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ARTICLE

Employee firing costs and accounting conservatism: Evidence from wrongful discharge laws

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Abstract

This paper identifies the causal effect of a firm's employee firing costs on its conditional conservatism, using the staggered adoption of US state wrongful discharge laws (WDLs) that increase a firm's cost of firing employees. We find that the adoption of WDLs leads to a significant increase in conditional conservatism. This result is greater for firms that are more labor-intensive, have higher propensities to fire employees, make more firm-specific investments and have greater risk. Overall, our findings support the view that higher firing costs lead to greater demand for conditional conservatism to decrease investment inefficiencies because higher firing costs make inefficient investments (including overinvestment in negative-net present value (NPV) projects and delays in disinvesting poorly performing projects) costlier for the firm.

KEYWORDS
conditional conservatism, firing costs, investment inefficiency, wrongful discharge laws

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G30, J63, M41

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1 | INTRODUCTION

Understanding the determinants of firms' accounting choices is an important goal in accounting research. Although labor is one of the key inputs of a firm's operation, how labor market frictions affect firms' accounting choices is relatively under-examined. In this article, we shed light on this strand of research by investigating the effect of employee firing costs on conditional conservatism.

Our study is based on the staggered adoption of state-level wrongful discharge laws (WDLs), which significantly increase firms' employee firing costs. We expect the adoption of WDLs in a state to increase conditional conservatism for firms located in that state for the following reasons. WDL makes inefficient investments (i.e., overinvestment in *ex-ante* negative-NPV projects and delays in disinvesting *ex-post* poorly performing investment projects) costlier for the firm because when the bad outcomes finally accumulate and losses need to be recognized, these losses will also include the firing costs, and WDLs will enhance such costs. Therefore, considering that conditional conservatism is effective in decreasing investment inefficiencies (Ball & Shivakumar, 2005; Bushman et al., 2011; Balakrishnan et al., 2016; García Lara et al., 2016), the demand for conditional conservatism is expected to increase following the state's adoption of WDL.

The staggered adoption of WDLs provides a good opportunity to examine the causal effect of employee firing costs on conditional conservatism for two reasons. First, US states pass WDLs in order to protect the interests of employees by encroaching on the employment-at-will doctrines. As the adoption of WDLs is not related to accounting conservatism, it is likely an exogenous shock. Second, since several states adopted WDLs in different years, we can provide clearer identification than the studies based on a single shock.

Based on a sample of 95,599 firm-year observations from 1969 to 2003, we find that the adoption of WDLs leads to an increase in firms' conditional conservatism. The results are consistent with the view that higher firing costs lead to greater demand for conditional conservatism after the adoption of WDLs because conservative accounting policies can help reduce firms' investment inefficiencies.

The essential assumption for our difference-in-differences model is the parallel trend assumption: Treatment and control firms have similar trends in conditional conservatism before the adoption of WDLs. Our parallel trends analysis indicates that the two groups indeed have similar trends in conditional conservatism prior to the passage of WDLs. Further, the influence of WDLs on firms' conditional conservatism appears after the law adoption, which supports our causal interpretation.

One possible concern is that the adoption of WDLs is correlated with local business conditions, which influence firms' accounting choices. To alleviate this problem, we focus on a subsample of treatment firms and control firms near the state borders. Considering that companies in such proximity share similar economic conditions, if our results are driven by local business conditions (rather than WDLs), we should expect no effect in this subsample. However, we still find a significantly positive effect of WDLs on conditional conservatism for treated firms on one side of the state border relative to their neighboring control firms on the other side of the state border.

We further investigate the cross-sectional variation of the treatment effect. We find that the positive effect of WDLs on conditional conservatism is more pronounced for firms that are more labor-intensive, have a higher propensity to fire workers, make more firm-specific investments and have higher risk. These results provide further evidence that the effect of WDLs on conditional conservatism is indeed related to firms' hiring costs.

Finally, we perform a number of robustness checks on our main findings: using other conservatism measures, controlling for the confounding effect of cost stickiness, implementing alternative difference-in-differences methods, addressing the changes in accounting standards during our sample period and conducting a placebo test. The positive effect of WDLs on conditional conservatism remains.

Our paper makes at least three contributions to the literature. First, it contributes to the literature on the relation between accounting conservatism and investment decisions. Bushman et al. (2011) show that the sensitivity of investment to declining investment opportunities is increasing in country-level accounting conservatism, supporting the view that accounting conservatism disciplines managers to avoid negative NPV projects. In the setting of mergers and acquisitions, Francis and Martin (2010) find that accounting conservatism helps the bidders achieve higher acquisition profitability. García Lara et al. (2016) document that accounting conservatism contributes to increasing investment efficiency by reducing both the under-investment and over-investment problems. Louis et al. (2012) argue that accounting conservatism can mitigate the value destruction associated with cash holdings. Louis and Urcan (2015) provide evidence that accounting conservatism helps reduce dividends because accounting conservatism can mitigate managers' incentives to engage in value-destroying projects and thus makes dividend payment less necessary. Although these studies help us better understand the impact of accounting conservatism on firm investment decisions, the role of the potential cost associated with inefficient projects is less studied. Intuitively, if an inefficient investment project is costlier, there should be a greater demand for accounting conservatism to avoid such a project (including avoiding this project *ex-ante* and disinvesting this project *ex-post*). Our paper complements this strand of literature by identifying the causal effect of firing costs (an important source of costs that firms need to face when dealing with inefficient investment projects) on conditional conservatism.

Second, our study contributes to the strand of literature that studies the economic consequences of adopting WDLs. On one hand, such labor regulations create some (unexpected) negative externalities such as reducing employment (Autor et al., 2006; Dertouzos & Karoly, 1992) and decreasing productivity and performance (Autor et al., 2007; Bird & Knopf, 2009). On the other hand, this regulation may have positive externalities such as spurring innovation and entrepreneurship (Acharya et al., 2014). We add to this literature by providing evidence on another positive externality: This regulation affects financial reporting and leads to greater accounting conservatism.

