Employment Protection and Share Repurchases:

Evidence from Wrongful Discharge Laws*

Viet A. Dang[a], Amedeo De Cesari[b], and Hieu V. Phan[c]

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[a] Alliance Manchester Business School, University of Manchester, vietanh.dang@manchester.ac.uk.

[b] Alliance Manchester Business School, University of Manchester, amedeo.decesari@manchester.ac.uk.

[c] Manning School of Business, University of Massachusetts Lowell, hieu_phan@uml.edu.

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Abstract

We use the staggered adoption of Wrongful Discharge Laws (WDLs) by different U.S. state courts as a quasi-natural experiment to examine the causal relation between employee firing costs and corporate payout policy. We find that greater employment protection and higher firing costs imposed by WDLs lead to higher stock repurchases. Further analysis indicates that since higher firing costs exacerbate the conflict of interests between shareholders and workers and potentially lead to rent extraction by the latter, firms increase share buybacks to reduce the risk of wealth transfer from shareholders to workers.

JEL Classifications: G35, G32, J63, K31.

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1. Introduction

Recent studies document that firms facing higher unemployment risk or greater labor market frictions, in the form of higher labor adjustment costs (LACs), adopt more conservative corporate policies (e.g., Agrawal and Matsa, 2013; Simintzi, Vig, and Volpin, 2015; Serfling, 2016; Bai, Fairhurst, and Serfling, 2017). Firms may incur implicit costs when discharging workers, such as wrongful termination lawsuits, discrimination claims, or collective bargaining power. Labor firing costs can, therefore, undermine a firm's operating flexibility and adversely affect corporate employment, investment, and financial policies. However, it is unclear whether and how labor firing costs affect corporate payouts, another important corporate policy. Our research attempts to answer these questions.

The effect of employee firing cost on corporate payouts is ambiguous *ex ante*. On one hand, faced with unemployment risk, workers may demand a wage premium as compensation for bearing such risk (Titman, 1984; Berk, Staton, and Zechner, 2010). Indeed, due to the substantial human capital costs associated with financial distress, firms tend to adopt more conservative financial policies and pay higher wages (Chemmanur, Cheng, and Zhang, 2013). Higher firing costs can, however, lower workers' unemployment risk, potentially leading to lower wage premiums. As the result of labor cost savings, firms adopt less conservative financial policies (Agrawal and Matsa, 2013) and should therefore be able to increase payouts to shareholders. Alternatively, higher firing costs may lead to wealth transfer from shareholders to workers that are harder to discharge, creating a conflict of interests between them. To discourage those workers from bringing lawsuits against the firms and mitigate their rent capture behavior, firms have an incentive to distribute cash flows by increasing payouts to shareholders.

On the other hand, Serfling (2016) argues that higher firing costs can heighten financial distress in two possible ways. First, since firms typically discharge employees when they experience cash flow shortfalls (Ofek, 1993; Kang and Shivdasani, 1997), higher firing costs may exacerbate corporate illiquidity. Second, higher firing costs can weaken firms' ability to reduce employment when they really need to do so, such as during economic downturns (Bentolila and Bertola, 1990; Autor, Donohue, and Schwab, 2006; Messina and Vallanti, 2007), making labor more rigid. Thus, higher employee discharge costs can render labor costs more fixed in nature, and increase operating leverage and distress risk (Mandelker and Rhee, 1984; Mauer and Triantis, 1994; Kahl, Lunn, and Nilsson, 2014; Serfling, 2016). To the extent that firing costs increase financial distress risk, firms may take precaution by reducing corporate payouts. Given the possible opposing effects of firing costs on corporate payouts, they need to be determined empirically.

Since payouts and discharging employees are important corporate decisions, they can be jointly determined or both of these decisions can be correlated with unobserved firm characteristics, such as investment opportunities. These sources of endogeneity can bias regression results, leading to invalid inference. To overcome this empirical challenge and to identify the causal relation between firing costs and corporate payouts, we follow previous studies (Dertouzos and Karoly, 1992; Autor, 2003; Autor, Kerr, and Kugler, 2007; Acharya, Baghai, and Subramanian, 2014; Serfling, 2016) and exploit the exogenous changes in employment protection and firing costs following the adoption of WDLs by U.S. state courts over the period 1971–1995.

WDLs include three common law exceptions to "at-will employment" that protect employees from wrongful discharge: the good faith, implied contract, and public policy

exceptions. Among these exceptions, we focus on the adoption of the good faith exception since it has the most far reaching effect of the three (Dertouzos and Karoly, 1992; Kugler and Saint-Paul, 2004). This law applies in cases when a court determines that an employer fires an employee out of bad faith, retaliation, or malice. As a result, the plaintiff employees can recover not only contractual losses but also punitive damages. Previous studies report that WDLs increase firing costs substantially. Jung (1997) finds that, on average, plaintiffs won $1.29 million in 1996, and Boxold (2008) reports that plaintiff won maximum award of $5.4 million over the period 2001–2007. Furthermore, WDLs motivate workers to bring more wrongful termination lawsuits, which increase the number of lawsuits and legal liability a firm may have to face at a point in time.

