ratio measured as the market value of equity divided by the book value of equity at the end of fiscal year *t*.

*LEV* is financial leverage calculated as the book value of total debt scaled by total assets at the end of fiscal year *t*.

*ROA* is return on assets defined as income before extraordinary items divided by total assets at the end of fiscal year *t*.

*ACCM* is the absolute value of discretionary accruals, where discretionary accruals are estimated by the modified Jones' (1991) model.

*GDP Growth* is measured as state-level GDP percent change (source: Bureau of Economic Analysis).

*Political balance* is measured as state-level fraction of the members of the House of Representatives from the Democratic Party in the current year.

*Unemployment rate* is measured as state-level unemployment rate.

*ln(GDP)* is the logarithm of state-level GDP.

*ln(population)* is the logarithm of the state-level population.

*CRASH* a dummy variable that takes the value of 1 when a firm experiences at least one crash week during the fiscal year, and zero otherwise. Crash weeks are considered as those weeks during which the firm-specific weekly returns are 3.09 standard deviations *below* the mean firm-specific weekly returns over the fiscal year.

*COUNT* is the number of crash weeks minus the number of jump weeks over the fiscal year. Jump weeks are those weeks during which the firm-specific weekly returns are 3.09 standard deviations *above* the mean firm-specific weekly returns over the fiscal year.

*SUE* is quarterly standardized earnings surprises and *N_SUE* is negative earnings surprises.

*Stock CAR (-10, 1)* is stock market response to negative earnings surprise (*N_SUE*) during the window (-10, 1).

*ResSI* is residual short interest estimated from the following model regression:

$$SI_{j,q} = \alpha + \beta_1 IO_{j,q} + \beta_2 CONVERT_{j,q} + Quarter\ fixed\ effects + \varepsilon_{j,q}$$

where *q* represents the quarter. *SI* is raw short interest scaled by shares outstanding at the quarter end. *IO* is institutional ownership at the quarter end. *CONVERT* is an indicator equal to one if the firm has convertible bonds or preferred stocks outstanding in the most recent fiscal year ending prior to the end of quarter, and 0 otherwise.

*Loan* is an indicator equal to one if the firm has above-median loan ratio, measured as the amount of cumulative bank loan scaled by the total assets in year *t* (source: DealScan).

*HPINDEX* is an indicator equal to one if the firm has above-median Hadlock-Pierce financial constraint index, which is developed by Hadlock and Pierce (2010), computed as $(-0.737\times SIZE) + (0.043\times SIZE^2) - (0.040\times AGE)$, where *SIZE* is the log of inflation-adjusted book assets and *AGE* is firm age.

*LOW_TANGIBLE* is an indicator equal to one if the firm has below-median ratio of total tangible assets (Compustat item ppent) to book value of total assets.

*LOW_CPAT* is an indicator equal to one if the firm has below-median paten counts. *CPAT* is measured as the natural logarithm of one plus total number of patents filed. Data source: Kogan et al. (2017).

*LOW_CWPAT* is an indicator equal to one if the firm has below-median citation-weighted patent counts (*CWPAT*). The weight is computed as one plus the number of citations scaled by the average number of cites to patents issued in the year *t*. Data source: Kogan et al. (2017).

*LOW_INSOWN* is an indicator equal to one for firms with below-median institutional ownership, and zero otherwise. Institutional ownership is the percentage of shares owned by institutional investors.

*LOW_COVER* is an indicator equal to one for firms with below-median analysts coverage, and zero otherwise.