Third, our research adds to the literature that examines the effect of regulatory changes on accounting conservatism. Andre et al. (2014) document a decline in the level of firms' conditional conservatism after the adoption of the International Financial Reporting Standards (IFRS) by European countries. Cheng et al. (2017) examine the impact of antitakeover laws on accounting conservatism. They find that firms have a lower level of conservatism when the states have adopted more antitakeover laws. Burke et al. (2019) show that import competition is positively associated with firms' conditional conservatism after trade liberalization. Huang (2021) studies the staggered passage of the Interstate Banking and Branching Efficiency Act and finds that firms report less conservatively after the passage of the act. Chen et al. (2021) show that the adoption of universal demand laws that restrict shareholder litigation rights decreases firms' accounting conservatism. Complementing this literature (which mainly focuses on regulatory changes in the financial market), we show that labor laws related to employee firing costs have a significant effect on firms' conditional conservatism.

The remainder of the paper is organized as follows. Section 2 reviews the background on WDL and develops our hypothesis. Section 3 describes our sample and key variable construction. Section 4 presents the empirical results. We conclude in Section 5.

2 | INSTITUTIONAL BACKGROUND AND HYPOTHESIS DEVELOPMENT

There is a long-established precedent in the United States that employees can be fired at will, whether there exists a good cause, a bad cause or no cause at all (Autor et al., 2006). There have been repeated calls since the peak of employment in the early 20th century for protecting employees' interests by reducing this discretion of employers. As a result, some states passed common law exceptions to the employment-at-will doctrines beginning in the 1970s. These so-called "WDLs" are typically classified into three categories: (1) the implied contract exception; (2) the public policy exception and (3) the good faith exception.

The implied contract exception is related to the implied promise by employers that employees cannot be discharged without proper cause. The public policy exception restricts employers from firing workers for reasons that contravene a statutory public policy. The good faith exception centers on the legal theory that upon hiring, employers and employees enter into an agreement of good faith and fair dealing.

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TABLE 1 The adoption years of wrongful discharge laws (WDLs) by state

State	Adoption year
New Hampshire	**1974 (reversed in 1980)**
Massachusetts	1977
California	1980
Connecticut	1980
Montana	1982
Alaska	1983
Arizona	1985
Oklahoma	1985 (reversed in 1989)
Nevada	1987
Idaho	1989
Utah	1989
Delaware	1992
Wyoming	1994
Louisiana	1998

Note: This table lists the adoption year of the good faith exception by state.

Following Serfling (2016), our study is based on the good faith exception, as this exception has the most significant impact. First, at-will employment is largely undermined by the good faith exception based on the implication that termination can only happen with just cause (Dertouzos & Karoly, 1992). Second, the good faith exception has a stronger application than the other exceptions. Under the good faith exception, employees are entitled to recovery of contractual losses as well as compensation due to emotional and punitive damages. Table 1 presents the detailed adoption years of the good faith exception by states from 1974 to 1998.

Our central assumption is that the adoption of WDLs increases employee firing costs. A natural question arises: Is discharging employees because of the poor performance of the project they work for considered a wrongful discharge by WDLs? Under ordinary circumstances, employee layoffs due to business failure do not give rise to a claim of wrongful termination (Strong, 1989). However, employers are still subject to wrongful termination claims if they use layoffs as an excuse to selectively fire certain employees for reasons unrelated to the economic necessity of the dismissals. For example, in the case of *Coelho v. Posi-Seal International, Inc.*, 208 Conn. 106, 544 A.2d 170, 544 A. 2 (1988), employees were fired as part of layoffs that the employer claimed to be motivated by poor firm performance. However, the plaintiff claimed that the layoffs were an excuse and that he was discharged as a result of a dispute with a manager of a different division. The defendant argued that termination due to a reduction in workforce is, as a matter of law, a just cause. The court ruled in favor of the plaintiff, as the court concluded that an employer's claim that some employees were terminated as a result of poor firm performance does not necessarily establish that all employees were discharged for the same reason. Further, the court also pointed out that employers may not use a reduction in the workforce as a pretext for discharges that would otherwise be subject to a just-cause attack by the employee.

Thus, as summarized by Serfling (2016), while economically motivated layoffs may reduce the risk of wrongful termination lawsuits, they do not eliminate this risk. In addition to the direct legal costs, WDLs also impose substantial indirect costs (Autor, 2003): The threat of litigation will induce employers to take avoidance actions like limiting the discretion of managers to hire and fire employees, instigating bureaucratic procedures for documenting and terminating employment and potentially retaining unproductive workers who would otherwise be fired.

We expect that a state's adoption of WDLs results in an increase in conditional conservatism because WDLs increase firms' vulnerability to inefficient investments. Constrained by the employee protection associated with

WDLs, firms cannot easily discharge employees and divest poorly performing projects to cover cash flow shortfalls once a bad investment decision is made. In this case, a bad investment is particularly costly to firms (Autor et al., 2006; Bentolila & Bertola, 1990).

Conservative accounting policies impose a higher recognition standard to record gains relative to losses, and such asymmetric verifiability speeds up the recognition of losses and pushes managers to acknowledge and report problems earlier and invest greater effort in solving interim problems of investment (Bushman et al., 2011; Roychowdhury, 2010). Such a timely recognition of loss prevents managers from taking negative net present value (NPV) projects *ex-ante* and enables firms to eliminate negative-NPV projects more promptly *ex-post* (Ball & Shivakumar, 2005; Francis & Martin, 2010; Watts, 2003). Considering that WDLs increase firms' costs for inefficient investments, the demand for accounting conservatism to avoid such investments will be enhanced accordingly.

In summary, we expect a positive effect of WDLs on firms' conditional conservatism because WDLs make it costlier for the firm to invest inefficiently (including overinvestment in negative-NPV projects *ex-ante* and delays in disinvesting poorly performing projects *ex-post*), and therefore there is a greater demand for conditional conservatism to avoid such inefficient investments.

