Wrongful Discharge Laws and Asymmetric Cost Behavior

Yongtae Kim[1], Siqi Li[1*], and Hyungshin Park[1]

ABSTRACT

Exploiting the natural experiment created by the adoption of wrongful discharge laws (WDL) across US states, we examine the effect of legal protection against unjust employment termination on firm-level cost behavior. We find that the adoption of WDL increases the asymmetric sensitivity of costs to activity (i.e., cost stickiness). The effect of WDL on cost stickiness is more pronounced when employees have less negotiation power in lay-off decisions or when firms can more easily fire their employees. Our evidence suggests that changes in the state-level legal environment have a significant effect on firm-level resource adjustment decisions and asymmetric cost behavior.

Keywords: cost stickiness; asymmetric cost behavior; wrongful discharge laws; employment laws; labor adjustment costs

JEL Classifications: E20, J30, K31, M41

[1] Santa Clara University

* Corresponding author:
Siqi Li, Associate Professor of Accounting,
Leavey School of Business, Santa Clara University
500 El Camino Real, Santa Clara, CA 95053, USA
Sli3@scu.edu

1 Introduction

How a firm's legal environment influences managerial incentives and financial reporting characteristics is of significant interest to market participants, legislators, and regulatory bodies. Extant evidence suggests that political economy, tax regime, and country-specific legal and judicial systems can create incentives that shape the properties of reported accounting numbers (Bushman and Piotroski, 2006; Holthausen, 2009; Stulz, 2009). In this paper, we examine how changes in state-level labor employment laws affect firm-level cost behavior.

Recent studies provide strong evidence in support of an asymmetric relation between changes in activity and changes in costs. That is, costs fall to a lesser extent in response to sales decreases than they rise in response to an equivalent level of sales increases (Anderson, Banker, and Janakiraman, 2003; Andersen, Banker, Huang, and Janakiraman, 2007). This asymmetric or "sticky" cost behavior occurs because managers make deliberate decisions to commit resources as changes in resource commitments entail adjustment costs such as hiring and firing costs for labor or installation and disposal costs for equipment. When sales decrease, managers retain some slack resources rather than to make full downward resource adjustment, expecting a potential near-future reversal in sales. In contrast, when sales increase beyond capacity, managers are likely to add needed resources instantaneously, as there is less room for exercising discretion on resource management to meet the growing demand. Studies suggest that sticky cost behavior increases in adjustment costs (Anderson et al., 2003).

There has been a long legal presumption in the US that workers can be fired at will. During the 1970s and 1980s, this presumption started to erode rapidly: most US state courts adopted three classes of common-law restrictions that limited employers' ability to fire workers: public-policy exception, good-faith exception, and implied-contract exception, which are commonly referred to as wrongful discharge laws (WDL) (Autor, Donohue, and Schwab, 2006; Acharya, Baghai, and Subramanian, 2014). The passages of these laws were largely unexpected because the judicial decisions in the precedent-setting cases are more likely to be driven by the merits of the case than by political economy considerations (Walsh and Schwarz, 1996).

The adoption of wrongful discharge laws increases downward labor adjustment costs by making layoffs more difficult. The greater adjustment costs and ensuing slower adjustment to labor resources lead to lower sensitivity of costs to sales *decreases*, thereby increasing cost stickiness. The adoption of WDL may also decrease cost sensitivity to sales *increases* if managers are more reluctant to hire additional workers in response to sales increases when potential future lay-off costs are greater. While the adoption of WDL can decrease cost sensitivities to both sales decreases and sales increases, we expect the former effect to dominate the latter because managers generally have less room for exercising discretion in resource commitments when sales increase beyond the current resource capacity (Anderson et al., 2003). Thus, we predict that the degree of cost stickiness increases following the adoption of WDL.

To test our hypothesis, we employ a difference-in-differences (DiD) design by comparing the changes in cost stickiness in states before and after the adoption of WDL, relative to the corresponding changes in states that did not adopt such laws. Our regression models control for previously documented time-varying characteristics that potentially influence the degree of cost stickiness. We also include state, industry, and year indicators to control for time-invariant, unobservable state and industry characteristics, as well as economy-wide shocks. Based on a large panel of 121,728 firm-years over 1970-1999, we find robust evidence that the state-wide adoption of the good-faith exception, arguably the most far-reaching WDL (Kugler and Saint-Paul, 2004), increases cost stickiness. The effect of WDL adoption on cost stickiness is also economically significant. For instance, we find that cost stickiness increases by 16.7% after the passage of the good-faith exception.

Our results are robust to a battery of sensitivity tests. Specifically, the results are robust to excluding firms headquartered in Washington D.C., addressing potential measurement errors in headquarters states, including additional state-level controls, and employing alternative model specifications. In addition, we find that the effect of WDL adoption on cost stickiness is more pronounced when managers are more optimistic about future sales changes (i.e., when managerial discretion in resource commitments creates asymmetric cost behavior).

To ensure that our results are not spurious, we conduct a placebo test in which we randomly assign firm-years into pseudo-WDL firm-years or non-pseudo-WDL firm-years and re-estimate the effect of these pseudo-WDL adoptions on firms' cost behavior. We repeat this exercise 1,000 times and plot the distribution of the coefficients from the randomly assigned samples. The results suggest that relative to the placebo coefficients obtained from such random assignments of WDL events, the effect of the good-faith exception on the asymmetric cost behavior is strongly significant. These analyses suggest that our main finding is unlikely to be obtained by chance.

To provide additional evidence that the effect of WDL on cost stickiness is through increased labor adjustment costs, we conduct a cross-sectional analysis that examines the heterogeneous treatment effects of WDL adoption. Our results suggest that the effect of the adoption of the good-faith exception on cost stickiness is more pronounced when employees collectively have less negotiation power in lay-off decisions or when firms can more easily fire their employees. These results provide further support for our inference on the causal relationship between WDL and cost stickiness.

Our study makes the following contributions to the literature. First, by studying the relation between state-level labor laws and internal resource allocation decisions, we extend prior studies that focus on the effect of securities laws on financial accounting outcomes (e.g., Bushee and Leuz, 2005; Bushman and Piotroski, 2006; Cohen, Dey and Lys, 2008). We provide evidence that state-level employment laws shape managerial incentives that influence firms' cost behavior.

Second, we contribute to the growing line of research on cost behavior and cost stickiness by documenting a *causal* effect of state-level employment protection laws on firm-specific asymmetric cost behavior. Our study is related to Banker, Byzalov and Chen (2013), which examines the effect of country-level employment protection legislation (EPL) on firm-level cost stickiness. However, given that EPL in Banker et al. (2013) is a time-invariant, country-specific measure which might be highly correlated with other country-level characteristics, disentangling the effect of employment protection strictness in a cross-country setting remains a challenge (Isidro, Nanda, and Wysocki, 2016).[1] We exploit the staggered adoption of employment protection legislations across US states, a setting that allows us to draw causal inferences on the effect of the legal protection against unjust employment termination on firm-level cost behavior. Our study also differs from Banker et al. (2013) by highlighting the importance of state-level factors in understanding the asymmetric cost behavior beyond the previously documented effects of firm-specific and country-level factors.

The rest of the paper is organized as follows: Next section provides the literature review and hypothesis development, which is followed by a section describing our sample and data.

[1] In Banker et al.'s (2013) setting, adding country fixed effects to control away the effect of correlated-omitted time-invariant country-level characteristics is not feasible, because the variable of interest (i.e., EPL) itself is also a time-invariant country-level characteristic.

Subsequently, we report our empirical results and then present the results of the additional analyses. We end the article by presenting summary and concluding thoughts.

2 Literature and Hypothesis Development

2.1 Wrongful Discharge Laws (WDL)

A long-standing tradition in the US labor market is that workers can be fired at will, that is, "for good cause, bad cause, or no cause at all."[2] Since the 1970s, however, this employment-at-will doctrine has been challenged as most U.S. states adopted one or more common law exceptions to limit employers' ability to fire employees. Collectively, these common law exceptions are referred to as wrongful discharge laws and fall under three categories: (1) public-policy exception, (2) good-faith exception, and (3) implied-contract exception.

The public-policy exception prevents employers from firing workers that would impede an important public policy such as performing jury duty, filing a worker's compensation claim, reporting an employer's wrongdoing, or refusing to commit perjury. This exception provides tort-based protection for employees as they can sue for lost earnings, pain and suffering, and punitive damages. By 1999, a total of forty-three states have adopted this exception. Despite its widespread adoption, legal scholars argue that its legal and economic significance is rather minor as courts typically limit public-policy cases to clear violations of explicit legislative commands (Edelman, Abraham, and Erlanger, 1992).

The good-faith exception, on the other hand, prohibits employers from firing workers for "bad cause," for example, to deprive them of a promised benefit, or more generally, from firing employees without a just cause. In contrast to the public-policy exception, many legal scholars

[2] See Payne v. Western & Atlantic Railroad, Supreme Court of Tennessee, 1884.

agree that the good-faith exception is the most far-reaching wrongful discharge law in the sense that it implies that dismissal must always be for a cause, and can potentially be very costly for employers due to the applicability of tort law (Kugler and Saint-Paul, 2004). As of 1999, there are in total eleven states that recognize this exception. New Hampshire and Oklahoma had adopted the good-faith exception but later reversed the adoption.

Finally, the implied-contract exception makes employers' informal assurances of ongoing employment legally enforceable. Under this exception, an employer can terminate a worker for only a good cause when the employer has implicitly offered an ongoing employment to the worker. In other words, this exception imposes a cost to employers in dismissing long-term employees who simply may not fit in or who cannot be shown by the employer to have performed poorly. In total forty-three states have adopted the implied-contract exception since the early 1970s but two states (Arizona and Missouri) reversed the adoption later, leaving forty-one states that recognize this exception as of 1999.

A large body of literature in economics has examined the implications of WDL. In general, studies find that these laws can impose substantial costs on employers. Dertouzos, Holland, and Ebener (1988), for example, investigate WDL trials in California in 1980-1986 and find that the trials can result in significant compensatory and punitive damages for employers. Jung (1997) draws similar conclusions by examining WDL jury verdicts in California and Texas from 1992 to 1996.

Given how costly these exceptions can be to employers, it is not surprising that the adoption of WDL has affected companies' hiring and firing practices in a significant way. Miles (2000) and Autor (2003) find that employers substitute temporary help agency workers for direct hire employees shortly after their states adopted implied-contract exceptions, presumably in an effort to minimize litigation risk. Kugler and Saint-Paul (2004) present a theoretical model on how firing costs affect worker flows and provide empirical evidence that the adoption of WDL reduces the re-employment probabilities of unemployed workers relative to employed workers. Autor et al. (2006) show that the adoption of the implied-contract exception leads to a reduction in state employment by 0.8 to 1.6 percent. Using firm-level employment data, Autor, Kerr, and Kugler (2007) find that the adoption of the good-faith exception reduces annual employment fluctuations and the entry of new establishments as well as total factory productivity in adopting states, consistent with the significant impact of dismissal protections on firms' production choices and productivity. Acharya et al. (2014) present a model that WDL limits an employer's capacity for holding up innovating employees when contracts are incomplete. Their empirical evidence supports this prediction and shows that WDL spurs innovation and new firm creation.

2.2 Hypothesis Development

Anderson et al. (2003) posit that costs behave asymmetrically. That is, costs increase to a larger extent when demand increases than costs decline when demand decreases. This notion of cost asymmetry, or cost stickiness, differs from the traditional view of a mechanical relation between changes in activities and changes in costs (Balakrishnan, Petersen, and Soderstrom, 2004), and is built on the premise that managers make deliberate resource commitment decisions. Changing resource commitment can be costly, as they often involve substantial adjustment costs including, for example, firing and hiring costs for labor resources, and disposal and installation costs for equipment. In addition, resource commitment decisions are sensitive to managers' incentives and behavioral biases (Chen, Lu, and Sougiannis, 2012; Dierynck, Landsman, and Renders, 2012; Kama and Weiss, 2013).

As argued by Anderson et al. (2003), resources adjustment costs are a key element to understanding the asymmetric cost behavior. However, an important challenge in establishing the casual link between resource adjustment costs and asymmetric cost behavior is the endogenous nature of as well as a lack of observability of resource adjustment costs. Prior studies (e.g., Anderson et al., 2003) find a greater degree of cost stickiness in asset- and employee-intensive firms, consistent with the idea that firms with an extensive use of internal resources face higher adjustment costs. Banker et al. (2013) use the strength of country-level employment protection legislation (EPL) reported in the OECD Employment Outlook as a proxy for labor adjustment costs, and find that cost stickiness is higher in countries with stricter employment protection. As noted in Banker et al. (2013), however, the EPL index is a time-invariant country-specific measure. As many country-level characteristics are highly correlated with each other (Isidro et al. 2016), disentangling the effect of EPL from other country-level characteristics is a challenge. Furthermore, establishing causality is not easy due to the time-invariant nature of the EPL index. We take advantage of the staggered adoption of WDL across US states to overcome this methodological challenge.

