Director-Liability-Reduction Laws and Conditional Conservatism

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ABSTRACT

We study non-officer directors' influence on the accounting conservatism of U.S. public firms. Between 1986 and 2002, all 50 U.S. states enacted laws that did not change officer-directors' litigation risk but limited non-officer-directors' litigation risk. We find decreases in conditional conservatism around the staggered law enactments, which we attribute to less non-officer director monitoring of financial reporting in affected firms. Conservatism fell less when shareholder or debtholder power were high, consistent with major stakeholders moderating the influence of non-officer directors. Affected firms switched away from Big N auditors more often, which reduced these firms' commitment to conservative financial reports. The conservatism reductions were concentrated in current assets and did not affect liabilities. The results are robust to many specification checks. Our results are consistent with non-officer directors monitoring and influencing the financial reporting process and have implications for corporate governance and corporate law reforms.

Keywords: litigation risk; corporate governance; D&O insurance; non-officer directors; board monitoring

JEL codes: G1; G14; G32; G34; K22; M41

1. Introduction

We study how non-officer directors influence the accounting conservatism of U.S. public firms. When directors do not act in shareholders' interests, shareholders can sue directors for breach of duty. Between 1986 and 2002, all 50 U.S. states enacted laws which did not change officer directors' risk but either limited or let shareholders limit non-officer directors' litigation risk (Romano [2006]). Conditional conservatism is associated with less agency costs (Watts [2003]) and lower litigation costs (Ettredge, Huang and Zhang [2016]). We argue that these laws reduced directors' incentive to monitor financial reporting, and hence, let managers reduce the conditional conservatism of affected firms' earnings. We also explore how the impact of director-liability-reduction laws varied cross-sectionally with shareholder power and debt contracting, which are important sources of demand for conditional conservatism.

Non-officer directors monitor and advise managers on behalf of shareholders (Brook and Rao [1994]). Different from the officer directors who manage the firm, non-officer directors are usually outsiders who are expected to oversee the firm's decision-making processes including financial reporting (Larcker and Tayan [2015]).[1] Non-officer directors (including directors not serving on the audit committee) were named as defendants in several shareholder class-action lawsuits under Section 11 of the Securities Act of 1933 and a few paid large sums out of their own pockets in major frauds (Black, Cheffins, and Klausner [2006]).[2] Srinivasan [2005] reports that when firms restate financial statements, their outside directors, and especially audit committee members, often lose their board seats on both the restating firm and other firms.

[1] A non-officer director is defined as a director who is not an officer (i.e., an executive) of the firm. In the U.S., non-officer directors are almost always outside directors unless they are family members of the founder. However, in other countries such as Germany, employee representatives are also important non-officer directors (Fauver and Fuerst [2006]). Because our study examines U.S. data, our findings are most relevant to outside directors. Conversely, officer directors are a subset of inside directors, who also include non-officer employees and direct stakeholders.

[2] Based on settlement agreements that were publicly available in 2005, Black, Cheffins and Klausner ([2006], Table 2) report that 12 outside directors at WorldCom paid a total of $24.75 million out of pocket, while 11 Enron outside directors paid out a total of $14.5 million. Most shareholder lawsuits are settled privately, and even when the settlement amounts are disclosed, the ultimate payers are often not specified (Black, Cheffins, and Klausner [2006], p. 1062), making it impossible to compile comprehensive data on outside director payments out of pocket.

We find that conditional conservatism decreases after the director-liability-reduction laws were enacted, which is consistent with non-officer directors being important monitors of their firms' financial reporting. When accounting is conservative, bad news about future cash flows is reflected in earnings more quickly than good news (Basu [1997]). Conditional conservatism helps to constrain managerial opportunism and mitigate agency costs, which falls under non-officer directors' monitoring responsibility (Watts [2003]; Mora and Walker [2015]; Ruch and Taylor [2015]). Because different states enacted director-liability-reduction laws in different years (i.e., in a staggered manner), firms incorporated in different states serve as controls for each other.

We predict and find that in firms with high shareholder power or high debt-contracting demand, conditional conservatism decreases less after these laws are enacted. In other words, we find that major stakeholders, who demand conservative financial reports and who can influence managers' behavior directly and/or indirectly by pressuring directors, ensure that conditional conservatism decreases less after non-officer directors' litigation risk is reduced.

We validate our results by examining the asymmetric timeliness of accruals. We first follow Collins, Hribar, and Tian [2014] and separately model operating accruals and cash flow from operations (CFO). We find that our results are concentrated in accruals rather than CFO. We next compare changes in current assets and changes in current operating liabilities. We find that the change in the asymmetric timeliness is concentrated in current assets, consistent with Ijiri and Nakano [1989], who emphasize that there is not a higher-or-cost-of-market rule for liabilities. Following Byzalov and Basu [2016], we show that accruals respond less asymmetrically after the law enactments to bad news indicators other than stock returns. We also find that firms are less likely to choose a Big N auditor after the laws are enacted, which is one way to reduce their commitment to conservative accounting.

Our results are robust to several sensitivity tests. We find that director-liability-reduction laws are associated with less conditional conservatism after we control for state antitakeover laws. We confirm that our results are not likely driven by either endogenous law enactments or a

violation of the parallel trends assumption. In Internet Appendix A, we also verify that our results are not changed qualitatively when we use alternative sample selection criteria or include additional control variables.

Our paper contributes in several ways. First, we provide insights into the role of the non-officer directors by showing that decreasing their litigation risk affects financial reporting and, in particular, reduces conditional conservatism. Unlike prior research that studied directors' and officers' litigation risk together using Director and Officer (D&O) insurance coverage, we isolate the role of non-officer directors and use law changes to alleviate the concern of endogenous D&O insurance procurement (Hermalin and Weisbach [1998]; Ball [2008]). Second, our findings help in evaluating corporate governance reforms and corporate law reforms that involve the board of directors, and specifically non-officer directors, such as the Sarbanes-Oxley Act (SOX) of 2002. Third, we contribute to the debate on the net impact of director-liability-reduction laws. Prior research has documented both positive and negative effects for shareholders, and our paper identifies an additional impact of the director-liability-reduction laws. Finally, we extend recent research that uses staggered law changes to infer causal links between conditional conservatism and other variables such as law enforcement (Jayaraman [2012]), managers' agency problem (Jayaraman and Shivakumar [2013]; Manchiraju, Pandey, and Subramanyam [2017]; Chen, Li, and Xu [2018]), and debtholders' demand (Aier, Chen, and Pevzner [2014]).

2. Background and Hypotheses

2.1 NON-OFFICER DIRECTORS AND CONDITIONAL CONSERVATISM

Non-officer directors are important monitors of firms' financial reporting processes (Larcker and Tayan [2015]). To strengthen non-officer directors' incentives to monitor firms' financial reporting practices, regulators and stock exchanges enacted corporate governance reforms after the accounting scandals in the late 1990s and early 2000s. For example, Section 301

of SOX requires that each member of the audit committee of a public firm be independent.[3] NYSE and NASDAQ also required that a majority of directors be independent for all their listed firms.[4]

Using extreme cases like financial statement frauds and Securities and Exchange Commission (SEC) enforcement actions as proxies for poor financial reporting quality, Beasley [1996] and Dechow, Sloan, and Sweeney [1996] find that firms with larger proportions of outside directors have better financial reporting quality. Firms with more outside directors are more likely to issue earnings forecasts and these management forecasts are more accurate on average (Ajinkya, Bhojraj, and Sengupta [2005]). Last, but most relevant to our study, board independence is positively associated with accounting conservatism (Beekes, Pope, and Young [2004]; Ahmed and Duellman [2007]). [5]

Directors serving on the audit committee directly monitor financial statements. Audit committee independence and the presence of financial experts are associated with less earnings management (Klein [2002]; Xie, Davidson, and Dadalt [2003]; Bedard, Chtourou, and Courteau [2004]), fewer internal control problems (Krishnan [2005]; Zhang, Zhou, and Zhou [2007]), and fewer restatements (Agrawal and Chadha [2005]). Audit committees with more accounting financial experts—defined by the SEC as individuals with experience as a certified public accountant, auditor, chief financial officer, controller, or chief accounting officer—are associated with greater conditional conservatism (Krishnan and Visvanathan [2008]), and higher stock prices when their appointments are announced (DeFond, Hann, and Hu [2005]). [6]

[3] SOX Section 301 defines independent directors as board members who do not accept any consulting, advisory, or other compensatory fees from the firm and are not affiliated with the firm or its subsidiaries. Thus, independent directors as defined under SOX are always non-officers.

[4] NYSE and NASDAQ define independent directors in substantially more detail than SOX does. However, they also do not consider firm officers to be independent.

[5] Enache and Garcia-Meca (2018) find that politician directors are negatively associated with conservatism in their U.S. biotech sample, which suggests that independent directors vary in their influence on conservatism.

[6] SOX requires public firms to disclose whether their audit committees include financial experts or not. The SEC initially defined financial experts narrowly as individuals with experience as a certified public accountant, auditor, chief financial officer, controller, or chief accounting officer (SEC [2002]). The SEC later defined financial experts more broadly to include non-accounting financial experts such as individuals with experience as a chief executive officer or president (SEC [2003]). Krishnan and Visvanathan [2008] and DeFond, Hann, and Hu [2005] defined accounting financial experts following the initial narrow definition.

Prior research argues that directors monitor financial reporting on behalf of shareholders to increase conditional conservatism (e.g. Watts [2003]). LaFond and Roychowdhury [2008] argue that managers with a shorter horizon than shareholders prefer aggressive accounting to receive higher bonuses that are difficult to recover. Ball [2001] argues that conditional conservatism forces managers to recognize losses when projects have bad outcomes, which reduces the overinvestment problem. Srivastava, Sunder, and Tse [2015] find that conservative accounting makes bad news about projects reduce current-period income, inducing managers to abandon poor projects quickly. Chen, Hemmer, and Zhang [2007] find that, in a setting where shareholders intend to sell the firm to a buyer, a conservative accounting system lessens managers' incentive to overstate earnings, reducing excessive bonus compensation and increasing shareholder welfare. Gao [2013] reaches a similar conclusion in a corporate financing setting.

Because officer directors include firm managers who prepare the financial statements, non-officer directors are introduced to act on the shareholders' behalf and ensure the supply of conservative accounting reports (Ahmed and Duellman [2007]). Although prior research establishes that non-officer directors are associated with higher conservatism, our study of changes in director-liability-reduction laws lets us draw stronger causal inferences and helps us better understand the non-officer directors' monitoring role in financial reporting.