3 | SAMPLE AND MODEL

3.1 | Research sample

We start with all non-financial US public firms in the Compustat database from 1969 to 2003, which spans the 5 years before the earliest adoption of the good faith exception in New Hampshire in 1974 to the 5 years after the latest adoption in Louisiana in 1998. We collect firms' headquarters information from Compustat and Compact Disclosure (which records historical changes of firms' headquarters). Finally, we require that all firm-year observations have available data for the regression analysis. Our final sample consists of 95,599 firm-year observations.

3.2 | Regression model

Basu (1997) constructs a piecewise linear regression to measure conditional conservatism. Although the Basu model is widely used in the conservatism literature, some studies cast doubt on the validity of this model to capture conditional conservatism. Dietrich et al. (2007) argue that the asymmetric timeliness regression induces bias in the test statistics that can be interpreted as evidence of conditional conservatism. Patatoukas and Thomas (2011) demonstrate that return variance and loss effects generate biases in Basu's conservatism measure. Patatoukas and Thomas (2016) further explain that such biases are due to the fact that earnings, accruals and other performance measures are related to the second and higher moments of returns. Dutta and Patatoukas (2017) point out that the asymmetric timeliness coefficient in the Basu model can be positive even in the absence of conditional conservatism.

Facing this criticism, some researchers provide solutions to the problems identified in the Basu model. Ball et al. (2013a, 2013b) argue that bias in the asymmetric timeliness measure can be eliminated by using the unexpected components of earnings and returns. Further, Collins et al. (2014) show that bias can be eliminated by employing the accrual component of earnings in the Basu model. Banker et al. (2016) suggest that incorporating cost stickiness in the Basu model can control for the confounding effect of cost stickiness. Recent research by Badia et al. (2021) supports that the known biases in Basu's asymmetric timeliness measure can be largely eliminated after adopting several modifications to the Basu regression.

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TABLE 2 Descriptive statistics

Variable	Mean	Std. dev	P25	Median	P75
UE	0.009	0.164	−0.022	0.016	0.061
WDL	0.201	0.401	0.000	0.000	0.000
UR	−0.205	0.644	−0.614	−0.254	0.088
D	0.694	0.461	0.000	1.000	1.000
Risk	0.140	0.083	0.083	0.121	0.173
MB	2.378	3.263	0.884	1.489	2.628
C-Score	0.137	0.202	0.016	0.130	0.245

Note: This table presents the descriptive statistics of variables. Variable definitions are in the Appendix.

Following Badia et al. (2021), we employ the following modified Basu model to examine the impact of WDLs on firms' conditional conservatism:[1]

$$UE_t = a_1 + a_2 WDL_t + a_3 UR_t + a_4 D_t + a_5 UR_t \times D_t + a_6 UR_t \times WDL_t + a_7 D_t \times WDL_t + a_8 UR_t \times D_t \times WDL_t + Firm\ FE + Year\ FE + \varepsilon. \quad (1)$$

The dependent variable *UE* denotes unexpected earnings, which is computed as annual earnings minus expected annual earnings, scaled by the beginning market value of equity. Expected annual earnings are estimated by the expectation model of Ball et al. (2013a). The indicator variable *WDL* takes the value of one if a firm's headquarter state passes the good faith exception and zero otherwise. *UR* denotes unexpected returns, defined as annual returns minus expected annual returns for the fiscal year. Expected annual returns are the value-weighted average return for the applicable portfolio in 25 portfolios formed each fiscal year by first sorting firms into quintiles based on the beginning market value of equity and then sorting each of these quintiles into quintiles based on the beginning book-to-market equity ratio. *D* is an indicator variable, taking the value of one when *UR* is negative and zero otherwise. The regression includes firm and year fixed effects. Throughout the paper, we report the *p*-values based on robust standard errors clustered by state.

Following Basu and Liang (2019), equation (1) includes no control variables. As a robustness test, we add the return variance (*Risk*) and the market-to-book ratio (*MB*) in the regression, following Li and Xu (2018) and Kim (2020). Further, as another robustness test, we construct *C-Score* to measure the degree of firms' conditional conservatism, following Khan and Watts (2009). The Appendix provides the details of the variable definition.

Our coefficient of interest is α_8. After including firm and year fixed effects in the regression, α_8 is an estimation of within-firm differences in firms' conditional conservatism before and after the adoption of WDLs, compared to the differences in states that did not experience the law change during the same time (Imbens & Wooldridge, 2009).

Table 2 reports the descriptive statistics of variables. The mean value of *UE* is 0.009. Approximately 20% of the firm-year observations adopt the good faith exception. The mean value of *UR* is −0.205. About 70% of the firm-year observations have negative unexpected returns. The average return variance is 0.140, and the average market-to-book ratio is 2.378. The mean value of *C-Score* is 0.137.

[1] Another prevalent asymmetric timeliness measure is *C-Score*, developed by Khan and Watts (2009). However, researchers have different views about this measure. On the one hand, Byzalov and Basu (2021) point out that *C-Score* cannot capture new sources of conservatism variation and is often the artifact of arbitrary technical assumptions. They suggest using the Basu model directly to avoid these problems. On the other hand, Ettredge et al. (2012) show that *C-Score* captures well the expected firm-level variation in conditional conservatism. García Lara et al. (2020) provide evidence that *C-Score* seems to rank firms properly according to their conditional conservatism level.

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4 | EMPIRICAL RESULTS

4.1 | Baseline regression

Table 3 presents the results from estimating equation (1).[2] In Column 1, the coefficient on *UR*×*D*×*WDL* is 0.0489 and significant at the 5% level, suggesting a positive effect of WDLs on the firm's conditional conservatism. The economic magnitude is also sizeable: Compared to the average conditional conservatism of our sample period, the coefficient indicates a 26.7% increase in conservatism. The results provide evidence that the increase in firing costs leads to greater demand for accounting conservatism after the adoption of WDLs since conservative accounting policies can help reduce firms' investment inefficiencies. In Column 2, the regression includes the return variance (*Risk*), the market-to-book ratio (*MB*) and their interaction terms. The coefficient on *UR*×*D*×*WDL* is still positive and significant, supporting that the adoption of WDLs increases firms' conditional conservatism.