We argue that as WDL increase potential costs of discharging employees, the adoption of such laws is likely to lead to an increase in labor adjustment costs and consequently a higher degree of cost stickiness. This is because when managers trade off the net present value of the productivity of marginal employee against her/his firing costs, an increase in firing costs subsequent to the adoption of WDL will motivate managers to lay off fewer workers as the activity level decreases. In addition, when the firing cost per worker is higher, managers will incur higher total firing costs for the same number of layoffs. This will limit net cost savings from layoffs when the activity level decreases, further amplifying cost stickiness (Banker et al., 2013).

On the other hand, it is worth noting that higher firing costs associated with WDL could also negatively affect a firm's cost stickiness in an indirect manner. Managers who are concerned about potential higher costs associated with *future* layoffs may be reluctant to hire additional workers when activity levels increase. If this is the case, cost sensitivity to an activity increase will be lower following the adoption of WDL, resulting in a potential decrease in cost stickiness. We expect, however, that this negative indirect effect is unlikely to be large enough to offset the positive effect of the WDL adoption on cost stickiness because managers have less room for discretion in resource commitments when sales increase beyond the current resource capacity (Anderson et al. 2003).

The preceding discussion leads to the following hypothesis, stated in an alternative form:

H1: Cost stickiness increases following the adoption of wrongful discharge laws.

3 Sample and Research Design

3.1 Data and Sample

We follow Banker and Byzalov (2014) and require operating expense and sales revenue to be non-missing and positive for the current and two previous years, and operating expense to be greater than 0.1 and less than 10 times of sales revenue to exclude firms that are going through major structural changes such as large mergers and acquisitions or spinoffs.[3] We adjust the financial variables for inflation by deflating them by CPI, following Konchitchki (2011). Our final sample includes 121,728 firm-year observations, spanning from 1970 to 1999. The sample period is restricted mainly by the availability of state WDL adoption data from Autor et al. (2006), which we match with the Compustat data using firms' headquarters location.[4] We classify a firm-year as a post-WDL year if the firm's fiscal year end date falls one year after the WDL adoption of the state where the firm's headquarters is located but before the year WDL is reversed, if there is any.

---

[3] Using alternative screening procedures such as in Banker et al. (2013) yields qualitatively similar results.

[4] Similarly, Acharya et al.'s (2014) sample encompasses the years 1971-1999. Following prior studies (e.g., Hilary and Hui 2009), we obtain headquarters-state information from Compustat, which provides firms' most recent, rather than historical headquarters location. We assess the implications of possible changes in firms' headquarters location on our results in robustness tests. The results of the robustness tests suggest that our results are unlikely to be affected by firms that have changed their headquarters location.

In Table 1 we present the adoption/reversal dates of WDL by states. The most common form of WDL is the public-policy exception (forty-three states), followed by the implied-contract (forty-three states; two states later reversed the adoption) and good-faith exceptions (thirteen states; two states later reversed the adoption). While most states adopted multiple forms of WDL (eleven states have adopted all three forms; thirty states have adopted two), a few of them have adopted only one form of WDL (six states) or even none (three states). The majority of states keep WDL effective once they adopted it, but some states have reversed it.[5]

In Table 2, we present the distribution of our sample across states (in Panel A), by years (in Panel B), and by Fama-French 49 industries (in Panel C). Our sample is represented by all states, where California contributes the largest number of firm-years to the sample, accounting for approximately 12%. Panel B of Table 2 shows that our sample is more represented by recent years. Panel C shows that our sample is represented by diverse industries, where the largest number of firm-year observations is from utilities, retail, and electronic equipment industries.

The descriptive statistics and correlations of key variables are presented in Table 3. Because our sample period is earlier than that of most cost stickiness studies due to the requirement on WDL adoption data availability, the descriptive statistics of our variables are slightly different from those studies. For instance, the annual changes in sales revenue or in operating costs are generally larger than those in recent studies.

3.2 Research Design

[5] For example, New Hampshire and Oklahoma have reversed their adoption of the good-faith exception after six and four years from the adoption, respectively, and Arizona and Missouri have reversed the adoption of the implied-contract exception after less than a year and five years from the original adoption, respectively.

Anderson et al. (2003) examine the asymmetric response of costs to activity changes using a piecewise-linear relation between log-changes in costs and concurrent log-changes in sales as in the following firm-level regression:

$$\Delta log(XOPR_t) = \alpha_0 + \alpha_1 \, \Delta log(Sales_t) + \alpha_2 \, DEC_t * \Delta log(Sales_t) + u \quad (1)$$

where $\Delta log(XOPR_t)$ is the change in the natural logarithm of operating costs, measured as the difference between sales and operating income in year *t*. $\Delta log(Sales_t)$ is the change in the natural logarithm of sales revenue in year *t*. DEC_t is an indicator variable equal to one if sales decrease in year *t*, and zero otherwise.

In equation 1, the coefficient α_1 represents the percentage change in operating costs for a percentage change in sales when sales increase. The coefficient α_2 captures the degree of cost stickiness. A negative α_2 would indicate that costs fall to a lesser extent for a one percent sales decrease than they rise for an equivalent sales increase. A positive α_2 would indicate anti-stickiness and suggests that costs fall faster for a sales decrease than they rise for an equivalent sales increase. The coefficients α_1 and α_2 are often modeled as a function of firm-level characteristics.

In a standard DiD design, one would examine the effect of treatment using the following regression specification:

$$Y = \beta_1 + \beta_2 \, TREAT + \beta_3 \, POST + \beta_4 \, TREAT*POST + CONTROLS \quad (2)$$

where *Y* is the dependent variable, *TREAT* is an indicator for the treatment sample (e.g., firms located in states that adopt WDL during the sample period), *POST* is an indicator for the post-treatment period (e.g., post WDL adoption period). However, in our setting, *POST* is not defined for firms located in non-adoption states as these states did not adopt WDL during our sample period. Therefore, by replacing *POST* with year fixed effects we modify this design as follows:

$$Y = \beta_1 + \beta_2 \, TREAT + \beta_3 \, TREAT*POST + CONTROLS + Year\ FE \quad (3)$$

where *Year FE* is the year fixed effects. Let's say, state A is the state that has adopted WDL and state B is the state that has never passed WDL. The variable *TREAT* will be one for all years for firms located in state A and zero for all years for firms located in state B. *TREAT*POST* will be one for firms located in state A for the years when WDL is in effect and zero for other years. *TREAT*POST* will be zero for all years for firms located in state B. In years when WDL is not in effect in state A, the difference in the cost stickiness between firms in state A and firms in state B will be captured by β_2. In years WDL is in effect in state A, the difference in the cost stickiness between firms in state A and firms in state B will be $\beta_2 + \beta_3$. Thus, the DiD coefficient is β_3.

Considering three different types of WDLs adopted across US states, we specify the slopes in equation 1 (i.e., α_1 and α_2) as a function of the state adoption of WDL and firm-level control variables as follows.

$$\alpha_1 = \beta_1 + \beta_2\, WDL_P + \beta_3\, WDL_G + \beta_4\, WDL_C + \beta_5\, POSTWDL_P + \beta_6\, POSTWDL_G + \beta_7\, POSTWDL_C + \beta_8\, GDPGROWTH_t + \beta_9\, AINT_t + \beta_{10}\, EINT_t + Year\ FE \tag{4}$$

$$\alpha_2 = \beta_{11} + \beta_{12}\, WDL_P + \beta_{13}\, WDL_G + \beta_{14}\, WDL_C + \beta_{15}\, POSTWDL_P + \beta_{16}\, POSTWDL_G + \beta_{17}\, POSTWDL_C + \beta_{18}\, DEC_{t-1} + \beta_{19}\, GDPGROWTH_t + \beta_{20}\, AINT_t + \beta_{21}\, EINT_t + Year\ FE \tag{5}$$

where WDL_P, WDL_G, and WDL_C are indicator variables, each representing whether the firm's headquarters state has ever adopted a particular type of WDL: the public-policy, good-faith and implied-contract exceptions of WDL. $POSTWDL_P$, $POSTWDL_G$, and $POSTWDL_C$ are indicator variables, each representing the post adoption period of the public-policy, good-faith, and implied-contract exceptions, respectively, by the firm's headquarters state in year *t*.[6] *GDPGROWTH* is the

[6] Let's say, state A is the state that has adopted the good-faith exception and state B is the state that has never passed the good-faith exception. The variable WDL_G will be one for all years for firms in state A and zero for all years for firms in state B. $POSTWDL_G$ will be one for firms in state A for the years when good-faith exception is in effect and zero for other years. $POSTWDL_G$ will be zero for all years for firms in state B. In years when the good-faith exception is not in effect in state A, the difference in the cost stickiness between firms in state A and firms in state B

real GDP growth rate in year t. $AINT_t$ reflects asset intensity and is measured as the natural logarithm of the ratio of assets to sales in year t. $EINT_t$ reflects employee intensity and is measured as the natural logarithm of the ratio of the number of employees to sales in year t. DEC_{t-1} is an indicator variable that takes the value of one if sales decreased in year $t-1$, and zero otherwise. Variable definitions and data sources are provided in Appendix A.

We control for managers' optimism or pessimism regarding future sales, proxied by real GDP growth rates ($GDPGROWTH_t$) and successive decreases in sales (DEC_{t-1}), because management's assessment about future sales changes influences asymmetric cost behavior. In particular, Anderson et al. (2003) argue that managers become more pessimistic and hence are more willing to reduce slack resources if they observe two consecutive sales decreases. Firm-level asset intensity ($AINT_t$) and employee intensity ($EINT_t$) proxy for the magnitude of adjustment costs at the firm level.

Combining equations 4 and 5 with equation 1, we expand Anderson et al.'s (2003) standard model of cost stickiness by allowing the adoption of WDL and control variables to affect α_1 and α_2. Our panel regressions also include state, industry (based on Fama-French 49 classification) and year fixed effects to control for time-invariant state-level or industry-level unobservable factors as well as intertemporal shocks. Thus, our main regression model is specified as follows:

$$\begin{aligned} \Delta log(XOPR_t) = {} & \beta_0 + \Delta log(Sales_t)*[\beta_1 + \beta_2\ WDL_P + \beta_3\ WDL_G + \beta_4\ WDL_C \\ & + \beta_5\ POSTWDL_P + \beta_6\ POSTWDL_G + \beta_7\ POSTWDL_C \\ & + \beta_8\ GDPGROWTH_t + \beta_9\ AINT_t + \beta_{10}\ EINT_t + Year\ FE] \\ & + DEC_t*\Delta log(Sales_t)*[\beta_{11} + \beta_{12}\ WDL_P + \beta_{13}\ WDL_G + \beta_{14}\ WDL_C \\ & + \beta_{15}\ POSTWDL_P + \beta_{16}\ POSTWDL_G + \beta_{17}\ POSTWDL_C + \beta_{18}\ DEC_{t-1} \\ & + \beta_{19}\ GDPGROWTH_t + \beta_{20}\ AINT_t + \beta_{21}\ EINT_t + Year\ FE] \\ & + State\ FE + Industry\ FE + Year\ FE + u \end{aligned} \tag{6}$$

---

will be captured by β_{13}. In years WDL_G is in effect in state A, the difference in the cost stickiness between firms in state A and firms in state B will be $\beta_{13} + \beta_{16}$. Thus, the DiD coefficient is β_{16}.

Employing the above specification, we compare changes in cost stickiness in states that passed WDL to the changes in states that did not. Interacting WDL_P, WDL_G, WDL_C, $POSTWDL_P$, $POSTWDL_G$ and $POSTWDL_C$ as well as year fixed effects with $\Delta log(Sales_t)$ and $DEC_t*\Delta log(Sales_t)$ enables us to examine the effect of adopting a particular type of WDL through a difference-in-differences design. The coefficients on $DEC_t*\Delta log(Sales_t)*POSTWDL_i$ (β_{15}, β_{16} and β_{17}) are our variables of interest, which represent the change in cost stickiness after the WDL adoption, compared to firms in non-WDL adopting states.

4 Empirical Results

4.1 Effects of WDL on Cost Stickiness

Table 4 reports our regression results estimating equation 6. We adjust the standard errors for state clustering in this and all subsequent regressions (Acharya et al., 2014). Columns (1) and (2) report the results without any fixed effects and with state, industry and year fixed effects, respectively. Consistent with prior studies, we find a significantly positive coefficient on $\Delta log(Sales_t)$ and a significantly negative coefficient on $DEC_t*\Delta log(Sales_t)$ for both models, suggesting that asymmetric cost behavior exists in states that never passed wrongful discharge laws.