We consider two major forms of payouts: stock repurchases and dividends. Jagannathan, Stephens, and Weisbach (2000) document that dividends are sticky and are typically associated with permanent cash flows. Firms do not usually reduce or skip dividend because doing so would send a negative signal about firms' future prospects to investors that leads to lower stock price. Indeed, prior research has provided ample evidence of firms' dividend smoothing behavior (Lintner, 1956; Brav et al., 2005; Leary and Michaely, 2011). On the other hand, stock repurchases tend to be associated with business cycles or temporary cash flows. Importantly, between the two forms of payout, stock repurchases give firms more flexibility. Grullon and Michaely (2002) report that U.S. firms have gradually substituted share repurchases for dividends. To the extent that WDLs reduce wage premiums or create a conflict of interests between shareholders and workers, leading to an increase in corporate payouts, we do not expect the cash flows associated with WDLs to be permanent. Likewise, if increased firing costs due to the WDL adoption heighten financial distress risk, firms are more likely to reduce stock

repurchases to temporarily increase cash reserves rather than reducing dividends permanently. These discussions suggest that the effects of the adoption of WDLs on corporate payouts, if any, are more pronounced for stock repurchases relative to dividend payments. For this reason, we focus our analysis on the impact of WDLs on stock repurchases, but we also consider the effects of these laws on dividends and total payout in the robustness checks.

We begin our analysis by estimating a difference-in-differences model that captures the relation between the passage of WDLs and share repurchases. We consider two specifications, with and without conventional firm-level determinants of stock repurchases; the use of the specification without the firm controls enables us to address the concern that these controls may be endogenous (e.g., Gormley and Matsa, 2017). To account for a possibility that WDLs are correlated with time-invariant heterogeneity as well as time-varying industry, local, and macroeconomic conditions, our stock repurchase model further includes firm, industry-by-year, region-by-year, and state of incorporation-by-year fixed effects. We find that, among the three exceptions to "at-will employment", only the good faith exception has a positive and significant effect on stock repurchases, consistent with our first hypothesis. The effect of the good faith exception on stock repurchases is economically important. Following the adoption of this exception, firms increase stock repurchases by 0.00172–0.00164 on average, which is equivalent to approximately 30% of the sample mean. Our findings are insensitive to controlling for other state-level variables, such as GDP per capita, GDP growth rate, political balance, union membership, union coverage, and the passage of right-to-work (RTW) laws.

It is possible that both the adoption of WDLs and stock repurchases follow pre-treatment time trends, implying that our documented results could arise from a spurious relation between the two. To rule out this possibility, we perform pre-treatment trend analysis and examine the

dynamic effects of these laws. Specifically, we include in our model the following dummy variables: *good faith*$^{-1}$, *good faith*^{0}, *good faith*$^{+1}$, and *good faith*$^{>=+2}$, which correspond to year *t*-1, *t*, *t*+1, and *t*+2 and after, respectively, where *t* is the year in which a state first adopted the good faith exception. If our finding arises from the spurious relation between the good faith exception adoption and stock repurchases due to the presence of pre-existing trends, we expect to obtain a positive and significant coefficient on the *good faith*$^{-1}$ dummy variable. Our results, however, indicate that the coefficient on *good faith*$^{-1}$ is statistically insignificant, while the coefficients on *good faith*^{0}, *good faith*$^{+1}$, and *good faith*$^{>=+2}$ are positive and highly significant. This evidence suggests that the parallel trends assumption (i.e., no pre-treatment trends) is satisfied, thus confirming the validity of our identification strategy and providing further support for a causal interpretation of a positive relation between WDLs and stock repurchases.

Our exploitation of the WDL adoption as an exogenous shock to firing costs is grounded on the premise that absent the passage of these laws, the stock repurchase behavior of two similar firms should evolve in a similar way. However, if two systematically different firms had their stock repurchase policies evolving in different ways even without the WDL adoption, our claim of causality could be invalid. To address this concern, we combine our difference-in-differences approach with propensity score matching; the latter method is used to identify control firms that are similar in several observable dimensions to treatment firms. In particular, for each treatment firm, we select a control firm from firms headquartered in a state that either has never adopted the good faith exception or has adopted the exception in a given year outside the three-year test window around the adoption of the law. The control and treatment firms are matched on all firm characteristics used in the baseline regression in year *t*-1 and the same (3-digit SIC) industry. Using this matched sample, we find that our main results persist.

Although we control for several sources of omitted variables that potentially arise from time-invariant or time-varying unobserved effects at firm, industry, state, and regional levels, it is possible that our results capture other unobserved shocks that coincide with the enactment of WDLs. To alleviate this concern, we perform placebo tests based on *counterfactual* adoptions of these laws. Specifically, we use the empirical distribution of the years in which the good faith exception was adopted to randomly assign states that never adopted the exception into each of these years (without replacement). If our results capture the true rather than the placebo effects of the WDL adoption, we do not expect to observe a significant effect of the counterfactual adoption of the good faith exception on stock repurchases. Indeed, we find that the coefficient on the *counterfactual* good faith variable is statistically insignificant in all models, indicating that our findings are unlikely driven by confounding effects.

To verify the robustness of our findings, we use alternative measures of share repurchases, such as the probability of share repurchases, the proportion of total payout in the form of share repurchases, or the volume of share repurchases scaled by the market or book values of equity instead of book assets. Our results are qualitatively similar. We further examine the effects of WDLs on dividends and total payout and, in line with our expectation, do not find significant results.

We are interested in uncovering the channels through which employment protection laws affect share repurchases. First, we examine whether and how the effect of WDLs on share repurchases varies among firms with different levels of financial constraints. If our observed increase in stock repurchases following the passage of WDLs is driven by lower unemployment risk, we expect to find a stronger effect of these laws for financially constrained firms. Since constrained firms lack capital, they are more exposed to unemployment risk and need to adopt

conservative financial policies (e.g., low leverage and high cash reserves) as a buffer against such risk. The adoption of WDLs reduces unemployment risk as well as the need for those firms to maintain conservative policies, while motivating them to increase share repurchases. Conversely, if the positive impact of WDLs on share repurchases is driven by the rent extraction channel, we expect to observe a more pronounced impact among financially unconstrained firms. Since unconstrained firms have relatively greater internal funds and cash flows, they are more vulnerable to rent extraction and, thus, have a stronger incentive to increase share repurchases to alleviate this incentive problem. Our analysis indicates that the positive effect of the adoption of WDLs on stock repurchases is more pronounced for financially unconstrained firms, suggesting that firms increase payouts to reduce employees' rent capturing behavior.