The results of the control variables show that the coefficient on *UR*×*D*×*MB* is negative and significant, consistent with prior literature (e.g., Badia et al., 2021; Ramalingegowda & Yu, 2012).

Overall, the results show that a state's adoption of WDLs leads to a significant increase in firms' conditional conservatism.

4.2 | Pre-treatment trends

The parallel trends assumption is crucial to the difference-in-differences estimation. That is, a treated firm should have a similar trend in conditional conservatism as a control firm before the adoption of WDLs. We define five indicator variables: $WDL^{-3\&-4}$, $WDL^{-1\&-2}$, WDL^{0}, $WDL^{1\&2}$ and WDL^{3+}. WDL^{0} indicates the year in which the good faith exception is enacted; $WDL^{-3\&-4}$ indicates the third and fourth years before the good faith exception enactment; $WDL^{-1\&-2}$ indicates the first and second years before the good faith exception enactment; $WDL^{1\&2}$ indicates the first and second years after the good faith exception enactment and WDL^{3+} indicates 3 or more years after the good faith exception enactment. Subsequently, we re-estimate our baseline regression by replacing the *WDL* variable with the five indicator variables above.

As reported in Table 4, the coefficients on $UR\times D\times WDL^{-3\&-4}$ and $UR\times D\times WDL^{-1\&-2}$ are not statistically significant, indicating that the parallel trend assumption is not violated. The coefficient on $UR\times D\times WDL^{1\&2}$ is 0.0990 and significant at the 1% level. Further, the coefficient on $UR\times D\times WDL^{3+}$ is significantly positive. The results indicate that the impact of WDLs on firms' conditional conservatism takes place after the WDLs' adoption, suggesting a causal effect.

4.3 | Confounding effects from local economy conditions

The adoption of WDLs and firms' conditional conservatism may be jointly determined by local economic factors. To mitigate this concern, we focus on a subsample of treatment firms and control firms in the neighboring states near the state line. Considering that firms on each side of state lines usually face similar economic conditions (Heider & Ljungqvist, 2015), if local economic conditions drive both the passage of WDLs and firms' conservative accounting policies, we would find no differences in conditional conservatism between treatment firms and their nearby control firms just across the state border.

We match each firm in the treated state to a control firm that is in an adjacent state that has not adopted WDLs, is in the same industry (two-digit Standard Industrial Classification (SIC) code) and is closest in distance to the treated firm. To ensure that the treated and matched control firms are indeed physically close to each other, we further impose

[2] The three terms of the Basu model (i.e., *UR*, *D*, *UR*×*D*) are subsumed by the firm and year fixed effects structure.

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TABLE 3 The impact of WDLs on conditional conservatism

	(1)	(2)
WDL	−0.0027	−0.0016
	(0.697)	(0.828)
UR×*WDL*	−0.0184	−0.0140
	(0.519)	(0.640)
D×*WDL*	0.0188	0.0182
	(0.106)	(0.131)
UR*×*D*×*WDL	0.0489**	0.0441*
	(0.050)	(0.089)
Risk		0.1047***
		(0.004)
UR×*Risk*		0.0515
		(0.320)
D×*Risk*		−0.0088
		(0.870)
UR×*D*×*Risk*		0.0652
		(0.213)
MB		−0.0006
		(0.263)
UR×*MB*		0.0012
		(0.108)
D×*MB*		0.0017**
		(0.012)
UR×*D*×*MB*		−0.0058***
		(0.000)
Constant	0.0007***	0.0007***
	(0.000)	(0.000)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	95,599	95,599
R-squared	0.222	0.231

Note: This table presents the impacts of WDLs on firms' conditional conservatism. The dependent variable *UE* denotes unexpected earnings, which is defined as annual earnings minus expected annual earnings, scaled by the beginning market value of equity. The indicator variable *WDL* takes the value of one if a firm's headquarter state has passed the good faith exception, and zero otherwise. *UR* denotes unexpected returns, defined as annual returns minus expected annual returns for the fiscal year. *D* is an indicator variable taking the value of one when *UR* is negative and zero otherwise. Variable definitions are provided in the Appendix. *p*-values based on robust standard errors clustered by state are reported in parentheses.
The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

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TABLE 4 Testing for pre-treatment trends

	(1) Coefficient	(2) p-value
$WDL^{-3\&-4}$	−0.0023	(0.698)
$UR\times WDL^{-3\&-4}$	0.0482	(0.203)
$D\times WDL^{-3\&-4}$	0.0103	(0.181)
$\boldsymbol{UR\times D\times WDL^{-3\&-4}}$	−0.0233	(0.401)
$WDL^{-1\&-2}$	0.0060	(0.373)
$UR\times WDL^{-1\&-2}$	0.0036	(0.806)
$D\times WDL^{-1\&-2}$	−0.0016	(0.849)
$\boldsymbol{UR\times D\times WDL^{-1\&-2}}$	0.0253	(0.328)
WDL^{0}	−0.0005	(0.949)
$UR\times WDL^{0}$	−0.0080	(0.468)
$D\times WDL^{0}$	−0.0088	(0.458)
$\boldsymbol{UR\times D\times WDL^{0}}$	0.0099	(0.710)
$WDL^{1\&2}$	−0.0158**	(0.031)
$UR\times WDL^{1\&2}$	0.0078	(0.536)
$D\times WDL^{1\&2}$	0.0206**	(0.010)
$\boldsymbol{UR\times D\times WDL^{1\&2}}$	0.0990***	(0.000)
WDL^{3+}	0.0099**	(0.047)
$UR\times WDL^{3+}$	−0.0184	(0.160)
$D\times WDL^{3+}$	0.0184**	(0.018)
$\boldsymbol{UR\times D\times WDL^{3+}}$	0.0862***	(0.001)
Constant	0.0004***	(0.001)
Firm fixed effect	Yes	
Year fixed effect	Yes	
Observations	95,599	
R-squared	0.222	

Note: This table examines pre-treatment trends between treated firms and control firms. WDL^{0} indicates the year of the good faith exception enactment; $WDL^{-3\&-4}$ indicates the third and fourth years before the good faith exception enactment; $WDL^{-1\&-2}$ indicates the first and second years before the good faith exception enactment; $WDL^{1\&2}$ indicates the first and second years after the good faith exception enactment and WDL^{3+} indicates 3 or more years after the good faith exception enactment. Variable definitions are provided in the Appendix. *p*-values based on robust standard errors clustered by state are reported in parentheses.
The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

a condition that the distance between the pair be within a given range (e,g., 40–100 miles). Otherwise, this pair is excluded from our analysis. We then re-estimate our baseline regression based on this matched subsample.