More importantly, the significantly negative coefficients on $DEC_t*\Delta log(Sales_t)*POSTWDL_G$ in both columns suggest that the good-faith exception (WDL_G) increases cost stickiness, whereas the insignificant coefficients on $DEC_t*\Delta log(Sales_t)*POSTWDL_P$ and $DEC_t*\Delta log(Sales_t)*POSTWDL_C$ indicate that the public-policy exception (WDL_P) or implied contract exception (WDL_C) has no statistically significant impact on cost stickiness. This is consistent with the findings in prior studies that good-faith exception generally has a more pronounced effect on the labor market than other WDLs (e.g., Acharya et al., 2014). The coefficient on $\Delta log(Sales_t)*POSTWDL_G$ is also significantly negative in both columns, suggesting that the adoption of the good-faith exception reduces the cost sensitivity to sales increases as well. This result is consistent with the notion that managers are concerned about future layoff costs, thus becoming more reluctant to add resources in response to increased demand when the state adopts the good-faith exception. However, the cost sensitivity to sales decreases falls even further after the adoption of WDL_G, increasing the overall cost asymmetry as evidenced by the significantly negative coefficients on $DEC_t*\Delta log(Sales_t)*POSTWDL_G$. The effect of good-faith exception on cost stickiness is economically significant. For instance, in the second model with state, industry and year fixed effects, the incremental effect of good-faith exception on cost stickiness is a 16.7% increase controlling for other factors.[7]

Regarding the other determinants of cost stickiness, we find that firms experiencing consecutive negative demand shocks in the prior two years ($DEC_t*\Delta log(Sales_t)*DEC_{t-1}$) have a lower degree of cost asymmetry, while firms with high asset intensity ($DEC_t*\Delta log(Sales_t)*AINT_t$) or with more employees to support their operations ($DEC_t*\Delta log(Sales_t)*EINT_t$) have a higher degree of cost asymmetry. These patterns are in line with those documented in prior studies. In sum, the results in Table 4 are consistent with our hypothesis that cost stickiness increases after a statewide WDL adoption because the adoption makes it costlier for managers to fire employees, causing them to be reluctant to lay off excess labor force when activity levels decrease.

4.2 Robustness Checks

We check the robustness of our regression results in Table 4 by employing alternative samples to address potential measurement errors in headquarters states. The results of these tests are presented in Table 5.

[7] For example, the cost stickiness after adopting the good-faith exception is -0.412 (= -0.376+0.023-0.059), while the cost stickiness before adopting the good-faith exception is -0.353 (= -0.376+0.023). So, the ratio of these two is 1.167 (= [-0.412]/[-0.353]).

In the first column of Table 5, we exclude 233 firm-years with D.C. headquarters as we code them as Virginia firms in our main analysis. We continue to find that the coefficient on DEC_t*$\Delta log(Sales_t)$*$POSTWDL_G$ is significantly negative, indicating that the good-faith (WDL_G) increases cost stickiness.

Next, we examine whether our results are sensitivity to firms that have changed headquarters state. In the preceding analyses, we follow prior studies (Hilary and Hui, 2009; Acharya et al., 2014) and obtain headquarters-state information from Compustat, which provides firms' most recent headquarters location. Because firms can change their headquarters location over time, the results based on Compustat headquarters information potentially suffer from measurement errors. To ensure that our results are not affected by firms that have changed their headquarters location, we utilize Bill McDonald's 10-K filing dataset, which spans from 1994 to 2010 and provides historical headquarters-state information (Chen, Li, and Xu, 2017; Li, Lin, and Zhang, 2018).[8] We conduct two robustness tests using this dataset. First, we identify firms that have changed their headquarters-state in Bill McDonald's dataset, and estimate our regression after excluding these firms. There are 2,931 firms (13.1% of 22,339 firms) that have changed their headquarters state between 1994 and 2010. Second, we replace the headquarters-state information with that in Bill McDonald's dataset. For years 1994-1999, Compustat headquarters information is replaced with historical headquarters location information from Bill McDonald's dataset and for years prior to 1994, Compustat headquarters information is replaced with that from the first year of Bill McDonald's dataset. This way, firms' headquarters-state reflects the historical headquarters

[8] The dataset is available at Bill McDonald's website: http://www3.nd.edu/~mcdonald/10-K_Headers/10-K_Headers.html.

location as of the year closest to the firm-year in question, hence minimizing the undue influence of headquarters-state changes.[9]

The results of these tests are presented in the second and third columns of Table 5, respectively. The result reported in the second column confirms that the adoption of good-faith exception increases cost stickiness even after we exclude firms that have changed their headquarters-state per Bill McDonald's dataset. The result in the third column also indicates that replacing the headquarters states with those in Bill McDonald's dataset does not affect our inferences.

In addition to the tests presented in Table 5, we run other robustness tests that are untabulated for brevity, including alternative clustering methods (clustering by firm), and excluding 1997-1999 because the US Bureau of Economic Analysis changed its GDP collection method in 1997. We also replace the dependent variable with the change in log of SG&A because the costs of hiring (firing) general employees to meet increased (decreased) demand for products are likely to be reflected in selling and administrative costs. Our inferences are not affected in these tests. Collectively, our findings are consistent with our hypothesis that the adoption of WDL, in particular, the good-faith exception, increases cost stickiness.

5 Additional Analyses

5.1 Alternative Models of Cost Stickiness

In this section, we estimate alternative cost stickiness models to gain further insights. We first include in our main model (equation 6) additional state-level controls to mitigate a potential correlated omitted variables problem. According to Gao and Ma (2016) and Klasa, Ortiz-Molina,

[9] Because Bill McDonald's data starts in 1994 and all WDL states had adopted WDL before 1994, if we drop firm-years for which we cannot identify historical headquarters-states (i.e., all firm years prior to 1994), conducting the analysis becomes infeasible.

Serfling, and Srinivasan (2018), Inevitable Disclosure Doctrine (IDD) can have a significant impact on the labor market by limiting employees' mobility. This is because in states that have adopted the IDD, former employees are prevented from working for a rival firm if this would "inevitably" lead them to divulge firm's trade secrets to the rival.[10] Limits to employee mobility may affect cost stickiness by influencing firms' hiring/firing decisions. In addition, if states that pass WDL are also more or less likely to adopt the IDD, it is necessary to disentangle the effect of WDL on cost stickiness from that of the IDD. We obtain data on the adoption of the IDD from Klasa et al. (2018). Furthermore, although prior studies (Walsh and Schwarz, 1996; Autor et al., 2006; Acharya et al., 2014) suggest that the adoption of WDL is unlikely to be driven by political economy considerations, we nevertheless control for proxies for state-level economic conditions including state GDP growth rate (*STGDPGROWTH*) and state unemployment rate (*UNEMPRATE*) to mitigate potential endogeneity concerns. We obtain data on state GDP growth rates from the US Bureau of Economic Analysis and unemployment rates from the US Bureau of Labor Statistics. Specifically, we estimate the following model:

$$
\begin{aligned}
\Delta log(XOPR_t) = {} & \beta_0 + \Delta log(Sales_t)*[\beta_1 + \beta_2\, WDL_P + \beta_3\, WDL_G + \beta_4\, WDL_C \\
& + \beta_5\, POSTWDL_P + \beta_6\, POSTWDL_G + \beta_7\, POSTWDL_C \\
& + \beta_8\, GDPGROWTH_t + \beta_9\, AINT_t + \beta_{10}\, EINT_t \\
& + \beta_{11}\, IDD_t + \beta_{12}\, STGDPGROWTH_t + \beta_{13}\, UNEMPRATE_t + Year\ FE] \\
& + DEC_t*\Delta log(Sales_t)*[\beta_{14} + \beta_{12}\, WDL_P + \beta_{15}\, WDL_G + \beta_{16}\, WDL_C \\
& + \beta_{17}\, POSTWDL_P + \beta_{18}\, POSTWDL_G + \beta_{19}\, POSTWDL_C + \beta_{20}\, DEC_{t-1} \\
& + \beta_{21}\, GDPGROWTH_t + \beta_{22}\, AINT_t + \beta_{23}\, EINT_t \\
& + \beta_{24}\, IDD_t + \beta_{25}\, STGDPGROWTH_t + \beta_{26}\, UNEMPRATE_t + Year\ FE] \\
& + State\ FE + Industry\ FE + Year\ FE + u
\end{aligned}
\tag{7}
$$

We report the result in Panel A of Table 6. The sample size for this test is reduced due to the limited availability of additional state-level data. The result shows that the coefficient on

[10] The IDD is more comprehensive than non-compete or non-disclosure agreement because it is applicable even when employees did not sign a non-compete or non-disclosure agreement with the firm, when there is no evidence of bad faith or actual wrongdoing, or when the rival is located in another state.

DEC_t*Δ$Log(Sales_t)$*$POSTWDL_G$ is significantly negative, indicating that our results are robust to the additional state-level controls.

We next test if the effect of WDL adoption on cost stickiness is more pronounced when managers are more optimistic about future sales changes. Banker et al. (2013) and Banker, Byzalov, Ciftci, and Mashruwala (2014) find that cost stickiness is stronger when sales change in the prior period is positive, because in this case managers are more likely to view the current poor performance as temporary. When managers are more optimistic about future demands, the cost-benefit ratio of downward resource adjustment will be greater. Thus, we expect the effect of WDL adoption to be stronger when prior period sales changes are positive. We estimate two specifications from prior literature that considers prior period sales increase and decrease separately. We first estimate a model adapted from Model A of Banker et al. (2014).

$$
\begin{aligned}
\Delta log(XOPR_t) = \beta_0 &+ INC_{t-1}*\Delta log(Sales_t)*[\beta_1 + \beta_2\, WDL_P + \beta_3\, WDL_G + \beta_4\, WDL_C \\
&+ \beta_5\, POSTWDL_P + \beta_6\, POSTWDL_G + \beta_7\, POSTWDL_C + \beta_8\, DEC_t \\
&+ \beta_9\, DEC_t *WDL_P + \beta_{10}\, DEC_t*WDL_G + \beta_{11}\, DEC_t*WDL_C \\
&+ \beta_{12}\, DEC_t *POSTWDL_P + \beta_{13}\, DEC_t*POSTWDL_G + \beta_{14}\, DEC_t*POSTWDL_C \\
&+ Year\ FE + DEC_t*\ Year\ FE] \\
&+ DEC_{t-1}*\Delta log(Sales_t)*[\beta_{15} + \beta_{16}\, WDL_P + \beta_{17}\, WDL_G + \beta_{18}\, WDL_C \\
&+ \beta_{19}\, POSTWDL_P + \beta_{20}\, POSTWDL_G + \beta_{21}\, POSTWDL_C + \beta_{22}\, DEC_t \\
&+ \beta_{23}\, DEC_t *WDL_P + \beta_{24}\, DEC_t*WDL_G + \beta_{25}\, DEC_t*WDL_C \\
&+ \beta_{26}\, DEC_t *POSTWDL_P + \beta_{27}\, DEC_t*POSTWDL_G + \beta_{28}\, DEC_t*POSTWDL_C \\
&+ Year\ FE + DEC_t*\ Year\ FE] \\
&+ State\ FE + Industry\ FE + Year\ FE + u \qquad (8)
\end{aligned}
$$

INC_{t-1} is an indicator variable that equals one if sales increased in year *t-1*, and zero otherwise. The first column of Table 6, Panel B reports the results of estimating equation 9. We find a significant effect of WDL adoption on cost stickiness when prior period sales changes are positive (i.e., INC_{t-1} =1) but not when prior period sales changes are negative (i.e., DEC_{t-1} =1). The coefficient of $INC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_G$ (β_{13}) is significantly negative, indicating that the good-faith exception strengthens cost stickiness when sales change in the prior period is positive. In contrast, the coefficients on $DEC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_G$ (β_{27}) is not significantly different from zero.

We also estimate a model adapted from Model B of Banker et al. (2013), which we augment by adding *EINT* as an additional control variable. We report the results in the second column of Table 6, Panel B.

$$
\begin{aligned}
\Delta log(XOPR_t) = \beta_0 &+ INC_{t-1}*\Delta log(Sales_t)*[\beta_1 + \beta_2\, WDL_P + \beta_3\, WDL_G + \beta_4\, WDL_C \\
&+ \beta_5\, POSTWDL_P + \beta_6\, POSTWDL_G + \beta_7\, POSTWDL_C + \beta_8\, DEC_t \\
&+ \beta_9\, DEC_t *WDL_P + \beta_{10}\, DEC_t*WDL_G + \beta_{11}\, DEC_t*WDL_C \\
&+ \beta_{12}\, DEC_t *POSTWDL_P + \beta_{13}\, DEC_t*POSTWDL_G + \beta_{14}\, DEC_t*POSTWDL_C \\
&+ Year\ FE + DEC_t*\ Year\ FE] \\
&+ DEC_{t-1}*\Delta log(Sales_t)*[\beta_{15} + \beta_{16}\, WDL_P + \beta_{17}\, WDL_G + \beta_{18}\, WDL_C \\
&+ \beta_{19}\, POSTWDL_P + \beta_{20}\, POSTWDL_G + \beta_{21}\, POSTWDL_C + \beta_{22}\, DEC_t \\
&+ \beta_{23}\, DEC_t *WDL_P + \beta_{24}\, DEC_t*WDL_G + \beta_{25}\, DEC_t*WDL_C \\
&+ \beta_{26}\, DEC_t *POSTWDL_P + \beta_{27}\, DEC_t*POSTWDL_G + \beta_{28}\, DEC_t*POSTWDL_C \\
&+ Year\ FE + DEC_t*\ Year\ FE] \\
&+ \Delta log(Sales_t)*[\beta_{29}\, GDPGROWTH_t + \beta_{30}\, AINT_t + \beta_{31}\, EINT_t] \\
&+ DEC_t*\Delta log(Sales_t)*[\beta_{32}\, GDPGROWTH_t + \beta_{33}\, AINT_t + \beta_{34}\, EINT_t] \\
&+ State\ FE + Industry\ FE + Year\ FE + u \qquad (9)
\end{aligned}
$$

Under this specification, we find that both the good-faith or implied-contract exceptions strengthen cost stickiness only when prior period sales changes are positive (i.e., $INC_{t-1} = 1$), as evidenced by significantly negative coefficients of $INC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_G$ and $INC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_C$ (i.e., β_{13} and β_{14}).[11] As in the first column, the coefficients on $DEC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_G$ (β_{27}) and $DEC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_C$ (β_{28}) are not significantly different from zero.