We further investigate the link between the enactment of WDLs and share repurchases for firms that vary on the levels of investor protection. Intuitively, firms with stronger investor protection are more likely to reduce the possibility of wealth transfer from shareholders to employees following the WDL adoption. We use institutional ownership as a proxy for investor protection. A higher level of institutional ownership indicates stronger investor protection. We find that the positive effect of WDLs on share repurchases is concentrated among firms with high institutional ownership. Collectively, our evidence suggests that firms increase stock repurchases to avoid rent extraction by employees that become better protected following the adoption of WDLs.

Our study adds to the literature in three important ways. First, to the best of our knowledge, our research is the first that examines the relation between employment protection laws and corporate payout policy. Using the staggered adoption of WDLs by different states at different points in time allows us to establish a causal effect of worker discharge costs on

corporate payouts. Our study thus contributes to a growing stream of literature that studies the relation between labor market frictions and corporate policies and outcomes, including financial leverage (Matsa, 2010; Agrawal and Matsa, 2013; Kuzmina, 2013; Schmalz, 2015; Simintzi, Vig, and Volpin, 2015; Serfling, 2016; Klasa et al., 2018), cash holdings (Klasa, Maxwell, and Ortiz-Molina, 2009; Ghaly, Dang, and Stathopoulos, 2017), capital expenditures and firm growth (Autor, Kerr, and Kugler, 2007; Bai, Fairhust, and Serfling, 2017), innovation (Acharya, Baghai, and Subramanian, 2013; 2014; Bradley, Kim, and Tian, 2016), mergers and acquisitions (John, Knyazeva, and Knyazeva, 2015; Dessaint, Golubov, and Volpin, 2017), risk management (Qui, 2016), and bankruptcy (Campello, Gao, Qiu, and Zhang, 2018).

We also add to the more established literature on corporate payouts (e.g., Jagannathan, Stephens, and Weisbach, 2000; Fama and French, 2001; Grullon and Michaely, 2002; DeAngelo, DeAngelo, and Stulz, 2006; Hoberg and Prabhala, 2009; Hoberg, Philipps, and Prabhala, 2014) and in particular research on share repurchases (e.g., Dittmar, 2000; Kahle, 2002; Billet and Xue, 2007). Our finding suggests that employment protection and firing costs play an important role in shaping firms' stock buyback decisions. We note that our study is closely related to recent research examining the effects of unionization on corporate payout policy (Chino, 2016; He, Tian, Yang, and Zuo, 2018). Since WDLs typically pertain to full-time or nonunionized workers (Serfling, 2016), especially those with higher labor skills (Ghaly, Dang, and Stathopoulos, 2017), the impact of these laws on corporate payout decisions is unlikely to be driven by unionization. Consistent with this argument, our analysis shows that the positive relation between WDLs and share repurchases remains significant after controlling for the role of unionized labor.

Our research contributes to the stream of literature that studies the conflicts of interests among various stakeholders of the firms as well as the implications of those conflicts for

corporate payout policy (e.g., Jensen, 1986; Easterbrook, 1984; La Porta et al., 2000; Brockman and Unlu, 2009; Chu, 2018). Our evidence indicates that, faced with potential employees' rent extraction motivated by increased firing costs following the WDL adoption, firms that have stronger investor protection increase stock repurchases to mitigate such rent extraction. In addition, previous research argues that firms may decrease cash reserves and increase debt financing to reduce rent extraction by labor (e.g., Matsa, 2010; Schmalz, 2015). Our finding suggests another channel, corporate payouts, through which firms mitigate value transfer from shareholders to employees. Overall, our findings provide important implications of labor market frictions for corporate managers, investors, and other stakeholders as they make decisions.

The rest of the paper proceeds as follows. Section 2 provides institutional background and hypothesis development. Section 3 discusses data and empirical methods. Section 4 presents empirical results. We discuss the channels of effects in Section 5 and Section 6 concludes the paper.

2. Institutional Background and Hypothesis Development

2.1 Employment Protection Laws

To protect employees from wrongful termination practices, many states started to adopt exceptions to the "at-will employment" rule in the 1970s. These exceptions, typically referred to as WDLs, evolved into three common laws related to the good faith exception, implied contract exception, and public policy exception. A state's passage of a particular WDL is based on the precedent setting court cases (Autor, Donohue, and Schwab, 2006; Serfling, 2016). Some states adopt all three exceptions while some other states adopt two, one or no exceptions. Miles (2000) points out that these exceptions apply to employees not already covered by explicit contractual

agreements or federal legislation that protects a particular class of workers, such as racial minorities, women, or union members.

As summarized by Serfling (2016, p. 2245), the good faith exception is "based on the legal theory that an implied promise of good faith and fair dealing exists between employers and employees." It requires that employers "treat employees in a fair manner (i.e., in good faith) and not take actions that will deprive employees of the benefit of employment without just cause." Employers violate the good faith exception if they discharge workers out of bad faith, retaliation, or malice. They also violate this law if they fire employees before pensions vest or employees receive their entitled commissions or bonuses. Generally, the good faith exception has been interpreted to imply that employee discharge decisions are subject to a "just cause" standard, which significantly expands the set of situations for which employees can sue employers. On the other hand, the implied contract exception protects employees from discharge when an employer implicitly promised the employees, in oral or written in a handbook, that they will not be terminated without good cause. Serfling (2016, p. 2245) documents that "[c]ourts have also determined that employee tenure, a history of promotions or salary raises, general company policies, and typical industry practices can constitute an implied promise of ongoing employment." Finally, the public policy exception protects employees from termination should the latter refuse to violate an established public policy or commit an illegal act, such as reporting the former's wrongdoing. Central to the public policy exception is the argument that a worker should not be dismissed for performing a public service even if his/her action is not in the interest of the employer.