Table 5 reports the results. In Columns 1–5, we require that the distance between the treated firm and its matched control firm be no more than 40, 50, 60, 80 and 100 miles, respectively. We find a significant and positive coefficient on *UR×D×WDL* through all columns, supporting that firms' conditional conservatism significantly increases following the adoption of WDLs. In summary, these results indicate that our results are unlikely driven by (unobserved) local economy conditions.

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TABLE 5 Analysis of treatment firms and neighboring control firms

	(1) 40 miles	(2) 50 miles	(3) 60 miles	(4) 80 miles	(5) 100 miles
WDL	−0.0063	0.0018	0.0048	0.0051	0.0086
	(0.697)	(0.876)	(0.622)	(0.355)	(0.243)
UR×WDL	−0.0037	−0.0326	−0.0381	−0.0399	−0.0478
	(0.925)	(0.436)	(0.346)	(0.187)	(0.107)
D×WDL	0.0241**	0.0191***	0.0134***	0.0072	0.0006
	(0.029)	(0.005)	(0.001)	(0.308)	(0.948)
UR×D×WDL	0.0810*	0.1024**	0.0986**	0.0785**	0.0758***
	(0.082)	(0.021)	(0.018)	(0.014)	(0.004)
Constant	0.0014**	0.0011*	0.0011*	0.0011**	0.0012**
	(0.014)	(0.051)	(0.055)	(0.038)	(0.021)
Firm fixed effect	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes
Observations	6107	7914	8956	11,034	11,912
R-squared	0.183	0.209	0.219	0.211	0.212

Note: This table presents the analysis of treatment firms and control firms in the neighboring states. Each treatment firm is matched to a control firm in the neighboring state, which is in the same industry (based on two-digit SIC code) and has the shortest distance to the treatment firm. Columns 1–5 require that the distance between the treatment firm and the control firm be within 40, 50, 60, 80 and 100 miles, respectively. Variable definitions are provided in the Appendix. *p*-values based on robust standard errors clustered by state are reported in parentheses.
The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

4.4 | Cross-sectional variation of treatment effects

To further support the view that the effect of WDLs on firms' conditional conservatism is indeed related to firing costs, we implement the cross-sectional test, which helps mitigate the concern that our results are driven by some omitted variables (Gao et al., 2018; Raddatz, 2006).

First, if the increased conditional conservatism after the adoption of WDLs is due to increased costs of firing employees, we expect that the treatment effect is stronger in firms that are labor-intensive. Following Agrawal and Matsa (2013), we estimate the labor intensity as the ratio of firms' labor expense to sales. We calculate the labor intensity measure at the industry level by averaging the firm data in an industry and classify an industry as high labor intensity if its ratio of labor expense to sales is above the median value. Then we re-estimate our baseline regression on the subsamples divided by labor intensity. The results are reported in Panel A of Table 6. In the high labor intensity subsample, the coefficient on *UR×D×WDL* is 0.0581 and significant at the 1% level. In contrast, in the subsample of low labor intensity, the coefficient on *UR×D×WDL* is insignificant. These results provide evidence that the effect of WDLs on conditional conservatism is greater for firms that are more labor-intensive.

Second, firms in industries that fire workers more frequently likely depend more on layoffs to cut costs. Thus, we expect WDLs to have a greater impact on conditional conservatism of firms in these industries. To test this prediction, we measure each industry's employee layoff propensity following Serfling (2016). Specifically, we compute the fraction of firms in each industry and year that reduce their employees by at least 5% and take the average of this measure over the previous 5 years. We then define a firm as having high layoff propensity if its employee layoff propensity is above the sample median. In Panel B of Table 6, we re-estimate our baseline regression on the subsamples formed based on layoff propensity. Column 1 shows that the coefficient on *UR×D×WDL* is 0.0887 and significant at the 5% level in

TABLE 6 Cross-sectional variation of treatment effects

Panel A. Labor intensity		
	High labor intensity	Low labor intensity
	(1)	(2)
WDL	0.0080*	−0.0191*
	(0.092)	(0.100)
UR×WDL	−0.0460**	0.0211
	(0.048)	(0.572)
D×WDL	0.0104	0.0307
	(0.132)	(0.134)
UR×D×WDL	0.0581***	0.0341
	(0.000)	(0.431)
Constant	0.0006***	0.0007***
	(0.000)	(0.000)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	57,039	38,560
R-squared	0.220	0.229
Panel B. Layoff propensity		
	High layoff propensity	Low layoff propensity
	(1)	(2)
WDL	−0.0214**	−0.0006
	(0.021)	(0.932)
UR×WDL	0.0106	0.0030
	(0.638)	(0.805)
D×WDL	0.0545***	0.0069
	(0.002)	(0.411)
UR×D×WDL	0.0887**	0.0285
	(0.011)	(0.427)
Constant	0.0008*	−0.0008*
	(0.067)	(0.060)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	47,253	48,346
R-squared	0.279	0.298
Panel C. Firm-specific Investment		
	High firm-specific investment	Low firm-specific investment
	(1)	(2)
WDL	−0.0016	−0.0029
	(0.875)	(0.573)
UR×WDL	−0.0285	−0.0114
	(0.288)	(0.722)

(Continues)

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TABLE 6 (Continued)

Panel C. Firm-specific Investment	High firm-specific investment	Low firm-specific investment
	(1)	(2)
D×WDL	0.0292*	0.0116
	(0.063)	(0.278)
UR×D×WDL	0.0812***	0.0280
	(0.001)	(0.301)
Constant	−0.0001	0.0012***
	(0.827)	(0.000)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	41,143	54,456
R-squared	0.257	0.256