Our analysis so far follows prior studies and specifies the sensitivities of cost changes to sales increases and decreases as a function of WDL as well as firm-level control variables (Anderson et al., 2003; Banker et al., 2013; Banker et al., 2014). We further check whether the preceding results are robust to adding both the main effects and all lower-order interactions to

[11] We also estimate Model C of Banker et al. (2014). Untabulated results show that the good-faith exception significantly increases cost stickiness only when the prior period sales change is positive, but not when it is negative.

models (8) and (9) and report the results in Panel C of Table 6. The results are similar to those in Panel B. The first column of Table 6, Panel C confirms that the coefficients of $INC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_G$ (β_{13}) and $INC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_C$ (β_{14}) are both significantly negative, indicating that the good-faith and implied-contract exceptions strengthen cost stickiness when sales change in the prior period is positive. In contrast, the coefficients on $DEC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_G$ (β_{27}) and $DEC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_C$ (β_{28}) are not significantly different from zero. Similarly, the second column of Panel C shows significantly negative coefficients on $INC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_G$ and $INC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_C$ (i.e., β_{13} and β_{14}). As in Panel B, the coefficients on $DEC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_G$ (β_{27}) and $DEC_{t-1}*DEC_t*\Delta log(Sales_t)*POSTWDL_C$ (β_{28}) are not significantly different from zero.

5.2 Random Assignment of WDL Firm-years

To ensure that our results are not driven by factors not modelled in our cost stickiness regressions, we conduct a placebo test and examine how likely it is to obtain the sign and magnitude of the coefficients on $DEC_t*\Delta Log(Sales_t)*POSTWDL_P$, $DEC_t*\Delta Log(Sales_t)*POSTWDL_G$ and $DEC_t*\Delta Log(Sales_t)*POSTWDL_C$ as shown in Table 4. To implement the placebo test, we conduct repetitive random assignments of firm-years into pseudo-WDL firm-years or pseudo-non-WDL firm-years, while maintaining the number of pseudo-WDL firm-years to be identical to the number of actual WDL firm-years in our original sample. We do this for each of WDL_P, WDL_G and WDL_C, and rerun the regression model in Table 4 to obtain the coefficients for $DEC_t*\Delta Log(Sales_t)*POSTWDL_P$, $DEC_t*\Delta Log(Sales_t)*POSTWDL_G$ and $DEC_t*\Delta Log(Sales_t)*POSTWDL_C$. We repeat this procedure 1,000 times to obtain the distribution of these coefficients.

The discretized distribution of the coefficients on DEC_t*Δ*Log(Sales*$_t$*)***POSTWDL*$_P$, DEC_t*Δ*Log(Sales*$_t$*)***POSTWDL*$_G$ and DEC_t*Δ*Log(Sales*$_t$*)***POSTWDL*$_C$ is presented in the first, second and third graphs of Figure 1, respectively.

We evaluate the likelihood of the sign and magnitude of each coefficient from our original sample by drawing a dotted vertical line on the corresponding distribution. The first graph shows that the empirical value of the coefficient on DEC_t*Δ*Log(Sales*$_t$*)***POSTWDL*$_P$ is located close to the center of the sampling distribution. The third graph also shows that the empirical value of the coefficient on DEC_t*Δ*Log(Sales*$_t$*)***POSTWDL*$_C$ is located left but still close to the center of the sampling distribution. In contrast, the empirical value of the coefficient on DEC_t*Δ*Log(Sales*$_t$*)***POSTWDL*$_G$ is located far left from the center of the sampling distribution. In particular, the empirical value of DEC_t*Δ*Log(Sales*$_t$*)***POSTWDL*$_P$ coefficient is located at 24.7% from the right end of the sampling distribution and that of DEC_t*Δ*Log(Sales*$_t$*)***POSTWDL*$_C$ is located at 21.9% from the left end of the sampling distribution. In contrast, the coefficient of DEC_t*Δ*Log(Sales*$_t$*)***POSTWDL*$_G$ is located at 0.6% from the left end of the sampling distribution. In sum, these results confirm that the sign and magnitude of the coefficient from our original sample regarding the effect of *WDL*$_G$ on cost stickiness is unlikely to be obtained by chance.

5.3 Cross-sectional Analysis

In this section, we examine whether the effect of wrongful discharge laws varies across firms. As wrongful discharge laws affect cost stickiness through making employee-firing decisions costlier, we expect that the WDL effect is stronger when employees have less negotiation power or when firms can more easily fire their employees. When employees are not unionized they collectively have less negotiation power in mass lay-off decisions. Following prior studies, we obtain industry labor union rates from the Union Membership and Coverage Database, maintained by Barry Hirsch and David Macpherson (www.unionstats.com) and divide the number of union members by the total employees to calculate labor union rates (Klasa et al., 2009; Chen, Kacperczyk, and Ortiz-Molina, 2011; Huang, Jiang, Lie, and Que, 2017). Labor union rate data is available only after 1982, so for firm-years before 1983 we use the union rate of 1983. Our inference is not affected when we drop years prior to 1983. We define firms with high labor union rates as those whose two-digit SIC industry labor union rate is above the median of the sample; otherwise firms are classified as low labor union rate firms. We capture the easiness of employee-firing decisions using the number of peer firms in same industry (3-digit NAICS) (Deng and Gao, 2013; Gao and Ma, 2016) because management will be less concerned about future hiring when well-trained employees are readily available from peer firms, which will make lay-off decisions relatively less costly.

The results of these tests are presented in Table 7. Panel A compares our main regression results (equation 6) for low versus high union rate firms. Consistent with our prediction, the coefficients on $DEC_t*\Delta Log(Sales_t)*POSTWDL_G$ is significantly negative only among firms with low labor union rate, which indicates that the good-faith exception has an impact on cost stickiness when employees are less unionized and have weaker negotiation power in mass firing decisions. The difference in the effect of good-faith exception across the partitions is statistically significant at the 10% level.

In Panel B of Table 7, we run a similar analysis by partitioning firms into those with low versus high peer firm groups. The coefficient on $DEC_t*\Delta Log(Sales_t)*POSTWDL_G$ is significantly negative only for the high peer firm group. The difference across the partitions is statistically significant at the 10% level. The results suggest that the good-faith exception has a significant impact on cost stickiness among firms that have a large number of peer firms in the same industry, that is, when firing decision is relatively easier due to the availability of well-trained labor from peer firms in the same industry.

In sum, our cross-sectional tests suggest that the effect of WDL on cost stickiness varies across firms. WDL has a greater impact on cost stickiness when employees collectively have less negotiation power in lay-off decisions or when firms can more easily fire their employees. These results strengthen our inference on the causal relationship between WDL and cost stickiness.

6. Conclusions

Extensive research provides evidence on how securities laws affect financial accounting outcomes (e.g., Bushee and Leuz, 2005; Bushman and Piotroski, 2006; Cohen et al., 2008). However, relatively few studies examine the effect of other types of laws on firm-level decisions. We examine the effect of state-level changes in the legal protection against unjust employment termination on firm-level resource allocation decisions and cost behavior. By exploiting the natural experiment created by the adoption of WDL across US states, we provide evidence that allows a *causal* inference on the relation between labor adjustment costs and cost stickiness, which is largely absent in the literature. More specifically, we show that the adoption of common law exceptions that limit unjust dismissals, especially those based on the good-faith exception, amplifies the asymmetric cost behavior. Our results are robust to a battery of sensitivity checks. We further find that the effect of WDL on cost stickiness is more pronounced when employees have less negotiation power in lay-off decisions or when firms can more easily fire their employees. Collectively, our results are consistent with the notion that increased labor adjustment costs after the adoption of WDL reduce cost sensitivity to activity declines, thereby increasing the degree of cost stickiness.

References

Acharya, V., R. Baghai, and K. Subramanian. 2014. "Wrongful discharge laws and innovation". *Review of Financial Studies.* 27 (1): 301–346.

Anderson, M., R. Banker, and S. Janakiraman. 2003. "Are selling, general, and administrative costs 'sticky'"? *Journal of Accounting Research.* 41: 47–63.

Anderson, M., R. Banker, R. Huang, and S. Janakiraman. 2007. "Cost behavior and fundamental analysis of SG&A costs". *Journal of Accounting, Auditing, and Finance.* 22: 1–28.

Autor, D. 2003. "Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing". *Journal of Labor Economics.* 21: 1–42.

Autor, D., J. Donohue, and S. Schwab. 2006. "The costs of wrongful-discharge laws". *The Review of Economics and Statistics.* 88: 211–231.

Autor, D., W. Kerr, and A. Kugler. 2007. "Does employment protection reduce productivity? Evidence from US states". *The Economic Journal.* 117: 189–217.

Balakrishnan, R., M. J. Petersen, and N. S. Soderstrom. 2004. "Does capacity utilization affect the "stickiness" of cost"? *Journal of Accounting, Auditing, and Finance.* 19: 283–300.

Banker, R., and D. Byzalov. 2014. "Asymmetric cost behavior". *Journal of Management Accounting Research.* 26: 43–79.

Banker, R., D. Byzalov, and L. Chen. 2013. "Employment protection legislation, adjustment costs and cross-country differences in cost behavior". *Journal of Accounting and Economics.* 55: 111–127.

Banker, R., D. Byzalov, M. Ciftci, and R. Mashruwala. 2014. "The moderating effect of prior sales changes on asymmetric cost behavior". *Journal of Management Accounting Research.* 26: 221–242.

Banker, R., and L. Chen. 2006. "Predicting earnings using a model based on cost variability and cost stickiness". *The Accounting Review.* 81: 285–307.

Bushee, B., and C. Leuz. 2005. "Economic consequences of SEC disclosure regulation: Evidence from the OTC bulletin board". *Journal of Accounting and Economic.* 39: 233–264.

Bushman, R., and J. Piotroski. 2006. "Financial reporting incentives for conservative accounting: The influence of legal and political institutions". *Journal of Accounting and Economics.* 42: 107–148.

Chen, H., M. Kacperczyk, and H. Ortiz-Molina. 2011. "Labor unions, operating flexibility, and the cost of equity". *Journal of Financial and Quantitative Analysis.* 46: 25–58.

Chen, F., Q. Li, and L. Xu. 2017. "Universal demand laws and the monitoring demand for accounting conservatism". Working paper, University of Toronto, Wuhan University, and Washington State University, Vancouver.

Chen, C., H. Lu, and T. Sougiannis. 2012. "The agency problem, corporate governance, and the asymmetrical behavior of selling, general, and administrative costs". *Contemporary Accounting Research.* 29: 252–282.

Cohen, D., A. Dey, and T. Lys. 2008. "Real and accrual-based earnings management in the pre- and post-Sarbanes-Oxley periods". *The Accounting Review.* 83: 757–787.

Deng, X., and H. Gao. 2013. "Nonmonetary benefits, quality of life, and executive compensation". *Journal of Financial and Quantitative Analysis.* 48: 197–218.

Dertouzos, J., E. Holland, and P. Ebener. 1988. "The legal and economic consequences of wrongful termination". *Rand Corporation document R-3602-ICJ*. Santa Monica, CA: Rand Corporation.

Dierynck, B., W. Landsman, and A. Renders. 2012. “Do managerial incentives drive cost behavior? Evidence about the role of the zero earnings benchmark for labor cost behavior in Belgian private firms”. *The Accounting Review.* 87: 1219–1246.

Edelman, L., S. Abraham, and H. Erlanger. 1992. “Professional construction of law: The inflated threat of wrongful discharge”. *Law and Society Review.* 26: 47–83.