Among the three exceptions to "at-will employment", the good faith exception is likely to have the most significant effect on corporate behavior because it can imply that employment

termination must be for just cause (Dertouzos and Karoly, 1992; Kugler and Saint-Paul, 2004). Moreover, this exception allows employees to seek compensation for both contractual losses and emotional and punitive damages. Since emotional and punitive damages can increase an employer's liability significantly and these damages can be determined by a jury without a standard formula, there is great uncertainty with the settlement amounts. On the other hand, firms can mitigate the threat of lawsuits based on implied contract exception by including disclaimers into personnel manuals and employees' handbooks that explicitly state that employment contracts are at-will (Miles, 2000; Autor, Kerr, and Kugler, 2007). Autor, Kerr, and Kugler (2007) further note that the public policy exception generally does not restrict employer behavior since courts typically limit recovery to employee termination where the employer violated constitutional provision or identifiable statue.

The enactment of WDLs increases firing costs substantially. Jung (1997) reports that, on average, plaintiffs won $1.29 million in 1996, while Boxold (2008) recently documents that plaintiff won a maximum award of $5.4 million over the period 2001–2007. Furthermore, WDLs can motivate wrongful termination lawsuits, which increase the number of lawsuits and legal liability a firm may have to face at a point in time. To reduce the financial liability that arises from the wrongful discharge lawsuits, firms can purchase Employment Practices Liability Insurance (EPLI). However, the market for EPLI did not develop until the early 1990s while the insurance coverage was poor and the insurance premiums were high, limiting a firm's ability to insure against wrongful termination claims during our sample period. Other traditional forms of insurance, such as directors and officers (D&O) liability insurance or commercial general liability insurance, provide little help with wrongful termination claims.

2.2 Hypothesis Development

Faced with potential job loss, workers demand wage premiums as compensation for bearing unemployment risk (Abowd and Ashelfelter, 1981; Titman, 1984; Topel, 1984; Berk, Stanton, and Zechner, 2010; Chemmanur, Cheng, and Zhang, 2013). In particular, Agrawal and Matsa (2013) argue that firms may choose conservative financial policies to decrease the risk of financial distress, which reduces employees' exposure to unemployment risk and, thus, the wage premiums required by workers. To the extent that WDLs increase firing costs, they can lower workers' unemployment risk, potentially leading to lower wage premiums. These labor cost savings are likely to increase firms' cash flows that can be used for distribution to shareholders.

Alternatively, since WDLs can increase the risk and costs associated with wrongful termination lawsuits, they may create a conflict of interests between shareholders and employees and lead to wealth transfer from shareholders to employees. To discourage employees from bringing lawsuits against firms and mitigate potential rent extraction by employees, firms may have an incentive to reduce cash flows while increasing payouts. These arguments lead to our first hypothesis as follows:

Hypothesis 1a: The adoption of WDLs leads to an increase in corporate payouts.

Serfling (2016) argues that higher firing costs can exacerbate a firm's financial distress for two reasons. First, since firms are more likely to terminate employment when they have cash flow shortfalls (Ofek, 1993; Kang and Shivdasani, 1997), higher firing costs may exacerbate the corporate liquidity problem. Second, higher firing costs can weaken firms' ability to reduce employment at precisely the points in time when they really need to do so, such as during economic downturns (Bentolila and Bertola, 1990; Autor, Donohue, and Schwab, 2006; Messina and Vallanti, 2007). Thus, higher employee firing costs can make labor costs more fixed in

nature and increase operating leverage and distress risk (Mandelker and Rhee, 1984; Mauer and Triantis, 1994; Kahl, Lunn, and Nilsson, 2014; Serfling, 2016). To the extent that the adoption of WDLs increases firing costs that heighten financial distress risk, firms may choose to follow a conservative payout policy by reducing corporate payouts. Following this argument, we state our alternative hypothesis as follows:

Hypothesis 1b: The adoption of WDLs leads to a decrease in corporate payouts.

Jagannathan, Stephens, and Weisbach (2000) report that dividends are associated with stable and permanent cash flows. Moreover, firms are less likely to reduce or skip dividends because this behavior may send a negative signal about the firms' future prospects to investors that adversely affects stock price. Indeed, it is well-documented that firms tend to smooth their dividends over time (Lintner, 1956; Brav et al., 2005; Leary and Michaely, 2011). On the other hand, stock repurchases are typically associated with business cycles or temporary cash flows. Overall, dividend policy tends to be sticky while stock repurchases can give firms more flexibility. Grullon and Michaely (2002) document that U.S. firms have gradually substituted stock repurchases for dividends, which is consistent with the view that stock repurchases give firms more flexibility (Bonaimé, Hankins, and Harford, 2014). To the extent that WDLs reduce the wage premiums or create a conflict of interests between employees and shareholders that leads to an increase in corporate payouts, we do not expect the cash flows associated with the WDLs to be stable and permanent. On the other hand, if increased firing costs due to the WDL adoption heighten financial distress risk, firms are more likely to reduce stock repurchases to shore up their cash reserves rather than reducing dividends permanently. The foregoing discussions indicate that the effects of WDLs on corporate payouts, if any, should be more relevant for stock repurchases than dividend payments. For this reason, we focus our analysis on

the relation between WDLs and stock repurchases. We consider the effects of these laws on dividends and total payout in the robustness checks.