2.2 DIRECTOR DUTY, D&O INSURANCE, AND DIRECTOR LIABILITY LAWS

Directors of U.S. corporations have two fiduciary duties: loyalty and care (Butler [2012]). The duty of loyalty requires fairness and/or disclosure when directors undertake self-interested transactions.[7] The duty of care requires a director to take or refrain from board action with care, diligence, and exercise of reasonable skill. Because directors of public firms oversee financial reporting, the duty of care and its standard influence directors' monitoring efforts, and hence, affect

[7] Statement of Financial Accounting Standards (SFAS) No. 57 (Financial Accounting Standards Board [1982]), now Accounting Standards Codification (ASC) 850 and Statement of Auditing Standards (SAS) No. 6 (American Institute of Certified Public Accountants [1975]), now Auditing Standard 2410 (Public Company Accounting Oversight Board [2015]), govern financial statement disclosures regarding related-party transactions including those between firms and directors.

the reported financial statements (Larcker and Tayan [2015]). Historically, U.S. corporate laws applied a negligence standard for duty of care (Romano [2006]), under which directors need not acquire all data on a firm's activities, but they must properly inform themselves before making decisions for the firm (Miller [2010]).

When directors breach their fiduciary duties, they are personally liable to shareholders. In such cases, shareholders can sue the directors on behalf of the firm for the harm done to it, which is called a derivative suit (Bourveau, Lou, and Wang [2018]). Such lawsuits hurt the sued directors' reputations and finances because firms cannot reimburse the payments of a derivative suit in most states and indemnification is limited to legal expenses (Eisenberg and Miller [2010]). To lessen directors' concern regarding their potential monetary losses, firms can purchase D&O insurance to cover the portion of the losses that cannot be indemnified.

The market for D&O insurance experienced a crisis near the end of 1984 (Baker and Griffith [2007]). The average D&O insurance premium increased more than fivefold during 1985, even while the coverage decreased on average (Wyatt Company [1988]). The *Smith v. Van Gorkom* (a.k.a. *TransUnion*) case was likely the largest contributor to the crisis (Romano [2006]). On January 29, 1985, the Delaware Supreme Court ruled that the TransUnion Corporation directors had violated their duty of care because they accepted a merger proposal after deliberating for only two hours and without informing themselves sufficiently about the firm value (Fischel [2002]). The *TransUnion* case shocked observers because the merger offer was at a large premium over TransUnion's stock price (Elson and Gyves [2004]). However, the court disregarded the outcome and focused on the decision-making process, which changed the corporate community's understanding of the duty of care and magnified the D&O insurance crisis (Romano [2006]).

In response to the D&O insurance crisis, by 2003 all 50 U.S. states had modified their corporate laws to limit non-officer directors' legal liability (Romano [2006]), but corporate officers' legal liability was usually left unchanged. Thus, the laws appear tailored to enable firms to attract and retain outside directors (Baker and Griffith [2007]). The states limited director liability using

three approaches (Romano [2006]). The relaxation-of-culpability-standard approach lowers the standard of duty of care for all firms incorporated in the state from gross negligence to, for example, willful misconduct or recklessness. In contrast, the limited-liability charter-amendment approach allows but does not require shareholders to modify the articles of incorporation to limit or eliminate directors' liability. Bradley and Schipani [1989] find that 94 percent of their sample of Delaware firms adopted the limited-liability charter provisions. Thus, the two approaches differed little in practice in limiting non-officer directors' liability. Six states relaxed culpability standards, while more than 40 states adopted limited-liability charter amendments. In Virginia, the only state to adopt the damage caps approach, the individual directors' monetary losses are capped at the greater of $100,000 and the directors' annual pay just before the misconduct.

The enactment of the director-liability-reduction laws affected firms in several ways. First, the rate of increase in D&O insurance premia fell after 1986 (Wyatt Company [1988]). The slowdown led to negative cumulative abnormal returns (CARs) for firms that wrote D&O insurance (Bradley and Schipani [1989]). Second, the laws also reduced the covered firms' market values. Delaware firms had negative CARs on the day of the law enactment (Bradley and Schipani [1989]; Romano [1990]) and also suffered negative CARs around the day they announced their adoption of the charter provisions (Bradley and Schipani [1989]).[8] Khan and Wald [2015] find that the enactment of director-liability-reduction laws increased accrual-based earnings management and audit fees. Overall, existing evidence suggests that, while director-liability-reduction laws helped to resolve the D&O insurance crisis, they also reduced firm value, likely reflecting the expected net costs of less outside-director monitoring.[9]

[8] Inconsistent with Bradley and Schipani [1989], Romano [1990] finds that the adoption announcement CARs are insignificant in her sample.

[9] Brook and Rao [1994] find positive stock price reactions to the adoption of limited liability provisions for poorly performing firms. However, because only 16 (out of 120) firms were classified as poorly performing firms, their results may not generalize to most other firms.

2.3 DIRECTOR LITIGATION RISK AND CONDITIONAL CONSERVATISM

Litigation risk significantly affects decisions and economic outcomes (e.g., Cheng, Huang, Li, and Lobo [2010], Hopkins [2017]; Levy, Shalev, and Zur [2018]). Basu [1997] argues and finds that auditor litigation risk is an important determinant of U.S. accounting conservatism, which is consistent with litigation costs being higher when earnings are overstated (Kellogg [1984]; St. Pierre and Anderson [1984]).[10] Prior studies use time periods, legal systems of different countries, cross-listing status of firms, and strength of legal enforcement to explore how litigation risk affects conditional conservatism (e.g., Ball, Kothari, and Robin [2000]; Holthausen and Watts [2001]; Lang, Raedy, and Yetman [2003]; Huijgen and Lubberink [2005]; Bushman and Piotroski [2006]; Jayaraman [2012]). The litigation risks of stakeholders such as auditors and directors are decreasing in conditional conservatism (e.g., Qiang [2007]; Chung and Wynn [2008]), as is the litigation risk to the firm itself (Ettredge, Huang, and Zhang [2016]).

We argue that a reduction in directors' litigation risk can reduce non-officer-directors' monitoring incentives, and hence, influence conditional conservatism. We build on Watts and Zimmerman [1990], who argue that contracting parties, which include non-officer directors and managers, select firms' "accepted sets" of accounting policies *ex-ante* to contain managerial opportunism *ex-post*, and that such accounting policies can be conservative. Because non-officer directors oversee both *ex-ante* policy sets and *ex-post* estimates and judgements, below we discuss how decreases in directors' monitoring of *ex-ante* and *ex-post* choices can change conservatism.

When the accounting policies are being selected *ex-ante*, a lower litigation risk will likely reduce non-officer directors' incentives to monitor and seek more conservative accounting policies. This is because, after the law change, non-officer directors are less likely to lose lawsuits, pay out money from their own pockets, or damage their reputation and labor market value (Srinivasan

[10] Basu, Hwang and Jan [2002] report that conservatism peaked in 1985, which matches the spike of the D&O insurance crisis, although they attribute the jump to the Rosenblum decision [*Rosenblum v. Adler*, 444 A. 2d 66 (N.J. 1982), 461 A. 2d 138 (N.J. 1983)] which let foreseeable parties sue auditors; and the Schact decision [*Schact v. Brown*, 711 F.2d (CA-7, 1983)] which applied the Racketeer Influenced and Corrupt Organizations Act (RICO) to auditors, and tripled damages.

[2005]; Fich and Shivdasani [2007]) while monitoring is still personally costly (Adams and Ferreira [2007]; Laux [2008]; Taylor [2010]). That is, while directors' personal costs of monitoring do not change after the law enactments, some benefits of monitoring (i.e., less litigation risk) fall. Hence, non-officer directors, especially those who sit on audit committees, are less likely to propose a stricter verifiability standard for good news than bad news and/or to choose a high-quality auditor to ensure conservative reporting. Therefore, in equilibrium, conditional conservatism will decrease after the director-liability-reduction laws are enacted. Internet Appendix B demonstrates this intuition more formally.

In contrast to containing managerial opportunism through monitoring the accounting policy selection *ex-ante*, non-officer directors can also reduce managerial opportunism by monitoring managers *ex-post*. For example, monitoring by independent directors may deter managers' empire building (Chen, Lu, and Sougiannis [2012]). Because both conservative accounting policies selected *ex-ante* and non-officer directors' monitoring *ex-post* of accounting estimates and other operating decisions can decrease managerial opportunism, they can be substitutes. Thus, if other contracting parties such as debtholders expect directors to reduce their monitoring efforts *ex-post*, they will demand more conditional conservatism *ex-ante* (Jayaraman and Shivakumar [2013]). If firms do not meet this demand, debtholders will raise interest rates to price-protect themselves. More conservative accounting policies may be selected *ex-ante* to avoid higher interest rates, and the equilibrium conservatism level may increase rather than decrease.

Finally, the lower litigation risk may increase the supply of non-officer directors (Baker and Griffith [2007]), especially bad or weak directors who do not monitor managers effectively and likely would have withheld their services when litigation risk was high. Thus, board monitoring could also weaken if ineffective non-officer directors are recruited after these law enactments. However, we acknowledge that the weak directors may not be selected because the talented and/or more risk-averse directors, both of whom could be better monitors, would still participate in the director labor market when litigation risk is lower.

Summing up, theory suggests that conditional conservatism will decrease with non-officer directors' monitoring *ex-ante* and will increase with their monitoring *ex-post*. We state our first hypothesis in the following alternative form:

H1: Conditional conservatism decreases after director-liability-reduction laws are enacted.

2.4 INTERACTION OF DIRECTOR LIABILITY LAWS AND OTHER DETERMINANTS OF CONDITIONAL CONSERVATISM

Bushman and Piotroski [2006] find that the presence of large shareholders is positively associated with conditional conservatism because they monitor the financial reporting process. Institutions are often powerful shareholders of U.S. public firms, and Ramalingegowda and Yu [2012] find that high-monitoring institutional ownership increases conservatism.[11] Institutional investors can influence conservatism by putting pressure on directors who do not monitor diligently, or by directly pressuring managers. However, when shareholder power is weak (or managerial power is strong), shareholders' demand for conservatism will not be satisfied when directors stop monitoring closely. Therefore, we predict that the director-liability-reduction laws reduce (increase) conditional conservatism less (more) for firms with high shareholder power (Jayaraman and Shivakumar [2013]). Thus, our second hypothesis stated in alternative form is:

H2: Enactment of director-liability-reduction laws is associated with less of a decrease (or more of an increase) in conditional conservatism in firms with higher shareholder power.