Gao, H., and Y. Ma. 2016. “Human capital driven acquisition: Evidence from the Inevitable Disclosure Doctrine”. Working paper, University of International Business and Economics, and Fudan University.

Hilary, G., and K. Hui. 2009. “Does religion matter in corporate decision making in America”? *Journal of Financial Economics.* 93: 455–473.

Holthausen, R. 2009. “Accounting standards, financial reporting outcomes, and enforcement”. *Journal of Accounting Research.* 47: 447–458.

Huang, Q., F. Jiang, E. Lie, and T. Que. 2017. “The effect of labor unions on CEO compensation”. *Journal of Financial and Quantitative Analysis.* 52: 553–582.

Isidro, H., D. Nanda, and P. Wysocki. 2016. “Financial reporting differences around the world: What matters”? Working paper. Instituto Universitario de Lisboa and University of Miami.

Jung, D. 1997. “Jury verdicts in wrongful termination cases”. Public Law Research Institute Report, University of California Hastings College of the Law.

Kama, I., and D. Weiss. 2013. “Do earnings targets and managerial incentives affect sticky costs”? *Journal of Accounting Research.* 51: 201–224.

Klasa, S., H. Ortiz-Molina, M. Serfling, and S. Srinivasan. 2018. “Protection of trade secrets and capital structure decisions”. *Journal of Financial Economics.* 128: 266–286.

Konchitchki, Y. 2011. "Inflation and nominal financial reporting: implications for performance and stock prices". *The Accounting Review.* 86: 1045–1085.

Kugler, A., and G. Saint-Paul. 2004. "How do firing costs affect worker flows in a world with adverse selection"? *Journal of Labor Economics.* 22: 553–84.

Li, Y., Y. Lin, and L. Zhang. 2018. "Trade secrets law and corporate disclosure: causal evidence on the proprietary cost hypothesis". *Journal of Accounting Research.* 56: 265–308.

Miles, T. 2000. "Common law exceptions to employment at will and U.S. labor markets". *Journal of Law, Economics and Organizations.* 16: 74–101.

Stulz, R. 2009. "Securities laws, disclosure, and national capital markets in the age of financial globalization". *Journal of Accounting Research.* 47: 349–390.

Walsh, D., and J. Schwarz. 1996. "State common law wrongful discharge doctrines: Up-date, refinement, and rationales". *American Business Law Journal.* 33: 645–89.

Table 1
Wrongful Discharge Laws Adoption and Reversal Dates by States

	Public-Policy		Good-Faith		Implied-Contract	
State	**Start date**	**End date**	**Start date**	**End date**	**Start date**	**End date**
Alabama					Jul 1987	Current
Alaska	Feb 1986	Current	May 1983	Current	May 1983	Current
Arizona	Jun 1985	Current	Jun 1985	Current	Jun 1983	Apr 1984
Arkansas	Mar 1980	Current			Jun 1984	Current
California	Jan 1970	Current	Oct 1980	Current	Mar 1972	Current
Colorado	Sep 1985	Current			Oct 1983	Current
Connecticut	Jan 1980	Current	Jun 1980	Current	Oct 1985	Current
Delaware	Mar 1992	Current	Apr 1992	Current		
Florida						
Georgia						
Hawaii	Oct 1982	Current			Aug 1986	Current
Idaho	Apr 1977	Current	Aug 1989	Current	Apr 1977	Current
Illinois	Dec 1978	Current			Dec 1974	Current
Indiana	May 1973	Current			Aug 1987	Current
Iowa	Jul 1985	Current			Nov 1987	Current
Kansas	Jun 1981	Current			Aug 1984	Current
Kentucky	Nov 1983	Current			Aug 1983	Current
Louisiana			Jan 1998	Current		
Maine					Nov 1977	Current
Maryland	Jul 1981	Current			Jan 1985	Current
Massachusetts	May 1980	Current	Jul 1977	Current	May 1988	Current
Michigan	Jun 1976	Current			Jun 1980	Current
Minnesota	Nov 1986	Current			Apr 1983	Current
Mississippi	Jul 1987	Current			Jun 1992	Current
Missouri	Nov 1985	Current			Jan 1983	Feb 1988
Montana	Jan 1980	Current	Jan 1982	Current	Jun 1987	Current
Nebraska	Nov 1987	Current			Nov 1983	Current
Nevada	Jan 1984	Current	Feb 1987	Current	Aug 1983	Current
New Hampshire	Feb 1974	Current	Feb 1974	May 1980	Aug 1988	Current
New Jersey	Jul 1980	Current			May 1985	Current
New Mexico	Jul 1983	Current			Feb 1980	Current
New York					Nov 1982	Current
North Carolina	May 1985	Current				
North Dakota	Nov 1987	Current			Feb 1984	Current
Ohio	Mar 1990	Current			Apr 1982	Current
Oklahoma	Feb 1989	Current	May 1985	Feb 1989	Dec 1976	Current
Oregon	Jun 1975	Current			Mar 1978	Current
Pennsylvania	Mar 1974	Current				
Rhode Island						
South Carolina	Nov 1985	Current			Jun 1987	Current
South Dakota	Dec 1988	Current			Apr 1983	Current
Tennessee	Aug 1984	Current			Nov 1981	Current
Texas	Jun 1984	Current			Apr 1985	Current
Utah	Mar 1989	Current			May 1986	Current
Vermont	Sep 1986	Current			Aug 1985	Current

Table 1 (Continued)

	Public-Policy		Good-Faith		Implied-Contract	
State	**Start date**	**End date**	**Start date**	**State**	**Start date**	**End date**
Virginia	Jun 1985	Current			Sep 1983	Current
Washington	Jul 1984	Current			Aug 1977	Current
West Virginia	Jul 1978	Current			Apr 1986	Current
Wisconsin	Jan 1980	Current			Jun 1985	Current
Wyoming	Jul 1989	Current	Jan 1994	Current	Aug 1985	Current

This table presents the adoption (start) and reversal (end) dates of Wrongful Discharge Laws (WDL) for each state, obtained from Autor et al. (2006). Three forms of WDL are considered: the public-policy, good-faith, and implied-contract exceptions.

Table 2
Sample Distribution

Panel A: Number of observations by states

State	Firm-years	Firms	State	Firm-years	Firms
Alaska	39	8	Tennessee	1,487	163
Alabama	718	70	Texas	11,037	1,233
Arkansas	527	44	Utah	889	115
Arizona	1,472	179	Virginia	3,327	306
California	14,579	1,929	Vermont	202	19
Colorado	2,931	419	Washington	1,539	179
Connecticut	3,509	337	Wisconsin	2,252	165
Delaware	495	43	West Virginia	229	21
Florida	5,139	652	Wyoming	44	9
Georgia	2,949	343	Total	121,728	12,919
Hawaii	278	23			
Iowa	864	73			
Idaho	310	28			
Illinois	6,525	562			
Indiana	1,843	154			
Kansas	810	97			
Kentucky	764	70			
Louisiana	739	69			
Massachusetts	5,646	633			
Maryland	1,870	221			
Maine	253	18			
Michigan	3,384	282			
Minnesota	3,695	374			
Missouri	2,245	191			
Mississippi	278	36			
Montana	113	12			
North Carolina	2,654	231			
North Dakota	119	9			
Nebraska	327	38			
New Hampshire	633	71			
New Jersey	6,700	701			
New Mexico	176	26			
Nevada	899	117			
New York	12,003	1,266			
Ohio	5,803	457			
Oklahoma	1,089	148			
Oregon	1,121	124			
Pennsylvania	5,820	525			
Rhode Island	539	46			
South Carolina	732	73			
South Dakota	132	10			

Table 2 (Continued)

Panel B: Number of observations by year

Year	Firms	Percentage
1970	1,917	1.6%
1971	2,361	1.9%
1972	2,453	2.0%
1973	2,609	2.1%
1974	2,771	2.3%
1975	2,901	2.4%
1976	4,233	3.5%
1977	4,381	3.6%
1978	4,371	3.6%
1979	4,249	3.5%
1980	4,107	3.4%
1981	3,984	3.3%
1982	3,998	3.3%
1983	3,963	3.3%
1984	4,060	3.3%
1985	4,091	3.4%
1986	4,074	3.3%
1987	4,196	3.4%
1988	4,382	3.6%
1989	4,391	3.6%
1990	4,299	3.5%
1991	4,285	3.5%
1992	4,382	3.6%
1993	4,585	3.8%
1994	4,770	3.9%
1995	4,908	4.0%
1996	5,073	4.2%
1997	5,459	4.5%
1998	5,394	4.4%
1999	5,081	4.2%
Total	121,728	100%

Table 2 (Continued)

Panel C: Number of observations by industry

Fama-French 49 Industry	Firm-years	Firms
Agriculture	481	58
Aircraft	943	66
Alcoholic Beverages	437	50
Almost Nothing	784	121
Apparel	2,520	249
Automobiles and Trucks	2,410	211
Business Services	5,927	779
Business Supplies	2,492	180
Candy and Soda	310	36
Chemicals	2,617	212
Coal	229	29
Communications	4,091	452
Computer Software	5,265	941
Computers	4,007	487
Construction	1,892	214
Construction Materials	4,786	396
Consumer Goods	3,274	294
Defense	232	20
Electrical Equipment	2,431	213
Electronic Equipment	6,914	681
Entertainment	2,112	319
Fabricated Products	910	79
Food Products	2,875	270
Healthcare	1,927	316
Machinery	4,913	448
Measuring and Control Equip	3,206	300
Medical Equipment	3,135	399
Nonmetallic and Industrial Metal	383	39
Personal Services	1,351	178
Petroleum and Natural Gas	5,111	639
Pharmaceutical Products	2,985	392
Precious Metals	374	42
Printing and Publishing	1,457	132
Recreational Products	1,234	151
Restaurants, Hotel, Motel	2,921	353
Retail	7,889	868
Rubber and Plastic Products	1,951	192
Shipbuilding, Railroad Eq	319	34
Shipping Containers	556	49
Steel Works, Etc.	2,531	212
FF 49 Industry	**Firm-years**	**Firms**
Textiles	1,657	148
Tobacco Products	155	13
Transportation	3,528	373
Utilities	8,888	405
Wholesale	5,614	650
Not classifiable	1,704	229
Total	121,728	12,919

This table presents the distribution of firm-years and firms in our sample by state (Panel A), by year (Panel B), and by industry (Panel C). Firms with D.C. headquarters are classified as Virginia firms. Industries are defined following Fama-French 49 industries.

Table 3
Summary Statistics

Panel A: Descriptive statistics (N=121,728)

Variables	Mean	S.D.	25%	Median	75%
XOPR (in millions US$)	402.71	1,779.40	12.35	52.93	213.34
Δ*Log(XOPR)*	0.06	0.22	-0.04	0.05	0.16
Sales (in millions US$)	477.87	2,100.05	13.75	61.05	254.21
Δ*Log(Sales)*	0.06	0.24	-0.05	0.05	0.16
$POSTWDL_P$	0.55	0.50	0.00	1.00	1.00
$POSTWDL_G$	0.16	0.37	0.00	0.00	0.00
$POSTWDL_C$	0.53	0.50	0.00	1.00	1.00
GDPGROWTH (%)	3.28	2.04	2.70	3.70	4.50
Assets (in millions US$)	525.69	2,667.59	11.54	48.99	230.99
AINT	-0.10	0.67	-0.55	-0.21	0.25
Employees (in thousands)	6.70	26.52	0.23	1.00	3.88
EINT	-4.13	0.71	-4.53	-4.06	-3.68
DEC_t	0.36	0.48	0.00	0.00	1.00
DEC_{t-1}	0.34	0.47	0.00	0.00	1.00
IDD	0.34	0.47	0.00	0.00	1.00

Table 3 (Continued)

Panel B: Correlations

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1)	$\Delta Log(XOPR_t)$	1.00								
(2)	$\Delta Log(Sales_t)$	0.85***	1.00							
(3)	$POSTWDL_P$	0.02***	0.02***	1.00						
(4)	$POSTWDL_G$	0.03***	0.03***	0.34***	1.00					
(5)	$POSTWDL_C$	0.04***	0.03***	0.47***	0.21***	1.00				
(6)	*GDPGROWTH*	0.10***	0.11***	-0.05***	-0.02***	0.02***	1.00			
(7)	*AINT*	0.03***	0.00	0.05***	0.04***	0.06***	0.01	1.00		
(8)	*EINT*	-0.00	-0.02***	-0.04***	-0.02***	-0.06***	0.00	-0.08***	1.00	
(9)	DEC_t	-0.59***	-0.67***	0.01*	0.00	-0.01	-0.14***	0.03***	0.01**	1.00
(10)	DEC_{t-1}	-0.26***	-0.21***	-0.01**	-0.01*	-0.01***	-0.06***	0.01***	0.02***	0.23***

This table reports summary statistics of the key variables. Panel A presents descriptive statistics of the variables and Panel B presents the Pearson correlation coefficients among them. See Appendix A for variable definitions. ***, ** and * indicate significance at 1%, 5% and 10%, respectively.