3. Data and Empirical Methods

3.1 Sample Selection and Variables

We follow Serfling (2016) and consider the exogenous changes in firing costs induced by the adoption of WDLs by U.S. state courts over the period 1967–1995; see Table 1 for details. Owing to the lack of information on repurchase transactions in Compustat in early years, our sample period ranges from 1971 to 1995. We consider all publicly listed U.S. firms in the CRSP / Compustat Merged Database (CCM), except financial firms (SIC codes 6000–6999), utilities (SIC codes 4900–4949), and firms without common stock.

[Insert Table 1 about here]

Although CCM is our main source of market and accounting data, we also collect information on the risk free rate and Fama-French industry classifications from Kenneth French's website,[1] and obtain state-level control variables from several sources: the Bureau of Economic Analysis, FRED, and www.unionstats.com. The institutional ownership data is from the Thomson Reuters Institutional Holdings (13F) database.

Table 2 reports some descriptive statistics for the main variables in our study. The detailed definitions of all the variables are provided in Appendix A. Two percent of the observations of all firm-specific continuous variables are winsorized to mitigate the impact of outliers. The payout dependent variables are winsorized at their 98th percentiles while the other continuous variables are winsorized at their 1st and 99th percentiles.

[1] http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/.

[Insert Table 2 about here]

3.2 Empirical Models

To examine the impact of WDLs on share repurchases, we estimate the following difference-in-differences model:

$$SP_{i,t} = \alpha + \beta_1 Good\ faith_{s,t} + \beta_2 Implied\ contract_{s,t} + \beta_3 Public\ policy_{s,t}$$
$$+\boldsymbol{\gamma}'\boldsymbol{X}_{i,t} + \mu_i + \rho_{j,t} + \theta_{r,t} + \omega_{s',t} + \varepsilon_{i,t}, \quad (1)$$

where *i*, *s*, *s'*, *j*, *r*, and *t* denote firm, state of location of headquarters, state of incorporation, industry, region, and year, respectively. Our main measure of the dependent variable is the actual dollar volume of share repurchases scaled by total assets (e.g., Brown, Liang, and Weisbenner, 2007; Hoberg, Phillips, and Prabhala, 2014; Bliss, Cheng, and Denis, 2015); the value of share repurchases is defined as the purchase of common and preferred stock less the reduction in the value of any preferred stock outstanding (e.g., Dittmar, 2000; Kahle, 2002; Hoberg, Phillips, and Prabhala, 2014). In our robustness checks, we follow prior research and verify the baseline results using other scaling variables such as the market and book values of equity,[2] cash flows, and sales. We also consider a stock repurchase indicator that captures the decision of firms to buy back their shares. As mentioned, in further analysis we use dividends and total payout as our dependent variables to examine the impacts of WDLs on those forms of payout.

Our main test variables include three WDL dummies: *good faith*, *implied contract*, and *public policy*, which are set equal to one if the state in which a firm is headquartered has adopted

[2] We refrain from using the market value of equity as the scaling variable for share repurchases because it is negatively affected by the passage of WDLs (Serfling, 2016). Employing this scaling variable leads to a mechanical positive effect on the share repurchases ratio.

the good faith, implied contract, or public policy exceptions, respectively. As in recent research (e.g., Agrawal and Matsa, 2013; Acharya, Baghai, and Subramanian, 2014; Serfling, 2016), we argue that employment protection laws typically apply to the state in which an employee is working. The state of location of headquarters is likely where the majority of plants and operations are based and executives and managements are concentrated and thus subject to wrongful termination lawsuits (Bai, Fairhurst, and Serfling, 2017).

In our model, we include firm fixed-effects, μ_i, to control for time-invariant firm heterogeneity. Following recent research (Gormley and Matsa, 2014), we further augment Eq. (1) by adding (i) industry-by-year fixed effects, ρ_{jt}, where industry fixed effects are defined using the Fama–French 49 industry classification;[3] (ii) region-by-year fixed effects, θ_{rt}, where regional fixed effects are constructed using the four census tract regions; and/or (iii) state of incorporation-by-year fixed effects, $\omega_{s',t}$.[4] By including those higher dimensional fixed effects, we can control for time-varying heterogeneity across industries (Bai, Fairhurst, and Serfling, 2017), regions (Autor, Donohue, and Schwab, 2006; Acharya, Baghai, and Subramanian, 2014), and states of incorporation (Mansi and Wald, 2011), which may be correlated with the adoption of WDLs. Put differently, controlling for those interactive fixed effects allows us to alleviate the omitted variable bias due to confounding factors related to industry, region, and state-level

[3] In unreported analysis, we find that our main findings are qualitatively unchanged when we define industry fixed effects at the 3-digit SIC level.

[4] In this model, we do not include state of location-by-year fixed effects because the main test variables, *good faith*, *implied contract*, and *public policy*, which are defined using the state of location of headquarters, are highly correlated with those fixed effects (see also Bai, Fairhurst, and Serfling, 2017 for a similar approach).

trends. This approach thus provides more conservative estimates than simply controlling for time-invariant industry, region, and state fixed effects (e.g., Gormley and Matsa, 2017).

In all regressions, we estimate and report *t*-statistics based on robust standard errors clustered at the state of location of headquarters level, i.e., the level at which WDLs are adopted. This method of clustering corrects for serial correlation within a given state and thus yields more conservative standard errors than those clustered at the firm level (Bertrand, Duflo, and Mullainathan, 2004).

In Eq. (1), we include in the vector of controls, $\boldsymbol{X}_{i,t}$, several firm-level variables commonly used in the literature (e.g., Dittmar, 2000; Fama and French, 2001; DeAngelo, DeAngelo, and Stulz, 2006; Billett and Xue, 2007; Hoberg and Prabhala, 2009; Hoberg, Philipps, and Prabhala, 2014): *cash flow*, *negative earnings*, *NYSE percentile*, *retained earnings*, *idiosyncratic risk*, *systematic risk*, *log of firm age*, *market-to-book*, *total liabilities*, and *cash holdings*.[5] Although we follow the conventional approach in prior research and include firm-specific characteristics as our control variables, in additional robustness checks (Section 4.2.1), we further control for a number of state-level variables that may be correlated with the adoption of WDLs.