Prior studies show theoretically and empirically that debt-contracting demand is a major driver of conditional conservatism (Ball and Shivakumar [2005]; Zhang [2008]; Gigler, Kanodia, Sapra, and Venugopalan [2009]; Göx and Wagenhofer [2009]; Nikolaev [2010]; Li [2013]).[12] Jayaraman and Shivakumar [2013] find that, after states passed antitakeover laws that effectively

[11] Cheng, Huang, and Li [2015] find similarly that hedge fund interventions increase accounting conservatism.

[12] Erkens, Subramanyam, and Zhang [2014] find that firms whose lenders have board seats report less conservatively than similar firms without affiliated bankers on their boards. They argue that when lenders have direct monitoring ability and inside access to data, they rely less on general-purpose financial statements for monitoring, reducing their demand for accounting conservatism. Because their data on banker affiliations is from 2000-2006, after most of our sample law enactments, we do not test this moderating influence.

protected managers' jobs, firms with high debt-contracting pressure reported more conservatively to resolve debtholders' concern about their higher agency conflict with managers. Thus, we expect that any negative (positive) effect of the new laws on director monitoring would be alleviated (strengthened) by higher debt-contracting demand.

Thus, our third hypothesis stated in alternative form is:

H3: Enactment of director-liability-reduction laws is associated with less decrease (or more increase) in conditional conservatism in firms with high debt-contracting demand.

3. Research Design

3.1 CONDITIONAL CONSERVATISM MEASURE

Following Basu [1997], we measure conditional conservatism as the asymmetric timeliness of earnings in reflecting news about expected future cash flows. We include firm fixed effects to control for unobservable firm characteristics that are correlated with expected earnings (Ball, Kothari, and Nikolaev [2013]). Consistent with Jayaraman and Shivakumar [2013], we modify the Basu [1997] model by adding firm and year fixed effects as follows:

$$EARN_{i,t} = \alpha_i + \omega_t + \beta_1 RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} \times NEG_{i,t} + \epsilon_{i,t} \quad (1)$$

where *EARN* is the income before extraordinary items scaled by beginning-of-year market value of equity, *RET* is the market-adjusted stock return during the fiscal year, *NEG* is a dummy variable that indicates bad future cash flow news and equals 1 if *RET* < 0 and equals 0 otherwise, and α_i and ω_t represent firm and year fixed effects. Model (1) does not need a separate intercept when fixed effects are added (Christensen, Hail, and Leuz [2013]). All variables are defined in the Appendix. In model (1), conditional conservatism is measured by the slope coefficient β_3, which captures the difference in earnings timeliness between bad and good news. With conservative accounting, earnings reflect bad news faster than good news, and hence, β_3 in regression model (1) should be positive (Basu [1997]).

3.2 CHANGE IN CONDITIONAL CONSERVATISM AFTER DIRECTOR-LIABILITY-REDUCTION LAW ENACTMENTS

Our goal is to study whether the asymmetric timeliness coefficient β_3 changes after the director-liability-reduction laws are enacted. Following Jayaraman and Shivakumar [2013], we interact every term except the fixed effects terms in model (1) with *POST*, an indicator for observations after the law enactment, and estimate the following regression model:

$$EARN_{i,t} = \alpha_i + \omega_t + \beta_1 RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} \times NEG_{i,t} + POST_{i,t} \times \left(\beta_4 + \beta_5 RET_{i,t} + \beta_6 NEG_{i,t} + \beta_7 RET_{i,t} \times NEG_{i,t}\right) + \epsilon_{i,t} \quad (2)$$

Because the different states enacted director liability laws in a staggered fashion, *POST* has different values for firms incorporated in different states in a given year. Thus, firms incorporated in different states can be viewed as control firms for each other, which enables us to separate the director-liability-reduction laws from time trends in conservatism identified previously (e.g., Basu [1997]; Givoly and Hayn [2000]; Holthausen and Watts [2001]; Basu, Hwang, and Jan [2002]; Ryan and Zarowin [2003]; Lobo and Zhou [2006]; Shroff, Venkataraman, and Zhang [2013]). *POST* is interacted with every term of the Basu model so that changes in conditional conservatism (as captured by β_7) and changes in the earnings reaction to cash flow news and the bad news indicator (as captured by β_5 and β_6, respectively) can be separated. To fully disentangle the change in conditional conservatism after the law enactments from the level of conditional conservatism determined by time-invariant state-level variables and the market-wide time trend, we further build a fixed-effects structure into the slope coefficients β_j (j = 1, 2, 3) as below:

$$\beta_j = \sum_K \delta_{j,k} State_k + \sum_M \theta_{j,m} Year_m \quad (3)$$

where *State* and *Year* are state and year indicator variables, respectively.[13] Adding this fixed-effects structure can eliminate alternative explanations such as that states differ in their conservatism levels before the law enactments because similar firms often co-locate (Ellison and

[13] For completeness, we apply the fixed effects structure as in model (3) to all the coefficients of the Basu model. In untabulated tests, we find that our main results are not changed if we use the fixed effects structure for β_3 only.

Glaeser [1997]). As before, we do not include an intercept in equation (3) because it is subsumed by the fixed-effects structure (Christensen, Hail, and Leuz [2013]).

Under H1, conditional conservatism will decrease after the director-liability-reduction laws were enacted. Therefore, β_7 is predicted to be negative. Because *POST* captures a change in non-officer directors' litigation risk that affects non-officer directors' monitoring effort but is not a result of, for example, optimal contracting (Hermalin and Weisbach [1998]), our research context and design have less of an endogeneity problem and can help us draw stronger causal inferences.

3.3 SHAREHOLDER POWER AND THE CHANGE IN CONDITIONAL CONSERVATISM

Next, we study how the change in conditional conservatism differs for firms with different levels of shareholder power. We test whether the coefficient β_7 in model (2) varies systematically between firms with different shareholder power. We consider two shareholder power measures, institutional ownership and G-index. Ramalingegowda and Yu [2012] argue and find that institutional investors are powerful shareholders who induce high conservatism. G-index, in contrast, measures strong managerial power and/or weak shareholder power (Gompers, Ishii, and Metrick [2003]; Cremers and Ferrell [2014]). We interact a shareholder power proxy *SHPOWER* with every term except the fixed effects in model (2) and estimate the following regression model:

$$\begin{aligned} EARN_{i,t} = {} & \alpha_i + \omega_t + \beta_1 RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} \times NEG_{i,t} \\ & + POST_{i,t} \times \left(\beta_4 + \beta_5 RET_{i,t} + \beta_6 NEG_{i,t} + \beta_7 RET_{i,t} \times NEG_{i,t}\right) \\ & + SHPOWER_{i,t} \times \left(\beta_8 + \beta_9 RET_{i,t} + \beta_{10} NEG_{i,t} + \beta_{11} RET_{i,t} \times NEG_{i,t}\right) \\ & + SHPOWER_{i,t} \times POST_{i,t} \\ & \quad \times \left(\beta_{12} + \beta_{13} RET_{i,t} + \beta_{14} NEG_{i,t} + \beta_{15} RET_{i,t} \times NEG_{i,t}\right) + \epsilon_{i,t} \end{aligned} \tag{4}$$

where *SHPOWER* represents either institutional ownership or G-index. We use two measures for institutional ownership, the percentage of outstanding common shares owned by institutional investors and the percentage of outstanding common shares owned by "dedicated" institutional shareholders following Bushee [2001]. We include the second measure of institutional ownership because Bushee [2001] finds that some institutional investors such as quasi-index funds monitor firm decisions less closely, and thus, they likely influence financial reporting less. Because the

original G-index developed by Gompers, Ishii, and Metrick [2003] is available starting only from 1990, we use the G-index data collected by Cremers and Ferrell [2014] for the earlier period. Similar to Cremers and Ferrell [2014], we require no more than 5 missing items (out of the 24 items that comprise the G-index) for the G-index calculation to eliminate the most imprecise G-index data in the sample.[14] The fixed-effects structure for the slope coefficients in equation (3) is also applied. Following H2, the decrease in conditional conservatism is expected to be less for firms with high shareholder power, so β_{15} is predicted to be positive for institutional ownership and negative for G-index.

3.4 DEBT-CONTRACTING DEMAND AND THE CHANGE IN CONDITIONAL CONSERVATISM

Finally, we study how the change in conditional conservatism varies across firms with different levels of debt-contracting demand. We test whether the coefficient β_7 varies between firms with different debt-contracting demand. As before, we interact a debt-contracting-demand proxy *DCD* with every term in regression model (2) and estimate the following model:

$$\begin{aligned} EARN_{i,t} = {} & \alpha_i + \omega_t + \beta_1 RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} \times NEG_{i,t} \\ & + POST_{i,t} \times \left(\beta_4 + \beta_5 RET_{i,t} + \beta_6 NEG_{i,t} + \beta_7 RET_{i,t} \times NEG_{i,t}\right) \\ & + DCD_i \times \left(\beta_8 RET_{i,t} + \beta_9 NEG_{i,t} + \beta_{10} RET_{i,t} \times NEG_{i,t}\right) \\ & + DCD_i \times POST_{i,t} \times \left(\beta_{11} + \beta_{12} RET_{i,t} + \beta_{13} NEG_{i,t} + \beta_{14} RET_{i,t} \times NEG_{i,t}\right) + \epsilon_{i,t} \end{aligned} \quad (5)$$

where, following Jayaraman and Shivakumar [2013], debt-contracting-demand proxy *DCD* is first defined as a dummy variable that equals 1 if a firm is a net debt issuer (i.e., a firm increased total debt, defined as short-term debt plus long-term debt scaled by total assets, around law enactment). For robustness, we use the change in total debt after law enactment itself as another proxy for *DCD*. One advantage of using this continuous measure is that it distinguishes the debt-contracting demand for firms within the net debt issuer and non-issuer groups. *DCD* is not included as a separate term in the regression because the firm fixed-effects subsume it. The fixed-effects

[14] Institutional ownership and G-index could be affected by the director-liability-reduction laws. In an untabulated robustness test, we measure them in the year just before the enactment of the laws and find consistent results.

structure on the slope coefficients in equation (3) is also applied. Following H3, the decrease in conditional conservatism is expected to be less for firms with high debt-contracting demand. Therefore, β_{14} is predicted to be positive.