Table 4
Wrongful Discharge Laws and Cost Stickiness

	Dep. Var. = $\Delta Log(XOPR_t)$	
	(1)	(2)
$\Delta Log(Sales_t)$	0.979***	0.943***
	(33.71)	(30.35)
$\Delta Log(Sales_t)*WDL_P$	0.011	0.009
	(0.53)	(0.42)
$\Delta Log(Sales_t)*WDL_G$	-0.013	-0.022
	(-0.95)	(-1.31)
$\Delta Log(Sales_t)*WDL_C$	0.000	0.004
	(0.02)	(0.25)
$\Delta Log(Sales_t)*POSTWDL_P$	-0.002	-0.004
	(-0.16)	(-0.28)
$\Delta Log(Sales_t)*POSTWDL_G$	-0.022*	-0.023*
	(-1.85)	(-1.79)
$\Delta Log(Sales_t)*POSTWDL_C$	-0.016	-0.018*
	(-1.64)	(-1.70)
$\Delta Log(Sales_t)*GDPGROWTH_t$	-0.000	0.000
	(-0.04)	(0.08)
$\Delta Log(Sales_t)*AINT_t$	-0.084***	-0.083***
	(-11.30)	(-12.82)
$\Delta Log(Sales_t)*EINT_t$	-0.005	-0.006*
	(-1.39)	(-1.83)
$DEC_t*\Delta Log(Sales_t)$	-0.443***	-0.376***
	(-7.95)	(-5.79)
$DEC_t*\Delta Log(Sales_t)*WDL_P$	0.013	0.017
	(0.90)	(0.96)
$DEC_t*\Delta Log(Sales_t)*WDL_G$	0.004	0.023
	(0.17)	(0.81)
$DEC_t*\Delta Log(Sales_t)*WDL_C$	-0.012	-0.020
	(-0.63)	(-0.82)
$DEC_t*\Delta Log(Sales_t)*POSTWDL_P$	**0.015**	**0.018**
	(1.19)	**(1.37)**
$DEC_t*\Delta Log(Sales_t)*POSTWDL_G$	**-0.062****	**-0.059****
	(-2.42)	**(-2.21)**
$DEC_t*\Delta Log(Sales_t)*POSTWDL_C$	**-0.015**	**-0.010**
	(-1.12)	**(-0.69)**
$DEC_t**\Delta Log(Sales_t)*DEC_{t-1}$	0.151***	0.151***
	(10.50)	(10.59)
$DEC_t*\Delta Log(Sales_t)*GDPGROWTH_t$	0.004	0.003
	(0.63)	(0.51)
$DEC_t*\Delta Log(Sales_t)*AINT_t$	-0.103***	-0.108***
	(-8.99)	(-9.75)
$DEC_t*\Delta Log(Sales_t)*EINT_t$	-0.056***	-0.053***
	(-6.56)	(-6.26)

Table 4 (Continued)

	Dep. Var. = $\Delta Log(XOPR_t)$	
	(1)	(2)
Intercept	0.007***	0.012**
	(9.39)	(2.06)
State FE	No	Yes
Industry FE	No	Yes
Year FE	No	Yes
Year Interactions	Yes	Yes
N	121,728	121,728
Adj. R^2	76.3%	76.4%

This table presents the effect of the state-wide adoption of Wrongful Discharge Laws on asymmetric behavior of the changes in operating costs relative to the changes in sales. Year interactions are year fixed effects interacted with $\Delta Log(Sales_t)$ and $DEC_t*\Delta Log(Sales_t)$. See Appendix A for variable definitions. Standard errors are clustered by state. ***, ** and * indicate significance at 1%, 5% and 10%, respectively.

Table 5
Robustness Checks using Alternative Samples

Dep. Var. = $\Delta Log(XOPR_t)$	(1) Excluding firms headquartered in D.C.	(2) Excluding HQ-state change firms	(3) Using the HQ-state from Bill McDonald's dataset
$\Delta Log(Sales_t)$	0.944***	0.943***	0.943***
	(30.59)	(29.68)	(30.35)
$\Delta Log(Sales_t)*WDL_P$	0.009	0.009	0.009
	(0.41)	(0.41)	(0.42)
$\Delta Log(Sales_t)*WDL_G$	-0.022	-0.024	-0.022
	(-1.31)	(-1.37)	(-1.31)
$\Delta Log(Sales_t)*WDL_C$	0.004	0.007	0.004
	(0.25)	(0.38)	(0.25)
$\Delta Log(Sales_t)*POSTWDL_P$	-0.004	-0.000	-0.004
	(-0.26)	(-0.00)	(-0.28)
$\Delta Log(Sales_t)*POSTWDL_G$	-0.023*	-0.024*	-0.023*
	(-1.80)	(-1.77)	(-1.79)
$\Delta Log(Sales_t)*POSTWDL_C$	-0.018*	-0.015	-0.018*
	(-1.69)	(-1.37)	(-1.70)
$DEC_t*\Delta Log(Sales_t)$	-0.377***	-0.360***	-0.376***
	(-5.83)	(-5.71)	(-5.79)
$DEC_t*\Delta Log(Sales_t)*WDL_P$	0.017	0.013	0.017
	(0.95)	(0.72)	(0.96)
$DEC_t*\Delta Log(Sales_t)*WDL_G$	0.024	0.025	0.023
	(0.82)	(0.83)	(0.81)
$DEC_t*\Delta Log(Sales_t)*WDL_C$	-0.020	-0.022	-0.020
	(-0.82)	(-0.87)	(-0.82)
$DEC_t*\Delta Log(Sales_t)*POSTWDL_P$	**0.018**	**0.018**	**0.018**
	(1.40)	**(1.30)**	**(1.37)**
$DEC_t*\Delta Log(Sales_t)*POSTWDL_G$	**-0.059****	**-0.053***	**-0.059****
	(-2.23)	**(-2.00)**	**(-2.21)**
$DEC_t*\Delta Log(Sales_t)*POSTWDL_C$	**-0.010**	**-0.012**	**-0.010**
	(-0.70)	**(-0.74)**	**(-0.69)**
$DEC_t**\Delta Log(Sales_t)*DEC_{t-1}$	0.151***	0.148***	0.151***
	(10.59)	(10.01)	(10.59)
Intercept	0.012**	0.011*	0.012**
	(2.05)	(1.80)	(2.06)
Other controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Year Interactions	Yes	Yes	Yes
N	121,495	113,770	121,728
Adj. R^2	76.4%	76.7%	76.4%

In this table we examine the robustness of our results in Table 4 to alternative samples. The first model excludes firms headquartered in Washington D.C. In the second and third models, we examine whether the effect of WDL on cost stickiness is robust to potential changes in headquarters states, using Bill McDonald's 10-K dataset that spans from 1994 to 2010. The second model excludes all firms that have changed their headquarters-states in Bill McDonald's dataset, and the third model uses the headquarters-state from Bill McDonald's dataset for years 1994 through 1999 and that from the earliest year of Bill McDonald's dataset for years 1970 through 1993. Year interactions are year fixed effects interacted with $\Delta Log(Sales_t)$ and $DEC_t*\Delta Log(Sales_t)$. See Appendix A for variable definitions. Standard errors are clustered by state. ***, ** and * indicate significance at 1%, 5% and 10%.

Table 6
Alternative Specifications of Cost Stickiness Model

Panel A: Including additional state controls

	Dep. Var. = $\Delta Log(XOPR_t)$
$\Delta Log(Sales_t)$	0.805*** (15.31)
$\Delta Log(Sales_t)*WDL_P$	0.001 (0.02)
$\Delta Log(Sales_t)*WDL_G$	-0.029 (-1.38)
$\Delta Log(Sales_t)*WDL_C$	-0.005 (-0.34)
$\Delta Log(Sales_t)*POSTWDL_P$	-0.003 (-0.20)
$\Delta Log(Sales_t)*POSTWDL_G$	-0.023 (-1.61)
$\Delta Log(Sales_t)*POSTWDL_C$	-0.011 (-0.93)
$\Delta Log(Sales_t)*GDPGROWTH_t$	-0.001 (-0.09)
$\Delta Log(Sales_t)*AINT_t$	-0.087*** (-14.37)
$\Delta Log(Sales_t)*EINT_t$	-0.005 (-1.36)
$\Delta Log(Sales_t)*IDD_t$	-0.021 (-1.10)
$\Delta Log(Sales_t)*STGDPGROWTH_t$	0.002 (1.10)
$\Delta Log(Sales_t)*UNEMPRATE_t$	0.001 (0.20)
$DEC_t*\Delta Log(Sales_t)$	-0.295*** (-2.92)
$DEC_t*\Delta Log(Sales_t)*WDL_P$	0.018 (0.77)
$DEC_t*\Delta Log(Sales_t)*WDL_G$	0.027 (0.75)
$DEC_t*\Delta Log(Sales_t)*WDL_C$	-0.016 (-0.72)
$\boldsymbol{DEC_t*\Delta Log(Sales_t)*POSTWDL_P}$	**0.023** **(1.44)**
$\boldsymbol{DEC_t*\Delta Log(Sales_t)*POSTWDL_G}$	**-0.060*** **(-1.81)**
$\boldsymbol{DEC_t*\Delta Log(Sales_t)*POSTWDL_C}$	**-0.014** **(-0.85)**

Table 6 (Continued)

	Dep. Var. = $\Delta Log(XOPR_t)$
DEC_t**$\Delta Log(Sales_t)$*DEC_{t-1}	0.155***
	(10.20)
DEC_t*$\Delta Log(Sales_t)$*$GDPGROWTH_t$	0.002
	(0.16)
DEC_t*$\Delta Log(Sales_t)$*$AINT_t$	-0.110***
	(-9.49)
DEC_t*$\Delta Log(Sales_t)$*$EINT_t$	-0.055***
	(-6.01)
DEC_t*$\Delta Log(Sales_t)$*IDD_t	0.010
	(0.61)
DEC_t*$\Delta Log(Sales_t)$*$STGDPGROWTH_t$	-0.000
	(-0.03)
DEC_t*$\Delta Log(Sales_t)$*$UNEMPRATE_t$	-0.002
	(-0.32)
Intercept	0.006
	(0.80)
State FE	Yes
Industry FE	Yes
Year FE	Yes
Year Interactions	Yes
N	104,666
Adj. R^2	75.2%

Table 6 (Continued)

Panel B: Banker et al. (2014) Model A and Banker et al. (2013) Model B

Dep. Var. = $\Delta Log(XOPR_t)$	(1) Banker et al. (2014) Model A	(2) Banker et al. (2013) Model B
$INC_{t-1}*\Delta Log(Sales_t)$	1.021***	0.976***
	(31.64)	(34.73)
$INC_{t-1}*\Delta Log(Sales_t)*WDL_P$	-0.015	-0.002
	(-0.70)	(-0.08)
$INC_{t-1}*\Delta Log(Sales_t)*WDL_G$	-0.028	-0.017
	(-1.57)	(-0.91)
$INC_{t-1}*\Delta Log(Sales_t)*WDL_C$	-0.004	-0.006
	(-0.31)	(-0.50)
$INC_{t-1}*\Delta Log(Sales_t)*POSTWDL_P$	-0.004	-0.015
	(-0.24)	(-0.95)
$INC_{t-1}*\Delta Log(Sales_t)*POSTWDL_G$	-0.001	-0.010
	(-0.10)	(-0.71)
$INC_{t-1}*\Delta Log(Sales_t)*POSTWDL_C$	0.000	0.000
	(0.03)	(0.06)
$INC_{t-1}*DEC*\Delta Log(Sales_t)$	-0.160***	-0.369***
	(-3.05)	(-5.98)
$INC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_P$	0.004	0.017
	(0.12)	(0.64)
$INC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_G$	0.017	0.010
	(0.44)	(0.29)
$INC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_C$	-0.012	0.001
	(-0.48)	(0.02)
$\boldsymbol{INC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_P}$	**0.050**	**0.042**
	(1.55)	**(1.52)**
$\boldsymbol{INC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_G}$	**-0.114*****	**-0.108*** **
	(-2.84)	**(-2.96)**
$\boldsymbol{INC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_C}$	**-0.033**	**-0.039****
	(-1.67)	**(-2.33)**
$DEC_{t-1}*\Delta Log(Sales_t)$	0.853***	0.794***
	(14.66)	(16.63)
$DEC_{t-1}*\Delta Log(Sales_t)*WDL_P$	0.028	0.042
	(0.85)	(1.68)
$DEC_{t-1}*\Delta Log(Sales_t)*WDL_G$	-0.027	-0.016
	(-0.70)	(-0.49)
$DEC_{t-1}*\Delta Log(Sales_t)*WDL_C$	0.026	0.016
	(0.79)	(0.56)
$DEC_{t-1}*\Delta Log(Sales_t)*POSTWDL_P$	0.030	0.024
	(1.16)	(1.11)

Table 6 (Continued)