Panel D. Firm risk	High firm risk	Low firm risk
	(1)	(2)
WDL	−0.0024	0.0042
	(0.847)	(0.486)
UR×WDL	−0.0237	−0.0219
	(0.258)	(0.340)
D×WDL	0.0204	0.0125
	(0.156)	(0.156)
UR×D×WDL	0.0765*	0.0387
	(0.055)	(0.172)
Constant	−0.0024	0.0042
	(0.847)	(0.486)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	47,234	48,365
R-squared	0.290	0.334

Note: This table presents the cross-sectional variation of the treatment effect. Panels A–D report the subsample analysis based on the sample median value of labor intensity, layoff propensity, firm-specific investment and firm risk, respectively. Variable definitions are provided in the Appendix. *p*-values based on robust standard errors clustered by state are reported in parentheses.
The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

the subsample of firms with high layoff propensity. In contrast, Column 2 reports that the coefficient on *UR×D×WDL* is insignificant in the subsample of firms with low layoff propensity. Consistent with our expectation, the impact of WDLs on conditional conservatism is more pronounced for firms with a higher propensity to fire employees.

Third, according to Klein et al. (1978), constraints on firing employees are costlier when firms have large amounts of firm-specific investments because such investments have a lower value in alternative uses and are harder to sell when firms face unfavorable investment outcomes. Thus, the adoption of WDLs should have a greater influence on

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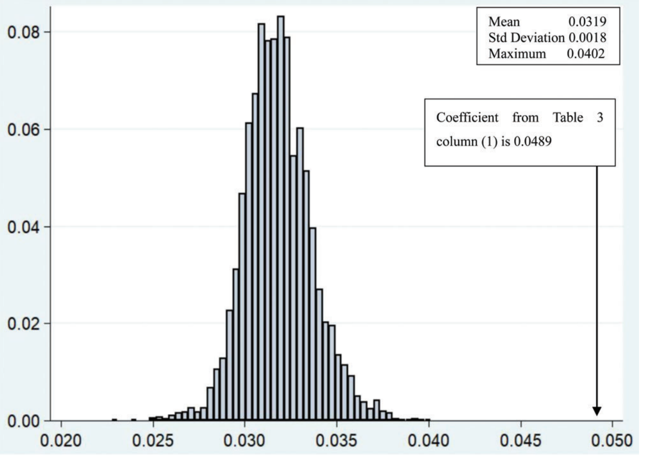

FIGURE 1 Placebo tests. This figure presents the distribution of the coefficients on *UR×D×WDL* from 5000 bootstrap simulations. For each state adopting the wrongful discharge laws (WDLs), we randomly assign a pseudo-event year from 1969 to 2003. We require that the pseudo-event year should be at least 5 years before the actual adoption year or at least 5 years after the actual adoption year.

conditional conservatism when firms make more firm-specific investments. Following Raman and Shahrur (2008), we measure firm-specific investment by the ratio of firms' (R&D and advertisement expenditures to sales. A firm is classified as having high firm-specific investments if its ratio of R&D and advertisement expenditures to sales is above the sample median. Panel C of Table 6 reports the results from our baseline regression for subsamples of high and low firm-specific investments. In Column 1, the coefficient on *UR×D×WDL* is 0.0812 and significant at the 1% level in the subsample of firms with high firm-specific investments. However, in Column 2, the interaction of *UR×D×WDL* generates an insignificant coefficient in the subsample of firms with low firm-specific investments. These results suggest that the treatment effect of WDLs on conditional conservatism is stronger for firms that make more firm-specific investments.

Last, the flexibility of adjusting employment is more important for firms facing higher uncertainty (Agrawal and Matsa, 2013; Cuñat & Melitz, 2012). Therefore, we predict a larger increase in conditional conservatism for firms with higher risk after the adoption of WDLs. We define a firm as having high risk if its return variance is above the sample median. We then re-estimate our baseline regression on two subsamples grouped by firm risk. Panel D of Table 6 reports the regression results. In the high-risk subsample, the coefficient on *UR×D×WDL* is positive and significant. In contrast, in the subsample of low risk, the coefficient on *UR×D×WDL* is insignificant. The results show a greater impact of WDLs on conditional conservatism for firms with higher risk.

Taken together, the effect of WDLs on conditional conservatism is stronger for firms that are more labor-intensive, for firms that are more likely to discharge employees, for firms that make more firm-specific investments and for firms that have a greater risk. These results indicate that the impact of WDLs on conditional conservatism is indeed tied to employee firing costs.

4.5 | Placebo test

We conduct a placebo test to examine the possibility that our results are driven by chance. In particular, for each state that adopts WDLs, we randomly choose a year during our sample period as the pseudo-event year. To ensure that the pseudo-event year is not confounded with the actual event year, we require that the pseudo-event year be either at least 5 years prior to the actual adoption year or at least 5 years after the actual adoption year. We then re-estimate our baseline regression based on those pseudo-event years and save the corresponding coefficients on *UR×D×WDL*. This procedure is repeated 5000 times.

We report the distribution of the coefficients estimated from these pseudo-events in Figure 1. The coefficient reported in Column 1 of Table 3 (0.0489) is greater than the maximum coefficient (0.0402). These results indicate that the adoption of WDLs (rather than any random events) leads to our main finding.

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TABLE 7 Other conservatism measure

	(1)	(2)
WDL	0.0104**	0.0088*
	(0.030)	(0.055)
Risk		0.0801***
		(0.000)
MB		−0.0131***
		(0.000)
Constant	0.0262***	0.0657***
	(0.000)	(0.000)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	95,362	95,362
R-squared	0.608	0.631

Note: This table presents the regression results of using *C-score* as an alternative measure of conditional conservatism. The dependent variable *C-Score* is estimated following Khan and Watts (2009). The indicator variable *WDL* takes the value of one if a firm's headquarter state has passed the good faith exception and zero otherwise. Variable definitions are provided in the Appendix. *p*-values based on robust standard errors clustered by state are reported in parentheses.
The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

4.6 | Robustness check and additional investigation

4.6.1 | Alternative measure of conservatism

As a robustness test, we construct *C-Score* following Khan and Watts (2009) and estimate the following difference-in-differences model:

$$C - Score_t = b_1 + b_2 WDL_t + b_3 Risk_t + b_4 MB_t$$
$$+ Firm\ FE + Year\ FE + \varepsilon. \tag{2}$$

The dependent variable is the firm-specific asymmetric timeliness measure *C-Score*. Other variables are defined as before. The regression includes firm and year fixed effects.