4. Sample Selection and Descriptive Statistics

We start our sample with all NYSE, AMEX, and NASDAQ firm-year observations between 1976 and 2002 in the Compustat/CRSP database. We end the sample in 2002 to eliminate any confounding effects of SOX. We first exclude firm-years with missing values for the key variables used in our regressions, with non-positive total assets, or with beginning-of-year stock price below $1.00. We also exclude financial institutions because their accounting is different and they are regulated. We drop cross-listed foreign firms and firm-years with missing incorporation state data. Firms that are incorporated in Washington, D.C., and U.S. territories such as Puerto Rico are also dropped.[15] Following Jayaraman and Shivakumar [2013] and Aier, Chen and Pevzner [2014], we exclude law-enactment-year observations and require our sample firms to have observations from at least one year before and one year after the law enactments. Our main sample consists of 32,418 firm-year observations. Our sample size is smaller when additional data such as institutional ownership are used. Our results are robust to many alternative sample selection criteria, which we discuss in Internet Appendix A.

Table 1 lists the year each state first enacted a director-liability-reduction law and the number of firm-year observations before and after the law was enacted for each state. For most states, there are more observations after law enactment than before. Most states enacted their first director-liability-reduction laws in the late 1980's and early 1990's, with only Missouri (2000) and West Virginia (2002) waiting until the 2000's. Approximately 43 percent (=14,059/32,418) of the

[15] Compustat provides the current incorporation state data, but companies change their states of incorporation over time. To obtain historical data, we combine the incorporation state data from (1) 10-K heading (downloaded on 02/21/2017 from Bill McDonald's website http://www3.nd.edu/~mcdonald/10-K_Headers/10-K_Headers.html), (2) Compact Disclosure company address data, and (3) IRRC database. Because these sources only contain incorporation state data as early as 1989, we further search company profiles on Mergent Online to identify any incorporation-state changes before 1989.

firm-year observations are from Delaware, which often pioneers corporate law reforms that are particularly attractive to public firms.

Table 2 reports the descriptive statistics of the samples before and after law enactments in Panel A and Panel B, respectively. Panel C in Table 2 reports the mean and median differences of the variables before and after law enactments and tests whether the differences are statistically different from zero. The average income before extraordinary items scaled by market value of equity (*EARN*) is smaller after law enactment (0.048) than before (0.102), which is a statistically significant difference (p-value < 0.001). However, our univariate analysis cannot identify whether this decrease is due to a general time trend of more frequent losses (e.g., Hayn [1995]; Fama and French [2001], [2004]) or due to the director-liability-reduction laws. There is also a statistically significant decrease (p-value < 0.001) in buy-and-hold market-adjusted stock return (*RET*). Both before and after law enactments, about half of the observations are classified as having bad news about future cash flows (*NEG* = 1). The average firm size, measured as the natural logarithm of market value of equity, grows, while the average book-to-market ratio falls after law enactments. These differences are statistically significant. Since book-to-market ratio is often used as a proxy for conservatism, the fall in average book-to-market ratio is consistent with accounting becoming more conservative after the director-liability-reduction laws were enacted, which is the opposite of our prediction and may be because our univariate analysis does not separately model the law enactments and market-wide time trends.[16]

5. Empirical Results

Table 3 reports results for the change in conditional conservatism after director-liability-reduction law enactments based on regression model (2). The standard errors are clustered at the state level for all regressions (Bertrand and Mullainathan [2003]; Bertrand, Duflo, and

[16] Book-to-market ratio reflects both unconditional and conditional conservatism in the past (e.g., Basu [2001], [2005]; Beaver and Ryan [2005]; Roychowdhury and Watts [2007]) although it also reflects other factors such as historical cost accounting, inflation, economic rents and technological change (e.g., Basu [1997]; Sunder, Sunder, and Zhang [2018]) and reductions in exchange listing standards over time (Fama and French [2001], [2004]; Srivastava [2014]).

Mullainathan [2004]; Jayaraman and Shivakumar [2013]). Our fixed-effects structure absorbs the intercept and the slope coefficients from the Basu [1997] model, so we do not report them in the table. In Column I, we find that the slope coefficient on the three-way interaction of *POST*, *NEG*, and *RET* is negative and statistically significant (β_7 = -0.221, *p*-value = 0.015). Consistent with H1, conditional conservatism decreases on average after the director-liability-reduction laws were enacted. In untabulated tests, we find that β_7 is negative and significant but smaller when the fixed-effects structure is dropped (β_7 = -0.073, *p*-value < 0.001).[17] Prior research reports a market-wide increase in conservatism during our sample period (e.g., Ryan and Zarowin [2003]), so our main estimate is likely biased upward when this trend is not controlled for. The coefficient on *POST* is statistically insignificant. Thus, the broad decline in earnings observed in Table 2 does not seem to be related to the enactment of the director-liability-reduction laws.

Under the internal affairs doctrine, the law of the state of incorporation governs corporate law disputes. However, firms can also be sued in their principal place of business (i.e., headquarters state). While the director-liability-reduction law of the incorporation state is always applied, the courts in the headquarters state (if different from incorporation state) might apply the laws slightly differently (Armour, Black, and Cheffins [2012]). For robustness, we estimate model (2) using a subsample of firms that are headquartered and incorporated in the same state in Column II of Table 3. We find consistent results. When comparing β_7 from this subsample (-0.223) with that from the subsample of firms that are headquartered and incorporated in different states (-0.220, untabulated), we find that they do not differ statistically or economically.

Table 4 presents the estimates of the moderating effect of shareholder power on the change in conditional conservatism after the director-liability-reduction-law enactments based on regression model (4). The sample sizes shrink because the tests require institutional ownership or G-index data (21,531 and 11,774 firm-years. respectively). Our two proxies for institutional

[17] The coefficient on the interaction of *NEG* and *RET* is 0.128 (*p*-value < 0.001). Thus, adding it to the coefficient on the three-way interaction we have 0.055 (*p*-value < 0.001), suggesting that firms still report conservatively after the law enactments.

ownership are the percentage of shares owned by institutional investors and the percentage of shares owned by dedicated institutional investors following Bushee [2001]. We estimate model (4) using both proxies and find that the coefficient on the four-way interaction of *SHPOWER*, *POST*, *NEG*, and *RET* is positive and significant at the 1 percent level (Specification I: β_{15} = 0.164, *p*-value = 0.007; Specification II: β_{15} = 0.489, *p*-value = 0.007). In Specification III, we examine the moderating effect of the G-index and find that the coefficient on the four-way interaction is negative and significant at the 5 percent level (β_{15} = -0.023, *p*-value = 0.050). Consistent with H2, firms with higher shareholder power have less of a decrease in conditional conservatism after law enactments. Using the estimates from Specification II, we calculate that the director-liability-laws would not reduce conditional conservatism when dedicated institutional investors hold more than 49.9 percent (=0.244/0.489) of the firm's shares. Because the highest dedicated institutional ownership in our sample is 38.3 percent, it is very unlikely that the laws increased conditional conservatism even for high institutional ownership firms. The coefficient on the interaction of *POST*, *NEG*, and *RET* is negative and significant for Specifications I and II (Specification I: β_7 = -0.296, *p*-value < 0.001; Specification II: β_7 = -0.244, *p*-value = 0.040), which is consistent with H1 and the findings in Table 3. This coefficient is negative but statistically insignificant for Specification III, indicating that conditional conservatism does not decrease for the firms with the least managerial power (i.e., G-index equals 0).

Table 5 shows the estimates of the moderating effect of debt-contracting demand on the change in conditional conservatism after the enactments of director-liability-reduction laws based on regression model (5). We use two proxies for debt-contracting demand: a dummy variable that equals 1 if the firm is a net debt issuer (i.e., total debt increases after law enactment) following Jayaraman and Shivakumar [2013] and the change in total debt itself. We find that the coefficient on the four-way interaction of *DCD*, *POST*, *NEG*, and *RET* is positive and significant at the 1 percent level for both proxies (Specification I: β_{14} = 0.121, *p*-value < 0.001; Specification II: β_{14} = 0.449, *p*-value < 0.001). This finding suggests that firms with higher debt-contracting demand

had less reduction in conditional conservatism after director-liability-law enactments consistent with H3. Consistent with H1 and findings in the previous tables, the coefficient on the interaction of *POST*, *NEG*, and *RET* is negative and statistically significant in both specifications (Specification I: β_7 = -0.256, *p*-value = 0.004; Specification II: β_7 = -0.211, *p*-value = 0.016). The coefficient on the interaction of *DCD*, *NEG*, and *RET* is negative and significant, which suggests that net debt issuers were less conservative before laws were enacted. Given that net debt issuers had lower leverage on average before law enactment (*p*-value < 0.001), this finding is consistent less levered firms reporting less conservatively (e.g., Ball and Shivakumar [2005]).

Summing up, we find strong evidence that conditional conservatism decreases after states enacted laws reducing non-officer-directors' incentives to monitor the financial reporting process. The smaller decrease in conditional conservatism for firms with high shareholder power and high debt-contracting demand reported in Table 4 and Table 5, respectively, suggests that the demand for conservatism from major stakeholders moderates the non-officer directors' influence on conditional conservatism.

6. Validation Tests: Asymmetric Timeliness of Accruals and Auditor Choice

6.1 ASYMMETRIC TIMELINESS OF OPERATING ACCRUALS AND CASH FLOWS

The asymmetric timeliness of earnings reported above could arise from operating accruals and/or cash flow from operations (CFO). Hsu, O'Hanlon, and Peasnell [2012] and Collins, Hribar, and Tian [2014] argue that only the asymmetric timeliness of operating accruals can be interpreted as conditional conservatism while the asymmetric timeliness of CFO cannot. Therefore, we conduct several accruals tests to validate that our main results stem from changes in conservatism.

First, we adopt Collins, Hribar, and Tian's [2014] approach and replace the dependent variable of regression (2) with accruals or CFO as follows:

$$Y_{i,t} = \alpha_i + \omega_t + \beta_1 RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} \times NEG_{i,t} + POST_{i,t} \times (\beta_4 + \beta_5 RET_{i,t} + \beta_6 NEG_{i,t} + \beta_7 RET_{i,t} \times NEG_{i,t}) + \epsilon_{i,t} \quad (6)$$

where *Y* is either operating accruals *ACCR* (calculated using a balance sheet approach following Collins, Hribar and Tian [2014] as the change in current non-cash assets minus the change in current non-debt liability minus depreciation) or operating cash flow *CFO* (defined as the difference between earnings *EARN* and accruals *ACCR*), with both scaled by beginning-of-year market value of equity. The sample size for this table is smaller because we require sufficient data to calculate accruals.[18] The fixed-effects structure in equation (3) is also added. The coefficient of interest for regression model (6) is β_7. If our main results reflect a decrease in conditional conservatism after law enactments, we expect β_7 to be negative in the accruals regression.