Dep. Var. = $\Delta Log(XOPR_t)$	(1) Banker et al. (2014) Model A	(2) Banker et al. (2013) Model B
$DEC_{t-1}*\Delta Log(Sales_t)*POSTWDL_G$	-0.079*	-0.085**
	(-1.97)	(-2.12)
$DEC_{t-1}*\Delta Log(Sales_t)*POSTWDL_C$	-0.048*	-0.041*
	(-1.84)	(-1.76)
$DEC_{t-1}*DEC*\Delta Log(Sales_t)$	0.051	-0.131
	(0.65)	(-1.57)
$DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_P$	-0.010	-0.013
	(-0.37)	(-0.42)
$DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_G$	0.023	0.024
	(0.45)	(0.51)
$DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_C$	-0.051	-0.037
	(-1.44)	(-0.94)
$\boldsymbol{DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_P}$	**0.011**	**-0.013**
	(0.43)	**(-0.44)**
$\boldsymbol{DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_G}$	**0.026**	**0.029**
	(0.63)	**(0.74)**
$\boldsymbol{DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_C}$	**0.015**	**0.025**
	(0.46)	**(0.91)**
$\Delta Log(Sales_t)*GDPGROWTH_t$		0.001
		(0.35)
$\Delta Log(Sales_t)*AINT_t$		-0.078***
		(-13.30)
$\Delta Log(Sales_t)*EINT_t$		-0.004
		(-1.30)
$DEC_t*\Delta Log(Sales_t)*GDPGROWTH_t$		0.002
		(0.45)
$DEC_t*\Delta Log(Sales_t)*AINT_t$		-0.111***
		(-10.75)
$DEC_t*\Delta Log(Sales_t)*EINT_t$		-0.056***
		(-6.34)
Intercept	0.015***	0.014**
	(3.24)	(2.62)
State FE	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes
Year Interactions	Yes	Yes
N	121,728	121,728
Adj. R^2	76.0%	77.2%

Table 6 (Continued)

Panel C: Including standalone variables and lower order interactions

Dep. Var. = $\Delta Log(XOPR_t)$	(1) Banker et al. (2014) Model A	(2) Banker et al. (2013) Model B
$INC_{t-1}*\Delta Log(Sales_t)$	0.995***	0.953***
	(32.79)	(32.28)
$INC_{t-1}*\Delta Log(Sales_t)*WDL_P$	-0.006	0.008
	(-0.24)	(0.35)
$INC_{t-1}*\Delta Log(Sales_t)*WDL_G$	-0.012	-0.005
	(-0.66)	(-0.23)
$INC_{t-1}*\Delta Log(Sales_t)*WDL_C$	-0.002	-0.007
	(-0.12)	(-0.57)
$INC_{t-1}*\Delta Log(Sales_t)*POSTWDL_P$	-0.012	-0.022
	(-0.55)	(-1.12)
$INC_{t-1}*\Delta Log(Sales_t)*POSTWDL_G$	-0.022	-0.026
	(-1.34)	(-1.61)
$INC_{t-1}*\Delta Log(Sales_t)*POSTWDL_C$	-0.011	-0.003
	(-0.94)	(-0.34)
$INC_{t-1}*DEC*\Delta Log(Sales_t)$	-0.121**	-0.309***
	(-2.41)	(-5.27)
$INC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_P$	-0.000	0.009
	(-0.00)	(0.29)
$INC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_G$	0.012	0.001
	(0.26)	(0.03)
$INC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_C$	-0.004	0.001
	(-0.16)	(0.03)
$\boldsymbol{INC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_P}$	**0.054**	**0.051***
	(1.58)	**(1.68)**
$\boldsymbol{INC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_G}$	**-0.116****	**-0.104****
	(-2.30)	**(-2.30)**
$\boldsymbol{INC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_C}$	**-0.039***	**-0.039***
	(-1.93)	**(-2.00)**
$DEC_{t-1}*\Delta Log(Sales_t)$	0.859***	0.812***
	(14.59)	(16.09)
$DEC_{t-1}*\Delta Log(Sales_t)*WDL_P$	0.038	0.052*
	(1.06)	(1.97)
$DEC_{t-1}*\Delta Log(Sales_t)*WDL_G$	-0.013	-0.008
	(-0.33)	(-0.22)
$DEC_{t-1}*\Delta Log(Sales_t)*WDL_C$	0.027	0.013
	(0.79)	(0.44)
$DEC_{t-1}*\Delta Log(Sales_t)*POSTWDL_P$	0.023	0.017
	(0.80)	(0.72)
$DEC_{t-1}*\Delta Log(Sales_t)*POSTWDL_G$	-0.099**	-0.099**
	(-2.05)	(-2.14)

Table 6 (Continued)

Dep. Var. = $\Delta Log(XOPR_t)$	(1) Banker et al. (2014) Model A	(2) Banker et al. (2013) Model B
$DEC_{t-1}*\Delta Log(Sales_t)*POSTWDL_C$	-0.059** (-2.14)	-0.043* (-1.72)
$DEC_{t-1}*DEC*\Delta Log(Sales_t)$	-0.016 (-0.19)	-0.184* (-1.96)
$DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_P$	-0.020 (-0.67)	-0.023 (-0.74)
$DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_G$	0.024 (0.41)	0.021 (0.39)
$DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*WDL_C$	-0.045 (-1.16)	-0.035 (-0.85)
$DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_P$	**0.009** **(0.30)**	**-0.006** **(-0.21)**
$DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_G$	**0.028** **(0.51)**	**0.035** **(0.66)**
$DEC_{t-1}*DEC_t*\Delta Log(Sales_t)*POSTWDL_C$	**0.018** **(0.51)**	**0.025** **(0.77)**
$\Delta Log(Sales_t)*GDPGROWTH_t$		0.001 (0.22)
$\Delta Log(Sales_t)*AINT_t$		-0.111*** (-11.01)
$\Delta Log(Sales_t)*EINT_t$		-0.006 (-1.19)
$DEC_t*\Delta Log(Sales_t)*GDPGROWTH_t$		0.001 (0.15)
$DEC_t*\Delta Log(Sales_t)*AINT_t$		-0.072*** (-4.44)
$DEC_t*\Delta Log(Sales_t)*EINT_t$		-0.046*** (-4.03)
WDL_P	*Omitted*	*Omitted*
WDL_G	*Omitted*	*Omitted*
WDL_C	*Omitted*	*Omitted*
$POSTWDL_P$	0.002 (0.61)	0.002 (1.01)
$POSTWDL_G$	0.007 (1.55)	0.006 (1.67)
$POSTWDL_C$	0.004* (1.80)	0.002 (0.90)
DEC_{t-1}	-0.009*** (-3.82)	-0.009*** (-3.51)
$GDPGROWTH_t$	0.000 (0.72)	-0.000 (-0.03)

Table 6 (Continued)

	Dep. Var. = $\Delta Log(XOPR_t)$	
	(1)	(2)
$AINT_t$	-0.005***	0.017***
	(-2.87)	(5.91)
$EINT_t$	-0.002**	0.001
	(-2.35)	(0.86)
DEC_t	0.070***	-0.001
	(7.98)	(-0.24)
DEC_t*WDL_P	0.001	0.004
	(0.10)	(1.15)
DEC_t*WDL_G	0.005	0.006
	(0.74)	(1.33)
DEC_t*WDL_C	0.003	-0.001
	(0.76)	(-0.35)
$DEC_t*POSTWDL_P$	-0.000	-0.002
	(-0.02)	(-0.55)
$DEC_t*POSTWDL_G$	-0.014**	-0.011**
	(-2.21)	(-2.54)
$DEC_t*POSTWDL_C$	-0.008**	-0.003
	(-2.27)	(-1.26)
DEC_t*DEC_{t-1}	-0.011***	-0.010***
	(-7.32)	(-6.71)
$DEC_t*GDPGROWTH_t$	-0.001***	-0.000
	(-3.48)	(-0.80)
DEC_t*AINT_t	0.040***	-0.015***
	(10.32)	(-9.34)
DEC_t*EINT_t	0.018***	0.002
	(9.09)	(1.43)
Intercept	0.006	0.019***
	(1.37)	(2.69)
State FE	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes
Year Interactions	Yes	Yes
N	121,728	121,728
Adj. R^2	76.6%	77.3%

In this table we assess the robustness of our results in Table 4 to alternative cost stickiness models. Panel A adds additional state-level controls to the base model in Table 4. Panel B employs Model A of Banker et al. (2014) and Model B of Banker et al. (2014). Panel C includes standalone variables and lower order interactions to the models in Panel B. In Panel A, Year interactions are year fixed effects interacted with $\Delta Log(Sales_t)$ and $DEC_t*\Delta Log(Sales_t)$. In Panels B and C, Year interactions are year fixed effects interacted with $INC_{t-1}*\Delta Log(Sales_t)$, $INC_{t-1}*DEC*\Delta Log(Sales_t)$, $DEC_{t-1}*\Delta Log(Sales_t)$, and $DEC_{t-1}*DEC*\Delta Log(Sales_t)$. See Appendix A for variable definitions. Standard errors are clustered by state. ***, ** and * indicate significance at 1%, 5% and 10%, respectively.

Table 7
Cross-sectional Variation in WDL Effect on Cost Stickiness

Panel A: Low versus high labor union rate

	(1)	(2)
	Labor Union Rate	
Dep. Var. = Δ*Log(XOPR$_t$)*	Low	High
$\Delta Log(Sales_t)$	1.020***	0.837***
	(22.53)	(17.64)
$\Delta Log(Sales_t)*WDL_P$	0.010	0.009
	(0.45)	(0.46)
$\Delta Log(Sales_t)*WDL_G$	-0.027	-0.013
	(-1.54)	(-0.49)
$\Delta Log(Sales_t)*WDL_C$	-0.000	0.020
	(-0.02)	(1.01)
$\Delta Log(Sales_t)*POSTWDL_P$	0.006	-0.026
	(0.39)	(-1.65)
$\Delta Log(Sales_t)*POSTWDL_G$	0.001	-0.071***
	(0.09)	(-2.95)
$\Delta Log(Sales_t)*POSTWDL_C$	-0.013	-0.033*
	(-1.00)	(-1.71)
$\Delta Log(Sales_t)*GDPGROWTH_t$	-0.002	0.005
	(-0.76)	(0.95)
$\Delta Log(Sales_t)*AINT_t$	-0.087***	-0.072***
	(-12.32)	(-4.14)
$\Delta Log(Sales_t)*EINT_t$	0.008	-0.023**
	(1.00)	(-2.41)
$DEC_t*\Delta Log(Sales_t)$	-0.416***	-0.348***
	(-4.96)	(-4.76)
$DEC_t*\Delta Log(Sales_t)*WDL_P$	0.009	0.037**
	(0.37)	(2.04)
$DEC_t*\Delta Log(Sales_t)*WDL_G$	0.034	0.006
	(0.70)	(0.21)
$DEC_t*\Delta Log(Sales_t)*WDL_C$	-0.000	-0.043
	(-0.01)	(-1.66)
$DEC_t*\Delta Log(Sales_t)*POSTWDL_P$ (1)	**0.033***	**-0.003**
	(1.68)	**(-0.14)**
$DEC_t*\Delta Log(Sales_t)*POSTWDL_G$ (2)	**-0.092****	**-0.001**
	(-2.21)	**(-0.02)**
$DEC_t*\Delta Log(Sales_t)*POSTWDL_C$ (3)	**-0.023**	**0.012**
	(-1.09)	**(0.47)**
$DEC_t**\Delta Log(Sales_t)*DEC_{t-1}$	0.181***	0.113***
	(8.15)	(7.22)
$DEC_t*\Delta Log(Sales_t)*GDPGROWTH_t$	0.011	-0.008
	(1.61)	(-1.20)
$DEC_t*\Delta Log(Sales_t)*AINT_t$	-0.124***	-0.091***
	(-9.39)	(-5.91)
$DEC_t*\Delta Log(Sales_t)*EINT_t$	-0.036***	-0.070***
	(-3.36)	(-5.85)

Table 7 (Continued)

	(1)	(2)
	Labor Union Rate	
Dep. Var. = $\Delta Log(XOPR_t)$	Low	High
Intercept	0.005	0.015***
	(0.78)	(5.53)
Two-tailed p-value: Low (1) ≠ High (1)	*0.20*	
Two-tailed p-value: Low (2) ≠ High (2)	*0.06**	
Two-tailed p-value: Low (3) ≠ High (3)	*0.30*	
State FE	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes
Year Interactions	Yes	Yes
N	63,159	57,255
Adj. R^2	75.5%	78.3%

Table 7 (Continued)