Table 7 presents the regression results. Column 1 reports the results where we only include *WDL*, firm fixed effect and year fixed effect. The coefficient on *WDL* is 0.0104 and significant at the 5% level, suggesting a positive effect of WDLs on the firm's conditional conservatism. In Column 2, we include control variables of *Risk* and *MB* in the regression. The coefficient on *WDL* is still significantly positive, providing further evidence that a state's adoption of WDLs leads to a significant increase in firms' conditional conservatism.

4.6.2 | Confounding effect of cost stickiness

Banker et al. (2016) find that the standard estimate of conditional conservatism by the Basu model may be biased due to sticky costs. To control for the confounding effect of cost stickiness, we incorporate sticky costs in the Basu model

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TABLE 8 Control for the confounding effect of cost stickiness

	(1)
WDL	0.0009
	(0.909)
UR×WDL	–0.0288
	(0.345)
D×WDL	0.0151
	(0.217)
UR×D×WDL	0.0471*
	(0.077)
△*Sale×WDL*	0.0166***
	(0.000)
DS×WDL	–0.0142***
	(0.000)
△*Sale×DS×WDL*	–0.0004
	(0.955)
Constant	0.0012***
	(0.007)
Firm fixed effect	Yes
Year fixed effect	Yes
Observations	95,599
R-squared	0.228

Note: This table presents the results of controlling for the confounding effect of cost stickiness. The dependent variable *UE* denotes unexpected earnings, defined as annual earnings minus expected annual earnings, scaled by the beginning market value of equity. The indicator variable *WDL* takes the value of one if a firm's headquarter state has passed the good faith exception and zero otherwise. *UR* denotes unexpected returns, defined as annual returns minus expected annual returns for the fiscal year. *D* is an indicator variable taking the value of one when *UR* is negative and zero otherwise. △*Sale* denotes sales change from year $t-1$ to year t, scaled by the beginning market value of equity. *DS* is an indicator variable taking the value of one if firms' sales decrease in a year and zero otherwise. Variable definitions are provided in the Appendix. *p*-values based on robust standard errors clustered by state are reported in parentheses.
The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

as follows:

$$
\begin{aligned}
UE_t = c_1 &+ c_2 WDL_t + c_3 UR_t + c_4 D_t + c_5 UR_t \times D_t + c_6 UR_t \times WDL_t \\
&+ c_7 D_t \times WDL_t + c_8 UR_t \times D_t \times WDL_t + c_9 \Delta Sale_t + c_{10} DS_t \\
&+ c_{11} \Delta Sale \times DS_t + c_{12} \Delta Sale \times WDL_t + c_{13} DS \times WDL_t \\
&+ c_{14} \Delta Sale \times DS \times WDL_t + Firm\ FE + Year\ FE + \varepsilon.
\end{aligned}
\tag{3}
$$

△*Sale* is the sales change from year $t-1$ to year t, scaled by firm market value at the beginning of the year. *DS* is an indicator variable taking the value of one if a firm's sales decrease relative to the previous year and zero otherwise. Other variables are defined as before.

Table 8 reports the regression results.[3] The coefficient on *UR×D×WDL* is positive and significant, indicating that the level of conditional conservatism increases after firms' states pass WDLs. The results suggest a positive effect of WDLs on the firms' conditional conservatism after controlling for the confounding effect of cost stickiness.

[3] The three terms of Δ*Sale*, *DS* andΔ*Sale×DS* are subsumed by the firm and year fixed effects structure.

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TABLE 9 Alternative difference-in-differences methods

	Sun and Abraham (2021)(1)	Stacked difference in-differences(2)
WDL	0.0023	0.0046
	(0.192)	(0.306)
UR×*WDL*	0.0123	−0.0442***
	(0.130)	(0.000)
D×*WDL*	−0.0060**	0.0104
	(0.025)	(0.281)
UR*×*D*×*WDL	0.0335*	0.0882***
	(0.068)	(0.000)
Constant	0.0006***	0.0006***
	(0.000)	(0.000)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	78,274	78,274
R-squared	0.014	0.013

Note: This table presents the regression results of alternative difference-in-differences methods. Column 1 applies the method of Sun and Abraham (2021); Column 2 applies the stacked difference-in-differences approach. The sample includes firms that were treated during the sample period over the years −5 to +20 relative to their treatment year (denoted as year 0) and clean control firms (never-treated observations) for all years with available data. Variable definitions are provided in the Appendix. *p*-values based on robust standard errors clustered by state are reported in parentheses.
The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

4.6.3 | Alternative difference-in-differences methods

Baker et al. (2022) show that standard difference-in-differences estimates can be biased when multi-treatments occur in different times, partially because earlier treatment groups serve as controls for later treatment groups. Given that we exploit staggered adoptions of WDLs in different years, we apply two alternative difference-in-differences methods as a robustness check. They include (1) the method proposed by Sun and Abraham (2021) and (2) the stacked difference-in-differences method proposed by Cengiz et al. (2019).

For the estimator developed by Sun and Abraham (2021), we first compute the individual cohort-time-specific treatment effects, allowing for treatment effect heterogeneity; we then aggregate these treatment effects to produce the overall treatment effects. As to the stacked difference-in-differences method, as described in Cengiz et al. (2019), the idea is to create event-specific clean 2 × 2 datasets for the treated groups and clean control groups within the treatment window. We then stack all these clean 2 × 2 datasets together and estimate a difference-in-differences regression with dataset-specific firm and year fixed effects.