Columns I and II in Panel A of Table 6 report the estimates of model (6) for accruals and CFO, respectively. We find that the coefficient on the three-way interaction of *POST*, *NEG*, and *RET* is negative and significant at the 1 percent level for accruals (β_7 = -0.235, *p*-value = 0.008) but is positive and statistically insignificant for CFO (β_7 = 0.011, *p*-value = 0.313). Thus, Table 6 suggests that our main findings are fully driven by changes in accruals asymmetry, consistent with H1. In untabulated tests, we find that the cross-sectional results in Tables 4 and 5 hold if we replace earnings with operating accruals as the dependent variable.

6.2 ASYMMETRIC TIMELINESS OF ACCRUAL COMPONENTS

Conditional conservatism is implemented by asset impairments and write-offs (Basu [1997]; Watts [2003]; Qiang [2007]). Thus, examining how the asymmetric timeliness of different accrual components changes differently after the law enactments can further verify if our main results likely capture conservatism.[19] For example, conditional conservatism applied via a lower-of-cost-or-market adjustment for inventory should lead to asymmetric timeliness of the change in

[18] We set depreciation (Compustat data item DP) and deferred tax and investment tax credit (Compustat data item TXDITC) to zero if missing. We use a balance sheet approach because statement of cash flow data under SFAS 95 (FASB 1987) become available only after many states had already adopted director-liability-reduction laws (Table 1).
[19] Conditional conservatism can be measured directly from goodwill impairments and other asset write-downs (Lawrence, Sloan, and Sun [2013]; Banker, Basu, and Byzalov [2017]). Unfortunately, detailed data on goodwill impairments and other asset write-downs are only available from Compustat since 1996, when all states except Missouri and West Virginia had already enacted their director-liability-reduction laws (Table 1). Small subsamples from these two states would cause a low-power test of the impact of director-liability-reduction laws on conditional conservatism.

inventory component of accruals, but there is no symmetric higher-of-cost-or-market rule for current liabilities (Ijiri and Nakano [1989]; Byzalov and Basu [2016]).

We analyze working capital accruals because Banker, Basu, Byzalov, and Chen [2016] report that the depreciation asymmetry is caused by cost stickiness rather than conservatism. We decompose working capital accruals into change in noncash current assets ΔCA (defined as change in current assets minus cash) and negative change in current operating liabilities $-\Delta CL$ (defined as negative change in current liabilities minus short-term debt minus deferred tax and investment tax credits). Based on the discussion above, we predict that current assets but not current liabilities will exhibit less asymmetric timeliness in model (6) after law enactments, so β_7 will be negative for current assets but not for current liabilities.

Columns III and IV in Panel A of Table 6 report our estimation results. We find that the asymmetric timeliness of the current assets component ΔCA is -0.251, similar in size to the asymmetric timeliness of total accruals (-0.235), and significant at the 5 percent level. We do not find a significant change for the current liability component $-\Delta CL$. These contrasting results are consistent with the predicted impact of conditional conservatism on working capital accrual components (Byzalov and Basu [2016]; Ijiri and Nakano [1989]). In untabulated tests, we find that the cross-sectional results in Tables 4 and 5 continue to hold if we replace earnings with the change in current assets, ΔCA.

Following Byzalov and Basu [2016], we decompose change in noncash current assets ΔCA into change in inventories (ΔINV) and change in receivables (ΔREC) and examine them separately. Columns V and VI report negative and statistically significant β_7 coefficients for both ΔINV and ΔREC. Cost stickiness should not affect receivables (which relate to revenues rather than costs) and predicts an opposite asymmetry for inventory (Byzalov and Basu [2016]), eliminating a possible alternative explanation for our main results.

6.3 ACCRUAL ASYMMETRY AND DISAGGREGATED BAD NEWS INDICATORS

Byzalov and Basu [2016] argue and show that accruals exhibit an asymmetry with respect to various disaggregated non-return bad news indicators such as negative sales growth, negative employee growth, and negative CFO.[20] Their model extends the Dechow and Dichev [2002], Ball and Shivakumar ([2005]; [2006]), and Allen, Larson, and Sloan [2013] models as follows:

$$\begin{aligned} ACC_{i,t} = {} & \alpha_i + \omega_t + \beta_1 SGR_{i,t} + \beta_2 EGR_{i,t} + \beta_3 CF_{i,t-1} + \beta_4 CF_{i,t} + \beta_5 CF_{i,t+1} \\ & + \beta_6 DS_{i,t} + \beta_7 DE_{i,t} + \beta_8 DC_{i,t-1} + \beta_9 DC_{i,t} + \beta_{10} DC_{i,t+1} + \beta_{11} SGR_{i,t} \\ & \times DS_{i,t} + \beta_{12} EGR_{i,t} \times DE_{i,t} + \beta_{13} CF_{i,t-1} \times DC_{i,t-1} + \beta_{14} CF_{i,t} \\ & \times DC_{i,t} + \beta_{15} CF_{i,t+1} \times DC_{i,t+1} + \epsilon_{i,t} \end{aligned} \quad (7)$$

where *ACC* is accruals scaled by beginning-of-year total assets, *SGR* is sales growth, *EGR* is employee growth, *CF* is operating cash flow scaled by beginning-of-year total assets, *DS*, *DE*, and *DC* are indicators for negative *SGR*, *EGR*, and *CF*, respectively. Byzalov and Basu [2016] predict and find that conservatism leads to positive β_{11}, β_{12}, β_{14}, and β_{15} and negative β_{13}.

We apply the Byzalov and Basu [2016] model by interacting every term except the fixed effects with *POST*. We also use a similar fixed-effects structure to equation (3) for this model (i.e., the state- and year- fixed-effects structure is added for β_j, j = 1, 2, …, 15). We predict that, because conditional conservatism decreases after the law enactments, the coefficients of the additional interaction terms will be opposite in sign to those in the Byzalov and Basu [2016] model. We require all our observations to have sales revenue and number of employee data. Following Banker, Basu, Byzalov, and Chen [2016], we also eliminate observations with more than 50 percent change in either sales or number of employees to remove firm-years that potentially had non-articulating transactions such as mergers & acquisitions and significant divestitures that systematically impact accruals through channels other than conservatism (Hribar and Collins [2002]).

[20] Patatoukas and Thomas [2011], [2016] argue that the conservatism measure from the Basu [1997] model is biased because of the return distribution. Because the Byzalov and Basu [2016] model examines non-return indicators, it does not suffer from this bias. In addition, Collins, Hribar and Tian [2014] report that the Patatoukas and Thomas [2011] bias is concentrated in CFO and does not affect the operating accruals that we analyze in this section.

Panel B of Table 6 reports the estimation results. We find that the coefficient signs are all consistent with our predictions. Three out of five coefficients are statistically significant at the 5 percent level or better. The coefficients are not all statistically significant perhaps because the bad news indicators often occur simultaneously, which can cause high multicollinearity. For example, the correlation between *POST*×*SGR*×*DS* and *POST*×*EGR*×*DE* is 0.52. Overall, our findings in Table 6 are consistent with accruals asymmetric timeliness falling after director-liability-reduction laws were enacted, validating our inference about decreased conservatism.

6.4 DIRECTOR-LIABILITY-REDUCTION LAWS AND AUDITOR CHOICE

Prior literature hypothesizes that Big N auditors are more likely to supply conditional conservatism for litigation and reputation reasons, and shows that they are positively associated with conditional conservatism (e.g., Basu, Hwang, and Jan [2001], Ruddock, Taylor and Taylor, [2006]). In other words, when selecting the "accepted set" of accounting policies *ex-ante*, Big N auditors can serve as a "commitment device" to ensure conservatism is supplied. Thus, we investigate whether firms are less likely to choose Big N auditors after the enactment of the director-liability-reduction laws, as one way to reduce their commitment to conservatism. Specifically, we estimate the following regression:

$$BIGN_{i,t} = \beta POST_{i,t} + \sum \delta Controls_{i,t} + StateFE + YearFE + \epsilon_{i,t} \quad (8)$$

where the dependent variable *BIGN* is a dummy variable that equals 1 if firm *i* chooses a Big N auditor (Compustat data item AU is between 1 and 8) in year *t* and equals 0 otherwise. We follow DeFond, Erkens, and Zhang [2016] and include log of total assets, asset turnover ratio, current ratio, leverage, and return-on-assets as control variables. Consistent with our main tests, state and year fixed effects are included, and standard errors are clustered by states. Because *BIGN* is a dummy variable, we estimate a logit regression. Our results are consistent if we estimate a linear regression instead. We expect that, if firms become less likely to choose Big N auditors, the coefficient on *POST* (i.e., β) should be negative.

Table 7 presents the estimation results of model (8). We find the coefficient on *POST* is negative and significant at the 5 percent level, consistent with our expectation. The marginal effect of the coefficient is -0.030, suggesting that firms are 3 percent less likely to choose a Big N auditor after the director-liability-reduction-law enactments.[21]

7. Robustness Checks

7.1 INTERACTION BETWEEN TIMING OF ANTITAKEOVER LAWS AND DIRECTOR-LIABILITY-REDUCTION-LAW ENACTMENTS

Jayaraman and Shivakumar [2013] predict and find that business combination laws, a type of antitakeover law, lead to an increased supply of conditional conservatism to counter an amplified agency problem between managers and shareholders/debtholders. Because many business combination laws were enacted during our sample period, we re-estimate our regression by controlling for these laws as follows:[22]

$$\begin{aligned} EARN_{i,t} = {} & \alpha_i + \omega_t + \beta_1 RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} \times NEG_{i,t} \\ & + POST_{i,t} \times \left(\beta_4 + \beta_5 RET_{i,t} + \beta_6 NEG_{i,t} + \beta_7 RET_{i,t} \times NEG_{i,t}\right) \\ & + BC_{i,t} \times \left(\beta_8 + \beta_9 RET_{i,t} + \beta_{10} NEG_{i,t} + \beta_{11} RET_{i,t} \times NEG_{i,t}\right) + \epsilon_{i,t} \end{aligned} \quad (9)$$

where *BC* is an indicator that equals one if the observation is after the business combination law enactment and equals zero otherwise. The fixed-effects structure as in equation (3) is also included in the estimation. We predict that β_7 is negative and β_{11} is positive. To make our results comparable to those of Jayaraman and Shivakumar [2013], we follow their sample selection criteria and use a subsample that (1) stops at 1995, (2) drops the business combination law-enactment-year observations, and (3) ensures that each firm has data from at least one year before and one year after the business combination law enactments.