To address the possibility that the firm-specific control variables may be endogenous, thus affecting inference on the treatment effect in the difference-in-differences approach (Angrist and Pischke, 2009; Gormley and Matsa, 2017), we further consider a variant of Eq. (1). Specifically, throughout the analysis we estimate an alternative specification model that only

[5] We note that by including total liabilities and cash holdings, the model used in our analysis already addresses the concern that our findings are driven by the relation between WDLs and financial leverage or cash holdings (Serfling, 2016), which, in turn, affect share repurchases (e.g., Farre-Mensa, Michaely, and Schmalz, 2017).

includes the aforementioned WDL dummy variables, firm fixed effects, and higher dimensional fixed effects but not the control variables.

4. Empirical Results

4.1 Baseline Regression Results

Table 3 tabulates the results from estimating our baseline model, Eq. (1). All regressions are estimated using OLS with firm fixed effects and higher dimensional fixed effects. We report the results for the alternative specification without the control variables in Models (1)–(3) and those for the main specification with the control variables in Models (4)–(6). The results under all specifications show that the coefficient on *good faith* is positive and significant at the 1% significance level, consistent with *Hypothesis 1a*. These results are robust to controlling for time-varying heterogeneity across industries, regions, and states of incorporation. This finding suggests that a firm increases its share repurchases after the state in which it operates has adopted the good faith exception. The effect of *good faith* on the volume of share buybacks is not only statistically significant but also economically important. Using the most stringent specifications in Models (3) and (6), we find that firms experience an increase of 0.00172–0.00164 in share repurchases following the enactment of the good faith exception, which translates to an increase of 30%–29% relative to the mean (0.0057). On the other hand, the coefficients on the other two exceptions, *implied contract* and *public policy*, are insignificant, both statistically and economically. This finding is in line with our expectation and recent evidence on the relative impacts of those exceptions on firm leverage and investment decisions (Serfling, 2016; Bai, Fairhurst, and Serfling, 2017).

[Insert Table 3 about here]

The results regarding the control variables in Models (4)–(6) are broadly in line with previous research (e.g., Dittmar, 2000; Fama and French, 2001; Kahle, 2002; DeAngelo, DeAngelo, and Stulz, 2006; Billet and Xue, 2007; Hoberg and Prabhala, 2009; Hoberg, Philipps, and Prabhala, 2014). Specifically, we find that firms with *negative earnings* have a lower volume of share buybacks, while those of a small size (i.e., with a low *NYSE percentile*) have a higher magnitude of stock repurchases. In contrast, firms with higher *cash holdings* and *cash flows* experience a higher value of share repurchases. Firm maturity, measured using *retained earnings* and *firm age*, is also positively associated with share buybacks. Both risk variables, *idiosyncratic* and *systematic risk*, have a negative and significant impact on share repurchases. However, *total liabilities* and *market-to-book* do not seem to have any significant impact on share buybacks.

Overall, our baseline results indicate that there is a positive impact of WDLs, particularly the good faith exception, on the value of share repurchases, consistent with *Hypothesis 1a* and inconsistent with *Hypothesis 1b*.

4.2 Addressing Econometric Issues

4.2.1 Additional State-Level Controls

Although our baseline model already includes region-by-year and state-by-year fixed effects to account for time-varying (unobserved) differences at the regional and state (of incorporation) levels, in Table 4 we further mitigate the omitted variable bias and endogeneity concern by controlling for several local economic and political factors that may be correlated with the adoption of WDLs, particularly the good faith exception. As in Acharya, Baghai, and Subramanian, (2014), Serfling (2016), and Bai, Fairhurst, and Serfling (2017) we first add the following (state-level) variables to our model: real GDP per capita, real GDP growth rate, and political balance (i.e., the ratio of the Democrat to Republican state representatives in the House

of Representatives). Next, we include three state-level labor-related variables: unionization membership density, unionization coverage density, and the passage of RTW laws. While recent research suggests that the adoption of the good faith exception is generally unaffected by most local political and economic factors and thus can be treated as exogenous (Acharya, Baghai, and Subramanian, 2014; Serfling, 2016), there remains some evidence that it may be positively related to per capita GDP, and negatively associated with both political balance and union membership (Bai, Fairhurst, and Serfling, 2017), hence the need to control for those factors. We further note that by controlling for state-level unionization and the adoption of RTW laws, we can address the concern that our findings are driven by alternative mechanisms related to unionization (Chino, 2016; He, Tian, Yang, and Zuo, 2018) and RTW laws (Matsa, 2010; Chen, Chen, and Yang, 2015).

[Insert Table 4 about here]

In Models (1) to (6) of Table 4, we separately control for each of the aforementioned additional state-level variables. In Model (7) we include all those controls. In all regressions, we estimate the most stringent specification that controls for firm, industry-by-year, region-by-year, and state-by-year fixed effects. The results show that share repurchases are not significantly associated with GDP per capita, political balance, union membership and coverage densities, and RTW laws. There is some evidence that GDP growth rate has a negative impact on stock buybacks. Importantly, the results across models suggest that the impact of the good faith exception on share repurchases remains significant after controlling for those additional political and economic factors. Overall, these results alleviate the endogeneity concern that our main finding is driven by omitted variables reflecting local conditions.