Panel B: Low versus high number of peer firms

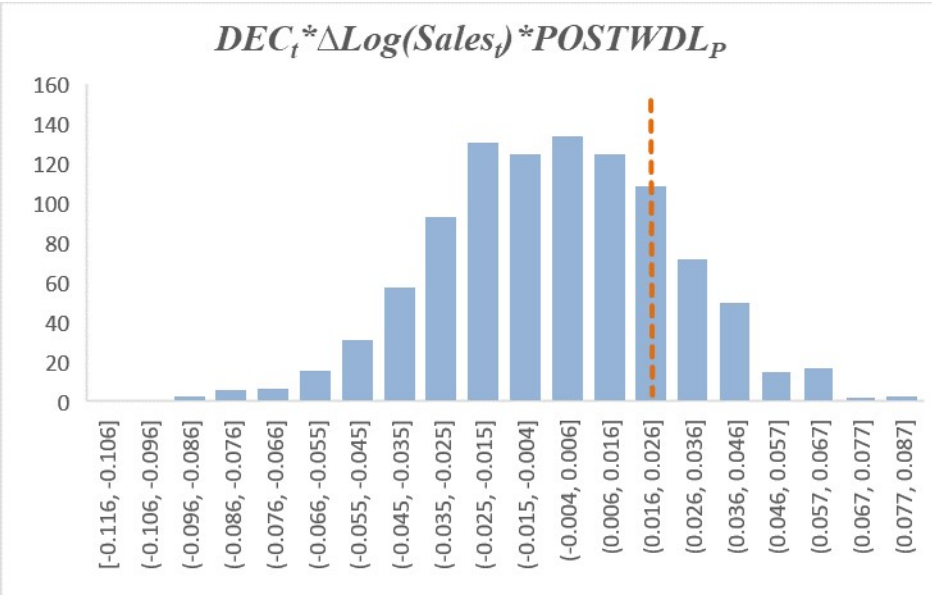
	(1)	(2)
	Peer Firms	
Dep. Var. = $\Delta Log(XOPR_t)$	Low	High
$\Delta Log(Sales_t)$	0.983***	0.885***
	(22.67)	(22.76)
$\Delta Log(Sales_t)*WDL_P$	-0.011	0.007
	(-0.53)	(0.25)
$\Delta Log(Sales_t)*WDL_G$	-0.024	-0.033
	(-1.12)	(-1.23)
$\Delta Log(Sales_t)*WDL_C$	0.015	0.007
	(0.80)	(0.45)
$\Delta Log(Sales_t)*POSTWDL_P$	-0.009	-0.007
	(-0.62)	(-0.30)
$\Delta Log(Sales_t)*POSTWDL_G$	-0.009	0.013
	(-0.53)	(0.80)
$\Delta Log(Sales_t)*POSTWDL_C$	0.001	-0.007
	(0.11)	(-0.42)
$\Delta Log(Sales_t)*GDPGROWTH_t$	0.003	-0.002
	(0.80)	(-0.48)
$\Delta Log(Sales_t)*AINT_t$	-0.072***	-0.082***
	(-8.84)	(-8.91)
$\Delta Log(Sales_t)*EINT_t$	-0.000	-0.020***
	(-0.03)	(-3.76)
$DEC_t*\Delta Log(Sales_t)$	-0.315***	-0.373***
	(-4.80)	(-4.81)
$DEC_t*\Delta Log(Sales_t)*WDL_P$	0.030	0.007
	(1.01)	(0.41)
$DEC_t*\Delta Log(Sales_t)*WDL_G$	0.008	0.022
	(0.21)	(0.69)
$DEC_t*\Delta Log(Sales_t)*WDL_C$	-0.034	-0.014
	(-1.28)	(-0.61)
$\boldsymbol{DEC_t*\Delta Log(Sales_t)*POSTWDL_P}$ (1)	**0.029**	**0.021**
	(1.14)	**(1.30)**
$\boldsymbol{DEC_t*\Delta Log(Sales_t)*POSTWDL_G}$ (2)	**-0.002**	**-0.084*****
	(-0.04)	**(-2.80)**
$\boldsymbol{DEC_t*\Delta Log(Sales_t)*POSTWDL_C}$ (3)	**-0.014**	**-0.008**
	(-0.59)	**(-0.44)**
$DEC_t**\Delta Log(Sales_t)*DEC_{t-1}$	0.104***	0.186***
	(8.69)	(11.54)
$DEC_t*\Delta Log(Sales_t)*GDPGROWTH_t$	0.000	0.006
	(0.04)	(0.69)
$DEC_t*\Delta Log(Sales_t)*AINT_t$	-0.093***	-0.121***
	(-6.01)	(-10.15)
$DEC_t*\Delta Log(Sales_t)*EINT_t$	-0.043***	-0.051***
	(-4.57)	(-4.72)

Table 7 (Continued)

	(1)		(2)
		Peer Firms	
Dep. Var. = $\Delta Log(XOPR_t)$	Low		High
Intercept	0.011**		0.006
	(2.30)		(0.48)
Two-tailed p-value: Low (1) ≠ High (1)		*0.79*	
Two-tailed p-value: Low (2) ≠ High (2)		*0.09**	
Two-tailed p-value: Low (3) ≠ High (3)		*0.85*	
State FE	Yes		Yes
Industry FE	Yes		Yes
Year FE	Yes		Yes
	Yes		Yes
N	62,265		59,463
Adj. R^2	82.1%		72.1%

In this table we examine whether the effect of WDL on cost stickiness varies across firms. Panel A compares the WDL effect between low versus high labor union rate firms. Panel B compares the WDL effect between firms in industries with smaller versus larger number of peer firms. Labor union rate is defined as high if the number of labor union members divided by total employees is above the median of our sample; and is defined as low otherwise. Peer firms are the number of firms in same industry (3-digit NAICS) (Deng and Gao 2013; Gao and Ma 2016). We define peer firms as high if it is above the median of our sample; and as low otherwise. Year interactions are year fixed effects interacted with $\Delta Log(Sales_t)$ and $DEC_t*\Delta Log(Sales_t)$. See Appendix A for variable definitions. Standard errors are clustered by state. ***, ** and * indicate significance at 1%, 5% and 10%, respectively.

Figure 1
Regression Coefficient of Post-WDL Adoption Relative to its Distribution based on Pseudo-WDL Adoption



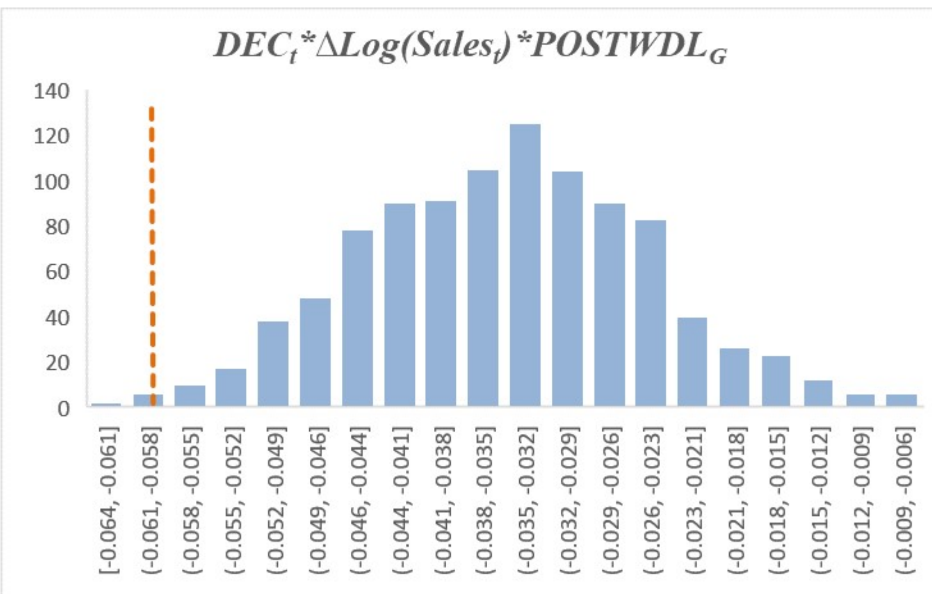
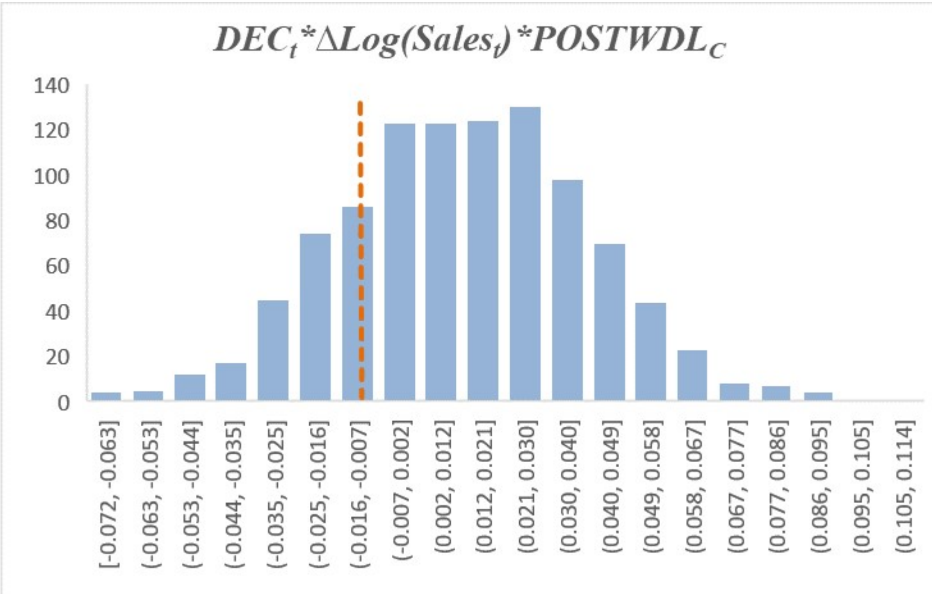


Figure 1 (Continued)



This figure compares the regression coefficient of post-WDL adoption in Table 4 (i.e., DEC_t*$\Delta$$Log(Sales_t)$*$POSTWDL_P$, DEC_t*$\Delta$$Log(Sales_t)$*$POSTWDL_G$, and DEC_t*$\Delta$$Log(Sales_t)$*$POSTWDL_C$) with its distribution based on pseudo-WDL adoption. The actual regression coefficient is shown as a vertical dotted line, and its distribution based on pseudo-WDL adoption is shown as bars. The distribution is obtained by randomly assigning firm-years to one or zero $POSTWDL_P$ ($POSTWDL_G$ or $POSTWDL_C$), such that the number of one and zero $POSTWDL_P$ ($POSTWDL_G$ or $POSTWDL_C$) firm-years remains identical to the original sample. We then run the regression in Table 4 and obtain the coefficients for three-types of post-WDL adoption. We repeat this procedure 1,000 times to obtain the distributions of the coefficients.

Appendix A
Variable Definition

Variable	Definition	Source
$XOPR_t$	Operating costs in million US$ in year t, adjusted for inflation	Compustat
$\Delta log(XOPR_t)$	$log(XOPR_t) - log(XOPR_{t-1})$	Compustat
$Sales_t$	Sales revenue in million US$ in year t, adjusted for inflation	Compustat
$\Delta log(Sales_t)$	$log(Sales_t) - log(Sales_{t-1})$	Compustat
WDL_I	I = P (public-policy exception), G (good-faith exception), or C (implied-contract exception); coded as one if a firm's headquarters state has adopted WDL_I, and zero otherwise	Autor et al. (2006)
$POSTWDL_I$	I = P (public-policy exception), G (good-faith exception), or C (implied-contract exception); coded as one if a firm's headquarters state has adopted WDL_I, and the firm's fiscal yearend date falls between one year after the state WDL adoption and its reversal date, if there is any, and zero otherwise	Autor et al. (2006)
$GDPGROWTH_t$	US real GDP growth rate (%) in year t	US Bureau of Economic Analysis
$Assets_t$	Total assets in million US$ in year t, adjusted for inflation	Compustat
$AINT_t$	$log(Assets_t/Sales_t)$ in year t	Compustat
$Employees_t$	The number of employees in thousands in year t	Compustat
$EINT_t$	$log(Employees_t/Sales_t)$ in year t	Compustat
DEC_t	An indicator variable that takes one if the firm's current year inflation adjusted sales is lower than its prior year inflation adjusted sales, and zero otherwise	Compustat
DEC_{t-1}	An indicator variable that takes one if the firm's prior year inflation adjusted sales is lower than the inflation adjusted sales in the year before, and zero otherwise	Compustat
INC_{t-1}	An indicator variable that takes one if the firm's prior year inflation adjusted sales is greater than the inflation adjusted sales in the year before, and zero otherwise	Compustat
IDD_t	Inevitable Disclosure Doctrine, coded as one if a firm's fiscal yearend date falls between one year after the state IDD adoption date and its reversal date, if there is any, and zero otherwise	Klasa et al. (2018)
$STGDPGROWTH_t$	State nominal GDP growth rate (%) in year t	US Bureau of Economic Analysis
$UNEMPRATE_t$	State unemployment rate (%) in year t	US Bureau of Labor Statistics