[21] We find that 15.6 percent of the sample firms chose non-Big N auditors before law enactments, so the 3 percent represents a nearly 20 percent increase in use of non-Big N auditors.

[22] Bertrand and Mullainathan [2003] identify 30 states that enacted business combination laws before 1995, and Barzuza [2009] extends the list to 2007. Combining their law enactment data with ours, we find that, among all 50 states, 36 states enacted the director-liability-reduction laws first, 9 states enacted both laws in the same year, and 5 states enacted the business combination laws first. Because business combination laws increase conditional conservatism, their confounding effect works against us finding a negative impact of director-liability-reduction laws on conditional conservatism.

Table 8 reports the results of the regression model (9). In Columns I and II, we estimate two specifications, one with only the year-fixed-effects structure for the Basu coefficients to be consistent with Jayaraman and Shivakumar [2013] and the other with the complete fixed-effects structure as in equation (3). We find that, for both specifications, the impact of director-liability-reduction laws is negative and significant, consistent with our main results. Furthermore, because Karpoff and Wittry [2018] find that the business combination laws may not be the only effective antitakeover laws, we extend model (9) by controlling for the impact of the other four types of antitakeover laws (i.e., control share acquisition law, fair price law, directors' duties law, and poison pill law) in Column III. Also, to ensure the results are not driven by the first-generation antitakeover laws, we exclude observations before 1983. We find consistent results again.[23] Consistent with Jayaraman and Shivakumar [2013], we find that conditional conservatism increases after the enactments of business combination laws. However, the changes in conditional conservatism are not statistically significant after the enactments of other types of antitakeover laws (untabulated).

Related to the above analysis but more generally, we also consider the possible endogenous timing of law enactments. The endogeneity may arise from two sources, reverse causality and correlated omitted variables. We argue that reverse causality is unlikely because the states enacted the laws to lower non-officer-directors' litigation risk and D&O insurance premium, not to respond to or facilitate aggressive accounting. For the potential correlated omitted variables problem, if such omitted variables are time-invariant for each state or have a homogeneous impact on the entire market for each period, our fixed-effects structure (equation (3)) controls sufficiently. However, there may be other state-level time-varying variables such as state gross domestic product (GDP) that simultaneously affect law enactment and conditional conservatism. To address this alternative explanation, we follow Jenkins [1995] and estimate a logit hazard model to

[23] In a robustness check, we further exclude the lobbying firms for the antitakeover laws as identified by Karpoff and Wittry [2018, Table 3] and find consistent results.

examine whether the timing of the law enactments can be predicted by state-level time-varying variables. The dependent variable is an indicator of whether the director-liability-reduction law was enacted for a particular state-year combination. Once a state enacts the law, all future observations of that state are dropped from the sample. The hazard rate *h* (i.e., the probability that the director-liability-reduction law is enacted in that year, conditional on the law has not been enacted yet) for state j and year t is as follows:

$$h_{j,t} = Logit(\omega_t + \sum_{k=1}^{K} \left(\gamma_k \bar{X}_{k,j} + \delta_k \left(X_{k,j,t} - \bar{X}_{k,j}\right)\right) \quad (10)$$

where $X_{k,j,t}$ is the kth state-level variable for state j in year t, $\bar{X}_{k,j}$ is the within-state average of this variable, and ω_t represents year fixed effects.[24]

Equation (10) distinguishes a time-invariant component of *X* (i.e., average of *X*, denoted as $\bar{X}$) and a time-varying component of *X* (i.e., $X - \bar{X}$). The coefficient of interest is δ_k. That is, if δ_k is statistically significant, we should control for the variable in our regression because of potential endogeneity. In contrast, ω_t and γ_k are less important for us because the market-wide trend and time-invariant component of state-level variables are already controlled for by the fixed-effects structure in equation (3). We select variables that may have a strong impact on law enactment, including (1) log of number of public firms incorporated in the state, (2) log of number of public firms headquartered in the state, (3) business combination law dummy, and more generic variables, including (4) log of total GDP and (5) log of per capita GDP.[25] To estimate the model, we construct a sample starting from 1985, the year when the *Smith v. Van Gorkom* suit took place. In untabulated tests, we find that all the coefficients are statistically insignificant except for the average log of number of public firms incorporated in the state (*p*-value = 0.078). This finding suggests that states with more incorporated firms are more likely to enact the director liability laws

[24] We do not include state fixed effects in the hazard rate function because they will "perfectly explain" the timing of law enactments (i.e., each state fixed effect explains the timing of law enactment for each state) and cause all the other coefficients to remain unidentified.

[25] We obtain state-level GDP and population data from U.S. Bureau of Economic Analysis website https://www.bea.gov/regional/ (downloaded on 11/09/2017).

early, presumably because the D&O insurance crisis affected more firms in those states and, hence, enacting director-liability-reduction laws became more urgent. Because the fixed-effects structure already controls for the average log of number of public firms incorporated in the state, it will not bias our main findings. Finally, we also explicitly control for these five variables as well as their interactions with RET, NEG, and $RET \times NEG$ in our regression. We find that our results are robust.

7.2 PARALLEL TRENDS

To verify that the parallel trends assumption holds, we examine when the change in conditional conservatism occurred (cf. Autor [2003]; Acharya, Baghai, and Subramanian [2014]). Specifically, we set the conditional conservatism five or more years before the enactment of the laws as a benchmark and examine whether conditional conservatism decreased before the enactment of the laws as follows:

$$\begin{aligned} EARN_{i,t} = {} & \alpha_i + \omega_t + \beta_1 RET_{i,t} + \beta_2 NEG_{i,t} + \beta_3 RET_{i,t} \times NEG_{i,t} \\ & + POST_{i,t} \times \left(\beta_4 + \beta_5 RET_{i,t} + \beta_6 NEG_{i,t} + \beta_7 RET_{i,t} \times NEG_{i,t}\right) \\ & + \sum_{\tau=1}^{4} PRE_{\tau,i,t} \times \left(\beta_{8,\tau} + \beta_{9,\tau} RET_{i,t} + \beta_{10,\tau} NEG_{i,t} + \beta_{11,\tau} RET_{i,t} \times NEG_{i,t}\right) + \epsilon_{i,t} \end{aligned} \tag{11}$$

where PRE_τ is a dummy variable that equals one if the observation is from τ (τ = 1, 2, 3, 4) year(s) before law enactments and equals zero otherwise.

If the parallel trends assumption holds, the coefficient $\beta_{11,\tau}$ should not be statistically significant (i.e., there is no difference in conditional conservatism between firms in different states before the laws were enacted). Table 9 reports the results of regression (11). The coefficient $\beta_{11,\tau}$ is statistically insignificant for every τ, which supports the parallel trends assumption.

7.3 OTHER ROBUSTNESS CHECKS

In Internet Appendix A, we discuss additional tests that show our results are robust to (1) the placebo test suggested by Patatoukas and Thomas [2016], (2) including additional firm characteristics as controls, (3) alternative sample-selection criteria, (4) self-selection for state of

incorporation, and (5) including state-specific linear time trends in the fixed-effects structure of equation (3).

8. Conclusion

Non-officer directors are usually outsiders who are elected to monitor and help resolve the agency conflict between managers and shareholders in various domains including financial reporting. However, verifying this assertion empirically is challenging, because director selection and director behavior are usually endogenous choices. To draw stronger causal inferences, we study the staggered enactment of director-liability-reduction laws by states that reduced non-officer-directors' litigation risk. We find that conditional conservatism decreased after the law enactments, which is consistent with director-liability-reduction laws reducing non-officer-directors' legal liability, and hence, reducing their monitoring of financial reporting practices. We also find that the decrease in conditional conservatism is smaller when firms have high shareholder power or high debt-contracting demand, which suggests that the demand for conservatism from major stakeholders moderates the relation between the law enactments and conditional conservatism.

Our results are robust to several sensitivity checks such as using alternative conditional conservatism models and controlling for confounding factors that could impact our asymmetric timeliness measure. Our results provide insight into the monitoring by non-officer directors of financial reporting and have implications for evaluating corporate governance reforms and corporate law reforms that involve non-officer directors such as the Sarbanes-Oxley Act of 2002. Prior research focuses on audit committee members as monitors of the financial reporting process (e.g., Abbott, Parker, and Peters [2004]; Krishnan and Visvanathan [2008]); our results suggest that researchers might benefit from expanding their analysis to all outside directors.

Appendix: Variable Definitions

Variable	Definition
ACC	Accruals scaled by total assets, defined as the change in non-cash current assets (Compustat data item ACT - Compustat data item CHE) minus the change in non-debt current operating liability (Compustat data item LCT - Compustat data item DLC - Compustat data item TXDITC) minus depreciation (Compustat data item DP) scaled by beginning-of-year total assets (Compustat data item AT)
ACCR	Accruals, defined as the change in non-cash current assets (Compustat data item ACT - Compustat data item CHE) minus the change in non-debt current operating liability (Compustat data item LCT - Compustat data item DLC - Compustat data item TXDITC) minus depreciation (Compustat data item DP) scaled by beginning-of-year market value of equity defined as common share price (Compustat data item PRCC_F) times common shares outstanding (Compustat data item CSHO)
ATURNOVER	Asset turnover ratio, defined as sales revenue (Compustat data item REVT) scaled by beginning-of-year total assets (Compustat data item AT)
BIGN	An indicator for choosing a Big N auditor. The variable takes the value of 1 if auditor (Compustat data item AU) is 1 to 8
BTM	Book-to-market ratio, defined as beginning-of-year book value of equity (Compustat data item CEQ) divided by beginning-of-year market value of equity defined as common share price (Compustat data item PRCC_F) times common shares outstanding (Compustat data item CSHO)
CF	Operating cash flows scaled by total assets, defined as the difference between income before extraordinary items (Compustat data item IB) scaled by beginning-of-year total assets (Compustat data item AT) and accruals (*ACC*)
CFO	Operating cash flows, defined as the difference between earnings (*EARN*) and accruals (*ACCR*)
CURRENT	Current ratio, defined as current assets (Compustat data item ACT) scaled by current liability (Compustat data item LCT)
DCF	An indicator for bad cash flow news. This variable takes the value of 1 when operation cash flow (*CF*) is negative
DCD	Proxy for debt-contracting demand, defined as (1) an indicator that equals 1 if a firm is a net debt issuer (i.e., a firm experienced an increase in total debt, defined as short term debt plus long term debt (Compustat data item DLC + Compustat data item DLTT) scaled by total assets (Compustat data item AT) between the pre- and post-law-enactment period), following Jayaraman and Shivakumar (2013), and (2) the change in firm total debt from before to after law enactment scaled by total assets