4.2.2 Pre-treatment Trend Analysis and Dynamic Effects

The validity of our difference-in-differences approach is based on the important assumption that the passage of WDLs is exogenous and that firms headquartered in states that adopt WDLs (treated) and those do not adopt those laws (control) follow parallel pre-existing trends. To evaluate this assumption, we follow prior research (e.g., Bertrand and Mullainathan, 2003; Acharya, Baghai, and Subramanian, 2014; Serfling, 2016) and estimate the dynamic effects of WDLs on share repurchases. To capture the impacts of the good faith exception, we decompose the passage of the WDLs into four time periods, captured by four dummy variables; these are set equal to one if the firm is headquartered in a state that (i) will adopt the good faith exception in one year (*good faith*$^{-1}$), (ii) adopted the exception in the current year (*good faith*^{0}), (iii) adopted the exception one year ago (*good faith*$^{+1}$), and (iv) adopted the exception two or more years ago (*good faith*$^{>=+2}$). We define the time period indicators for the other two WDL exceptions, namely *implied contract* and *public policy*, in the same way.

The results in Table 5 show that the coefficient on *good faith*$^{-1}$ is statistically insignificant, except in Model (3) where it is marginally significant at the 10% significance level. Importantly, this coefficient remains insignificant in the preferred (most stringent) specification in Model (6). The corresponding coefficients on *implied contract*$^{-1}$ and *public policy*$^{-1}$ are also insignificant across all models. Taken together, there is little evidence of pre-treatment trends, suggesting that the parallel trends assumption is satisfied. The results also imply that our main finding is not affected by reverse causality, that is, past share repurchases do not explain the enactment of WDLs.

[Insert Table 5 about here]

The coefficients on *good faith*^{0}, *good faith*$^{+1}$, and *good faith*$^{>=+2}$ are highly significant and positive, consistent with our baseline results. They further indicate that the effect of the good faith exception on share repurchases occurs immediately after the exception has been adopted. While this effect appears to diminish over time, it remains permanent and economically important as indicated by a significant coefficient on *good faith*$^{>=+2}$. As in the baseline results, the corresponding coefficients on the implied contract and public policy exceptions are always insignificant, suggesting that those exceptions do not affect share repurchases in any way. Overall, the results from our pre-treatment trend analysis confirm the validity of our identification strategy and provide additional evidence supporting a causal interpretation of the effect of WDLs on share repurchases.

4.2.3 Propensity Score Matching

Using the difference-in-differences model with firm fixed effects and high dimensional fixed effects already controls for both time-invariant unobserved (firm) heterogeneity and time-varying unobserved heterogeneity across industries, regions, and states. To address the remaining concern that the treatment effect may be driven by observed factors, we follow Serfling (2016) and combine the difference-in-differences approach with propensity score matching (PSM). Specifically, we first perform a PSM analysis to construct two groups of firms: control and treatment firms, which are similar along observable dimensions. We then run the difference-in-differences regression using the propensity-score-matched sample. This approach has the advantage of controlling for both unobserved heterogeneity and observable differences along various dimensions and arguably allows for a cleaner estimation of the treatment effect.

To construct the treatment and control groups, we follow a matching procedure used in Bai, Fairhurst, and Serfling (2017). Specifically, our treatment group consists of all firms

headquartered in states that adopted the good faith exception in year *t*.[6] The control group includes matched firms headquartered in states that either (i) never adopt the good faith exception or (ii) have adopted the exception in a given year outside of the three-year test window around the adoption of the law.[7] We perform one-to-one matching (with replacement) based on industry (3-digit SIC industry) and all control variables used in our baseline regression model in year *t*-1 (one year before the adoption of the exception). We require a control firm to have a propensity score within 0.5% of the treatment firm's propensity score. We retain only treatment and control firms that have available data for at least one year in both the pre- and post-treatment periods. Panel A of Table 6 reports the results from our matching exercise. In sum, we are able to identify 396 pairs of treatment and control firms that satisfy the matching criteria. Importantly, the characteristics of the treatment firms are statistically similar to those of the control firms, as demonstrated by the insignificant *t*-statistics in tests for differences in the means of those characteristics. Overall, our matching exercise seems to be satisfactory.

[Insert Table 6 about here]

In Panel B of Table 6, we estimate an augmented specification of our baseline model (Eq. (1)) using the matched sample. This specification excludes the WDL exceptions but includes three additional terms, *Treated*, *Post*, and *Treated*×*Post*. *Treated* is an indicator equal to one if a firm belongs to the treatment group, i.e., if the state in which the firm is headquartered has recognized the good faith exception in year *t*. *Post* is an indicator equal to one in the period after the recognition of the exception. Our variable of interest is *Treated*×*Post*, which captures the

[6] In this analysis, we focus on the good faith exception as our evidence thus far has shown that share repurchases are only affected by this exception. Our approach is similar to the one employed by Serfling (2016).

[7] We obtain qualitatively similar results when using a five-year test window.

effect of the good faith exception on the treatment firm, in the period after the law has been adopted. The results across all models indicate that the coefficient on *Treated*×*Post* is significant with a positive sign, suggesting that the stock repurchases of the treatment firms increase in value after the states in which those firms are headquartered have adopted the good faith exception. Overall, these results provide additional evidence to support our main inference.

4.2.4 Placebo Tests

Although our analyses have so far controlled for many sources of omitted variables (e.g., time-invariant and time-varying unobserved and observed heterogeneity at different levels), one concern remains that our results may still be driven by unobserved shocks that may coincide with the state-level adoption of WDLs. To address this concern, we follow recent corporate research using quasi-natural experiments (e.g., Cornaggia et al., 2015) and carry out placebo tests based on artificial WDL events that are different from true events. Using the empirical distribution of the years in which the good faith exception was adopted, we randomly assign states that never adopted this exception into each of those years (without replacement). This approach ensures that our analysis will still maintain the empirical distribution of the adoption years and thus will pick up any unobservable shocks related to those years that may be driving our baseline results. However, since the placebo test uses incorrect assignments of states to adoption years, we do not expect the effect of those shocks to be highly significant. Put differently, if our main findings are not affected by placebo effects, we should find a weak, if any, impact of the *counterfactual* good faith exception on share repurchases.