Table 9 reports the results. The sample includes firms that were treated during the sample period over the years −5 to +20 relative to their treatment year (denoted as year 0) and clean control firms (never-treated observations) for all sample years with available data. In Column 1 for the method of Sun and Abraham (2021), the coefficient on *UR*×*D*×*WDL* is 0.0335 and significant at the 10% level. Further, in Column 2 for the stacked difference-in-differences method, the coefficient on *UR*×*D*×*WDL* is 0.0882 and significant at the 1% level. Overall, these results indicate that our main inference still holds under the alternative difference-in-differences methods.

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TABLE 10 Control for new accounting standard issuing

	(1)	(2)
WDL	−0.0031	−0.0030
	(0.655)	(0.670)
UR×*WDL*	−0.0183	−0.0193
	(0.507)	(0.502)
D×*WDL*	0.0181*	0.0185
	(0.094)	(0.119)
UR*×*D*×*WDL	0.0467*	0.0487*
	(0.065)	(0.052)
New Standard		0.0045
		(0.233)
UR×*New Standard*		0.0081
		(0.406)
D×*New Standard*		0.0113***
		(0.009)
UR×*D*×*New Standard*		0.0195*
		(0.065)
Constant	0.0010***	0.0007***
	(0.000)	(0.000)
Firm fixed effect	Yes	Yes
Year fixed effect	Yes	Yes
Observations	85,859	95,599
R-squared	0.225	0.222

Note: This table presents the regression results of controlling for new accounting standard issuing. In Column 1, we re-estimate Table 3, Column 1, by excluding the 2001–2003 period. In Column 2, the indicator variable *New Standard* equals to one for the year of FASB issuing the accounting standards that have been shown to have an effect on firms' accounting conservatism in the literature, including FAS 2, FAS 106, FAS 121, FAS 131 and FAS 142 and zero otherwise. Variable definitions are provided in the Appendix. *p*-values based on robust standard errors clustered by state are reported in parentheses.
The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

4.6.4 | Changes in accounting standards

In our sample period, there are several changes in accounting standards that can affect realized conditional conservatism. FAS 142, introduced in 2001, is particularly relevant as it increases firms' accounting conservatism by requiring firms to estimate the implied fair value of a reporting unit's goodwill and the corresponding write-offs when the implied fair value is below its book value (Cedergren et al., 2015; Jarva, 2014; Olante, 2013). In Table 10, Column 1, we re-estimate our baseline regression by excluding the 2001–2003 period, and we show that our inference is largely unchanged. Moreover, in addition to FAS 142, existing literature has shown that FAS 2 (1974), FAS 106 (1990), FAS 121 (1995) and FAS 131 (1997) also have significant effects on conditional conservatism (Bens et al., 2018; Chandra, 2011; García Lara et al., 2020; Oler, 2014). We define the indicator variable *New Standard* to flag the years of FASB issuing the above accounting standards. In Column 2 of Table 10, we then directly control for the effect of these new standards by including *UR*×*New Standard*, *D*×*New Standard* and *UR*×*D*×*New Standard* in our baseline regression. We

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continue to find a positive and significant effect of WDLs on firms' conditional conservatism. Thus, our main findings are robust to any changes in accounting standards during our sample period.

5 | CONCLUSION

In this paper, we study the effect of employee firing costs on firms' conditional conservatism. To identify the causal effect, we exploit US states' staggered adoption of WDLs, which protect workers from termination out of bad faith, malice or retaliation and raise firms' costs of firing workers accordingly.

Based on a difference-in-differences approach, we find a significant increase in conditional conservatism for firms headquartered in states that adopt WDLs as compared to firms headquartered elsewhere. Further, our pre-trend tests show that there is no pre-treatment difference in conditional conservatism between the treatment and control firms and that the effect shows up only after the law adoption. Further, we continue to find the positive impact of WDLs on firms' conditional conservatism after we differentiate away unobserved confounding economic conditions by examining treated firms and their matched control firms closely located on state borders. Last, the cross-sectional variation of the treatment effects indicates that the effect of WDLs on conditional conservatism is indeed related to employee firing costs. The treatment effect is stronger for firms that are more labor-intensive, for firms that have a higher propensity to fire employees, for firms that make more firm-specific investments, and for firms that have higher risk. Overall, our findings are consistent with the view that the cost of discharging employees makes firms more vulnerable to investment inefficiencies and thus leads to a greater demand for conservative accounting policies to curb inefficient investment projects (including avoiding negative-NPV projects *ex-ante* and eliminating these projects *ex-post*).

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DATA AVAILABILITY STATEMENT

Data sources are indicated in the manuscript. Restrictions apply to the availability of these data, which were used under license for this study.

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APPENDIX

VARIABLE DEFINITION

Variable	Definition
UE	Annual earnings minus expected annual earnings, scaled by the beginning market value of equity. Expected annual earnings are estimated by the expectation model of Ball et al. (2013a)
WDL	An indicator variable taking the value of one if a firm's headquarter state has passed the good faith exception and zero otherwise
UR	Annual returns minus expected annual returns for the fiscal year Expected annual returns are the value-weighted average return for the applicable portfolio in 25 portfolios formed each fiscal year by first sorting firms into quintiles based on the beginning market value of equity and then sorting each of these quintiles into quintiles based on the beginning book-to-market equity ratio
D	An indicator variable taking the value of one if *UR* is negative and zero otherwise
Risk	Variance of equity returns during the fiscal year
MB	Market value of equity divided by the book value of equity
C-Score	Conservatism measure estimated following Khan and Watts (2009)
$\triangle$*Sale*	Sales change from year $t-1$ to year t, scaled by the beginning market value of equity
DS	An indicator variable taking the value of one if firms' sales decrease relative to the previous year and zero otherwise
New Standard	An indicator variable taking the value of one for the year of FASB issuing the accounting standards that have been shown to have an effect on firms' accounting conservatism in the literature, including FAS 2 (1974), FAS 106 (1990), FAS 121 (1995), FAS 131(1997) and FAS 142 (2001) and zero otherwise

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