DE	An indicator for bad employment news. This variable takes the value of 1 when employee growth (*EGR*) is negative
DP	Depreciation component of accruals, defined as depreciation and amortization (Compustat data item DP) scaled by beginning-of-year market value of equity defined as common share price (Compustat data item PRCC_F) times common shares outstanding (Compustat data item CSHO)
DS	An indicator for bad sales news. This variable takes the value of 1 when sales growth (*SGR*) is negative
EARN	Earnings, defined as income before extraordinary items (Compustat data item IB) scaled by beginning-of-year market value of equity defined as common share price (Compustat data item PRCC_F) times common shares outstanding (Compustat data item CSHO)
EGR	Employee growth rate, defined as the percentage change in total number of employees (Compustat data item EMP) from the previous year
LEV	Leverage, defined as beginning-of-year short term debt plus beginning-of-year long term debt (Compustat data item DLC + Compustat data item DLTT) scaled by beginning-of-year total assets (Compustat data item AT)
LN(ASSETS)	Log total assets, defined as the natural logarithm of beginning-of-year total assets (Compustat data item AT)
NEG	An indicator for bad cash flow news. This variable takes the value of 1 when market-adjusted stock return (*RET*) is negative and is 0 otherwise
POST	An indicator for firm-years after a director-liability law enactment. This variable takes the value of 1 when firm-year observations occur after the year in which a director-liability-reduction law is enacted in a firm's state of incorporation, and is 0 otherwise
RET	Market-adjusted stock return, defined as buy-and-hold stock return (CRSP data item RET) over the fiscal year (starting from three months after the fiscal year starts) adjusted by the value-weighted stock return (CRSP data item VWRETD) over the same period
ROA	Return on assets, defined as earnings before extraordinary items (Compustat data item IB) scaled by beginning-of-year total assets (Compustat data item AT)
SGR	Sales growth rate, defined as the percentage change in total sales revenue (Compustat data item REVT) from the previous year
SHPOWER	Proxy for shareholder power, defined as (1) the percentage of outstanding shares owned by institutional investors (Thomson Reuters data item INSTOWN_PERC), (2) the percentage of outstanding shares owned by dedicated institutional investors following Bushee (2001), and (3) G-index (indicates low shareholder power)

SIZE	Firm size, defined as the natural logarithm of market value of equity at the beginning of the year defined as beginning-of-year common share price (Compustat data item PRCC_F) times beginning-of-year common shares outstanding (Compustat data item CSHO)
Δ*CA*	Current assets component of accruals, defined as change in non-cash current assets (Compustat data item ACT - Compustat data item CHE) scaled by market value of equity at beginning-of-year defined as beginning-of-year common share price (Compustat data item PRCC_F) times beginning-of-year common shares outstanding (Compustat data item CSHO)
Δ*CL*	Current liability component of accruals, defined as change in non-debt current liabilities (Compustat data item LCT - Compustat data item DLC - Compustat data item TXDITC) scaled by market value of equity at beginning-of-year defined as beginning-of-year common share price (Compustat data item PRCC_F) times beginning-of-year common shares outstanding (Compustat data item CSHO)
Δ*INV*	Change in inventories, defined as change in inventories (Compustat data item INVT) scaled by market value of equity at beginning-of-year defined as beginning-of-year common share price (Compustat data item PRCC_F) times beginning-of-year common shares outstanding (Compustat data item CSHO)
Δ*REC*	Change in receivables, defined as change in receivables (Compustat data item RECT) scaled by market value of equity at beginning-of-year defined as beginning-of-year common share price (Compustat data item PRCC_F) times beginning-of-year common shares outstanding (Compustat data item CSHO)

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Table 1. Director-Liability-Reduction Laws' Enactment Years

State	Enactment Year	Before	After	Total
Alabama	1994	60	22	82
Arizona	1987	35	42	77
Arkansas	1987	32	36	68
California	1987	624	619	1243
Colorado	1987	79	103	182
Connecticut	1989	184	179	363
Delaware	1986	4897	9162	14059
Florida	1987	282	409	691
Georgia	1987	160	313	473
Hawaii	1989	24	26	50
Idaho	1987	32	34	66
Illinois	1993	241	88	329
Indiana	1986	207	366	573
Iowa	1987	103	104	207
Kansas	1987	43	58	101
Kentucky	1988	57	72	129
Louisiana	1987	53	68	121
Maine	1988	88	77	165
Maryland	1988	244	300	544
Massachusetts	1986	400	673	1073
Michigan	1987	234	320	554
Minnesota	1987	340	544	884
Mississippi	1991	3	11	14
Missouri	2000	267	38	305
Nebraska	1988	13	38	51
Nevada	1987	220	333	553
New Hampshire	1991	12	19	31
New Jersey	1987	367	468	835
New Mexico	1987	16	19	35
New York	1987	1070	1286	2356
North Carolina	1987	94	180	274
Ohio	1986	549	805	1354
Oklahoma	1987	57	79	136
Oregon	1987	99	162	261
Pennsylvania	1987	543	756	1299
Rhode Island	1987	43	69	112
South Carolina	1988	60	107	167
South Dakota	1987	20	30	50
Tennessee	1987	38	69	107
Texas	1987	330	463	793
Utah	1987	44	80	124

Vermont	1993	51	25	76
Virginia	1987	205	297	502
Washington	1987	106	200	306
Wisconsin	1987	259	334	593
Wyoming	1987	21	29	50
Total		12906	19512	32418
States Excluded Because of Missing Before or After Law Enactment Data				
Alaska	1988	-	-	-
Montana	1987	-	-	-
North Dakota	1993	-	-	-
West Virginia	2002	-	-	-

This table reports the first director liability reduction law enactment years and the number of firm-year observations before enactment, after enactment, and in total for each state.

Table 2. Descriptive Statistics

Panel A. Firm-year Observations Before Director-Liability-Reduction Law Enactment (N=12,906)

Variable	Mean	Median	Std. Dev.	P1	P25	P75	P99
EARN	0.102	0.104	0.116	-0.373	0.058	0.163	0.339
RET	0.075	0.004	0.479	-0.770	-0.209	0.252	2.032
NEG	0.494	0.000	0.500	0.000	0.000	1.000	1.000
SIZE	4.537	4.357	1.829	1.441	3.154	5.822	8.955
BTM	0.867	0.768	0.531	0.096	0.466	1.149	2.600

Panel B. Firm-year Observations After Director-Liability-Reduction Law Enactment (N=19,512)

Variable	Mean	Median	Std. Dev.	P1	P25	P75	P99
EARN	0.048	0.063	0.106	-0.457	0.029	0.091	0.283
RET	0.025	-0.035	0.466	-0.779	-0.253	0.205	1.814
NEG	0.546	1.000	0.498	0.000	0.000	1.000	1.000
SIZE	5.615	5.472	2.082	1.644	4.012	7.104	10.587
BTM	0.666	0.576	0.434	0.074	0.369	0.837	2.374

Panel C. Variable Difference Before and After Director-Liability-Reduction Law Enactment

Variable	ΔMean	*t*-statistic	ΔMedian	χ^2-statistic
EARN	-0.054	-43.13	-0.041	2.4×10^3
RET	-0.050	-9.38	-0.039	68.98
NEG	0.052	9.18	1.000	N/A
SIZE	1.078	47.83	1.115	1.2×10^3
BTM	-0.201	-37.28	-0.192	1.0×10^3

This table reports the sample descriptive statistics. Panel A (Panel B) summarizes the descriptive statistics for the subsample before (after) director liability law enactment. Panel C tests the difference in the mean and median of the variables for the periods before and after director liability law enactment, and whether the differences are statistically significant. Mood's non-parametric equality-of-medians test is used for the difference in medians (STATA command 'median'). The median difference for *NEG* cannot be tested because *NEG* is an indicator. All variables are defined in the Appendix and winsorized at the extreme percentiles.

Table 3. Change in Conditional Conservatism after Director-Liability-Reduction Law Enactments

Dependent Variable: *EARN*	Prediction	I	II
POST		-0.013	-0.012
		(-1.36)	(-1.43)
POST × NEG		-0.003	0.007
		(-0.21)	(0.38)
POST × RET		0.064	0.088***
		(1.64)	(2.79)
POST × NEG × RET	**–**	**-0.221****	**-0.223****
		(-2.52)	**(-2.02)**
Year Fixed Effects (Main)		Yes	Yes
Firm Fixed Effects (Main)		Yes	Yes
Year Fixed Effects (Basu Coefficients)		Yes	Yes
State Fixed Effects (Basu Coefficients)		Yes	Yes
Observations		32,418	14,143
R^2		0.561	0.634

This table presents regression results of earnings on stock return, negative stock return dummy, post director liability law dummy, and their interaction terms. The three terms of the Basu model (i.e., *NEG*, *RET*, *NEG × RET*) are subsumed by the fixed effects structure, thus their coefficients are not reported. Column II is estimated using firms that are headquartered and incorporated in the same state. All variables are defined in the Appendix. All continuous variables are winsorized at the extreme percentiles. *, **, and *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively, for a two-tailed test; *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level.

Table 4. Shareholder Power and the Change in Conditional Conservatism after Director-Liability-Reduction Law Enactments

Dependent Variable: *EARN*	Prediction	I *SHPOWER = % inst. investors owned shares*	II *SHPOWER = % dedicated inst. investors owned shares*	III *SHPOWER = G-Index*
POST		-0.014**	-0.014	-0.005
		(-2.03)	(-1.09)	(-0.30)
POST × NEG		-0.018	-0.010	0.063
		(-1.26)	(-0.78)	(0.88)
POST × RET		0.043**	0.068	0.061**
		(2.35)	(1.29)	(2.27)
POST × NEG × RET	−	**-0.296*** **	**-0.244** **	**-0.021**
		(-3.87)	**(-2.12)**	**(-0.84)**
SHPOWER		0.020**	-0.100***	-0.002
		(2.44)	(-3.14)	(-1.13)
SHPOWER × NEG		0.027**	-0.010	0.007***
		(2.54)	(-0.20)	(3.46)
SHPOWER × RET		0.014	0.171***	0.007
		(1.00)	(3.22)	(1.41)
SHPOWER × NEG × RET		-0.170***	-0.303**	0.023*
		(-3.66)	(-2.22)	(1.87)
SHPOWER × POST		0.040***	0.100***	0.001
		(4.46)	(3.63)	(0.61)
SHPOWER × POST × NEG		-0.014	0.026	-0.006***
		(-0.78)	(0.60)	(-3.18)
SHPOWER × POST × RET		-0.059***	-0.244***	-0.006
		(-4.90)	(-3.64)	(-1.54)
SHPOWER × POST × NEG × RET	I &II:+ III: −	**0.164*** **	**0.489*** **	**-0.023** **
		(2.84)	**(2.85)**	**(-2.02)**
Year Fixed Effects (Main)		Yes	Yes	Yes
Firm Fixed Effects (Main)		Yes	Yes	Yes
Year Fixed Effects (Basu Coefficients)		Yes	Yes	Yes
State Fixed Effects (Basu Coefficients)		Yes	Yes	Yes
Observations		21,531	21,531	11,774
R^2		0.551	0.526	0.575

This table presents regression results of earnings on stock return, negative stock return dummy, post-director-liability-law indicator, shareholder power and their interaction terms. The three terms of the Basu model (i.e., *NEG*, *RET*, *NEG × RET*) are subsumed by the fixed effects structure, thus their coefficients are not reported. All variables are defined in the Appendix. All continuous variables are winsorized at the extreme percentiles. *, **, and *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively, for a two-tailed test; *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level.

Table 5. Debt-Contracting Demand and the Change in Conditional Conservatism after Director-Liability-Reduction Law Enactments

Dependent Variable: *EARN*	Prediction	I	II
		DCD =1(change in net debt > 0)	*DCD = change in net debt*
POST		-0.016	-0.013
		(-1.61)	(-1.43)
POST × NEG		-0.001	0.001
		(-0.05)	(0.07)
POST × RET		0.069*	0.068*
		(1.84)	(1.81)
POST × NEG × RET	**–**	**-0.256*** **	**-0.211** **
		(-3.05)	**(-2.50)**
DCD × NEG		-0.002	-0.013
		(-0.46)	(-0.57)
DCD × RET		0.008*	0.038
		(1.82)	(1.28)
DCD × NEG× RET		-0.090***	-0.363***
		(-5.36)	(-6.52)
DCD × POST		0.008**	-0.018
		(2.01)	(-0.88)
DCD × POST × NEG		0.003	0.021
		(0.59)	(0.96)
DCD × POST × RET		-0.018***	-0.067**
		(-2.89)	(-2.12)
DCD × POST × NEG × RET	+	**0.121*** **	**0.449*** **
		(7.59)	**(6.50)**
Year Fixed Effects (Main)		Yes	Yes
Firm Fixed Effects (Main)		Yes	Yes
Year Fixed Effects (Basu Coefficients)		Yes	Yes
State Fixed Effects (Basu Coefficients)		Yes	Yes
Observations		32,418	32,418
R^2		0.563	0.564

This table presents regression results of earnings on stock return, negative stock return dummy, post-director-liability-law indicator, debt-contracting demand and their interaction terms. The three terms of the Basu model (i.e., *NEG*, *RET*, *NEG × RET*) are subsumed by the fixed effects structure, thus their coefficients are not reported. All variables are defined in the Appendix. All continuous variables are winsorized at the extreme percentiles. *, **, and *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively, for a two-tailed test; *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level.

Table 6. Change in Accruals and Cash Flows Asymmetry After Director-Liability-Reduction Law Enactments

Panel A: Director-Liability-Reduction Laws and Asymmetric Timeliness of Accruals, Accrual Components, and Cash Flows

Dependent Variable:	Prediction	I	II	III	IV	V	VI
		ACCR	*CFO*	ΔCA	$-\Delta CL$	ΔREC	ΔINV
POST		-0.056***	0.051***	-0.030**	-0.009	-0.017*	-0.019**
		(-6.74)	(6.15)	(-2.61)	(-0.77)	(-1.97)	(-2.26)
POST × NEG		0.027**	-0.027	0.010	0.011	0.014	-0.004
		(2.16)	(-1.23)	(0.73)	(0.67)	(1.57)	(-0.38)
POST × RET		0.175***	-0.152**	0.180**	-0.009	0.079**	0.123**
		(3.31)	(-2.32)	(2.68)	(-0.26)	(2.47)	(2.62)
POST × NEG × RET	I, III, V, & VI: − II & IV: 0	**-0.235*** **	**0.011**	**-0.251** **	**0.035**	**-0.095***	**-0.211*** **
		(-2.78)	**(1.02)**	**(-2.66)**	**(0.73)**	**(-1.74)**	**(-4.04)**
Year Fixed Effects (Main)		Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects (Main)		Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects (Basu Coefficients)		Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects (Basu Coefficients)		Yes	Yes	Yes	Yes	Yes	Yes
Observations		31,219	31,219	31,219	31,219	31,120	31,120
R^2		0.354	0.572	0.251	0.277	0.211	0.182

Panel B: Director-Liability-Reduction Law and Asymmetric Timeliness of Accruals to Disaggregated Bad News Indicators

Dependent Variable: *ACC*	Prediction	I
POST		0.001 (0.17)
POST* × *SGR* × *DS	−	**-0.055** **(-0.31)**
POST* × *EGR* × *DE	−	**-0.139***** **(-2.91)**
***POST* × CF_{t-1} × DC_{t-1}**	+	**0.377***** **(4.22)**
***POST* × CF_t × DC_t**	−	**-0.530**** **(-2.19)**
***POST* × CF_{t+1} × DC_{t+1}**	−	**-0.122** **(-1.05)**
Interactions between POST and Byzalov-Basu Terms		Yes
Year Fixed Effects (Main)		Yes
Firm Fixed Effects (Main)		Yes
Year Fixed Effects (Byzalov-Basu Coefficients)		Yes
State Fixed Effects (Byzalov-Basu Coefficients)		Yes
Observations		26,325
R^2		0.805

This table presents regression results of the change in accruals asymmetric timeliness around director-liability-law enactments. Panel A regresses accrual, accrual components, and operating cash flows on stock return, negative stock return dummy, post-director-liability-law indicator, and their interaction terms. Panel B regresses accruals on sales growth, employee growth, cash flows, bad news indicators, post-director-liability-law indicator, and their interaction terms. The three terms of the Basu model (i.e., *NEG*, *RET*, *NEG* × *RET*) in Panel A and the fifteen terms of the Byzalov and Basu model in Panel B are subsumed by the fixed effects structure; thus their coefficients are not reported. All variables are defined in the Appendix. All continuous variables are winsorized at the extreme percentiles. *, **, *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively, for a two-tailed test; *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level.

Table 7. Auditor Choice after Director-Liability-Reduction Law Enactments

Dependent Variable: *BIGN*	Prediction	I
POST	−	**-0.352****
		(-2.10)
LN(ASSETS)		0.372***
		(6.90)
ATURNOVER		0.233**
		(1.97)
CURRENT		-0.016
		(-0.63)
LEV		-0.147***
		(-4.73)
ROA		-0.675
		(-1.40)
Year Fixed Effects (Main)		Yes
State Fixed Effects (Main)		Yes
Observations		32,418
R^2		0.128

This table presents logit regression results of Big N dummy variable on post director liability law dummy, log total assets, asset turnover ratio, current ratio, leverage, and return on assets. Variables are defined in the Appendix. All continuous variables are winsorized at the extreme percentiles. *, **, *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively, for a two-tailed test; *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level.

Table 8. Controlling for Antitakeover Law Enactments

Dependent Variable: *EARN*	Prediction	I	II	III
POST		-0.011	-0.016**	-0.009
		(-1.11)	(-2.15)	(-0.73)
POST × NEG		0.010**	-0.001	-0.009
		(2.45)	(-0.09)	(-0.39)
POST × RET		0.063***	0.082**	0.061
		(7.09)	(2.07)	(1.22)
POST × NEG × RET	−	**-0.154****	**-0.250*****	**-0.235****
		(-2.55)	**(-3.09)**	**(-2.51)**
BC		0.010**	0.005	0.008
		(2.60)	(0.97)	(1.47)
BC × NEG		0.003	-0.001	0.005
		(0.87)	(-0.21)	(0.52)
BC × RET		-0.012	-0.003	-0.016*
		(-1.30)	(-0.27)	(-1.71)
BC × NEG × RET	+	**0.081*****	**0.024**	**0.067****
		(4.34)	**(1.03)**	**(1.79)**
Year Fixed Effects (Main)		Yes	Yes	Yes
Firm Fixed Effects (Main)		Yes	Yes	Yes
Year Fixed Effects (Basu Coefficients)		Yes	Yes	Yes
State Fixed Effects (Basu Coefficients)		No	Yes	Yes
Other Types of Antitakeover Laws		No	No	Yes
Observations		23,227	23,227	16,504
R^2		0.597	0.610	0.514

This table presents regression results of earnings on stock return, negative stock return dummy, post director liability law dummy, business combination law dummy, and their interaction terms. Column III is generated using observations after (including) 1983 only, and the regression also controls for the impact of other types of antitakeover laws. The three terms of the Basu model (i.e., *NEG*, *RET*, *NEG × RET*) are subsumed by the fixed effects structure, thus their coefficients are not reported. Variables are defined in the Appendix. All continuous variables are winsorized at the extreme percentiles. *, **, and *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively, for a two-tailed test; *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level.

Table 9. Test for Parallel Trends Assumption

Dependent Variable: *EARN*	Prediction	I
$PRE_1 \times NEG \times RET$	0	-0.027
		(-0.57)
$PRE_2 \times NEG \times RET$	0	-0.045
		(-0.98)
$PRE_3 \times NEG \times RET$	0	0.033
		(0.72)
$PRE_4 \times NEG \times RET$	0	0.006
		(0.14)
POST × NEG × RET	**–**	**-0.234****
		(-2.54)
POST, *POST* × *NEG*, and *POST* × *RET*		Yes
PRE_τ, $PRE_\tau \times NEG$, and $PRE_\tau \times RET$		Yes
Year Fixed Effects (Main)		Yes
Firm Fixed Effects (Main)		Yes
Year Fixed Effects (Basu Coefficients)		Yes
State Fixed Effects (Basu Coefficients)		Yes
Observations		32,418
R^2		0.562

This table presents regression results of earnings on stock return, negative stock return dummy, post director liability law dummy, dummies for one to four years before law enactment, and their interaction terms. The three terms of the Basu model (i.e., *NEG*, *RET*, *NEG* × *RET*) are subsumed by the fixed effects structure, thus their coefficients are not reported. Variables are defined in the Appendix. All continuous variables are winsorized at the extreme percentiles. *, **, *** indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively, for a two-tailed test; *t*-statistics are reported in parentheses and are based on standard errors clustered at the state level.