We report the results from our placebo test in Table 7. We use the same model specification as in our baseline analysis, with one exception that it only includes the good faith exception and does not include the other two exceptions. As in the previous section, our focus is

on the impact of the former law. We find that the coefficient on the *counterfactual* good faith exception is insignificant in all models, indicating that our main evidence is unlikely to be driven by placebo effects.

[Insert Table 7 about here]

To strengthen the inference from our placebo test analysis, we repeat the process 1,000 times, that is, we randomly assign states to adoption years and then run the regression using those data 1,000 times (see Bradley, Kim, and Tian, 2016 for a similar approach). Figure 1 reports the histogram of the *t*-statistics obtained from those placebo regressions. To facilitate comparisons, we also include a vertical line that represents the *t*-statistic estimated using the actual data. The results show that, when using randomly assigned (incorrect) state data, the treatment effect of the *counterfactual* good faith exception on share repurchases is almost always insignificant. They imply that the positive coefficients on *good faith* and the sufficiently large *t*-statistic estimated in our baseline regressions are not likely to be driven by chance. In short, our main finding is unlikely to be affected by placebo effects.

Overall, while it is impossible to completely rule out the endogeneity concern, taken together, the analyses in Section 4.2 indicate that our estimates are unlikely to capture a spurious relationship between WDLs and stock buybacks. The results from those analyses consistently point toward a causal effect of the good faith exception on share repurchases.

4.3 Additional Robustness Checks

4.3.1 Propensity of Share Repurchases

Our analysis has thus far examined the impact of WDLs on the value of stock buybacks. In this section, we study whether these laws affect the propensity of stock buybacks. To this end, we use a share repurchase dummy variable as our dependent variable; the variable is equal to one

if the firm has positive share repurchases and zero otherwise (e.g., Dittmar, 2000). We then specify Eq. (1) as the linear probability model and estimate it using OLS.[8]

[Insert Table 8 about here]

The results tabulated in Table 8 show that the coefficient on *good faith* is positive and statistically significant, except in Models (2) and (5) where it is significant at the 10% level. The results are robust to alternative specifications with or without control variables. Regarding the implied contract and public policy exceptions, we find that the coefficients on those variables are generally insignificant, with a few exceptions where they are marginally significant at the 10% level. Overall, there is strong evidence that firms are significantly more likely to repurchase their shares following the adoption of the good faith exception. This finding is in line with our earlier results regarding the impact of this law on the volume of share repurchases. It suggests that the enactment of the good faith exception leads to an increase in both the volume and propensity of share repurchases, consistent with *Hypothesis 1a*.

4.3.2 Alternative Measures of Share Repurchases

We have thus far defined share repurchases as the volume of stock repurchases in a given year scaled by total assets. We next evaluate the robustness of our main finding to using alternative scaling variables commonly used in the literature, including the market and book values of equity, cash flow, and sales (e.g., Dittmar, 2000; Chay and Suh, 2009; Leary and Michaely, 2011; He, Tian, Yang, and Zuo, 2018). To conserve space, for each measure we

[8] We refrain from using non-linear models such as logit and probit models because our specification includes a complex structure of firm fixed effects and high dimensional fixed effects, which are difficult to estimate using maximum likelihood. The linear probability model can approximate the average partial effects of the independent variables on the probability response (Angrist and Pischke, 2009).

estimate the most stringent specification for Eq. (1) that controls for firm, industry-by-year, region-by-year, and state-by-year fixed effects, without and with firm-specific controls. We obtain qualitatively similar results when using the other specifications as in Table 3. The regression results tabulated in Table 9 show that the coefficient on *good faith* is statistically significant and positive across all models. The coefficients on *implied contract* and *public policy* are insignificant, as expected. We thus conclude that our main finding regarding the positive impact of the good faith exception on share repurchases is not sensitive to alternative measures of the latter variable.

[Insert Table 9 about here]

4.3.3 Dividends and Total Payout

While our analysis has demonstrated a significant causal effect of WDLs on share repurchases, it is not obvious whether such a finding will extend to dividends, another important form of payout. Our prior expectation is that WDLs are likely to have a weaker, if any, impact on dividends, considering that managers are much more reluctant to increase dividends as they prefer to smooth dividends (e.g., Lintner, 1956; Brav et al., 2005; Leary and Michaely, 2011). To test this conjecture, we rerun our regressions for dividends using the most stringent specification of Eq. (1). We define dividends as common dividends scaled by total assets to be consistent with our measure of share repurchases; the results (untabulated) are robust to using alternative scaling variables. Models (1) and (2) of Table 10 reveal that the coefficient on *good faith* is insignificant. We obtain a similar finding regarding the implied contract and public policy exceptions. These results indicate that WDLs do not have a significant effect on dividends, which is in line with the argument that firms tend to maintain stable dividend policies.

[Insert Table 10 about here]

For completeness, we next examine the impact of WDLs on total payout. We first measure total payout as the sum of dividends and share repurchases, scaled by total assets. The results in Models (3) and (4) show that all the three exceptions have no significant effect on total payout. This finding is not surprising considering that these laws are not related to dividends, one major component of total payout. Second, we follow Bonaimé, Hankins, and Harford (2014) and use a measure of payout flexibility, defined as share repurchases scaled by total payout. The results in Models (5) and (6) suggest that payout flexibility is significantly and positively associated with the good faith exception but is not significantly related to the implied contract and public policy exceptions. The former finding indicates that firms are more likely to use share repurchases, a more flexible form of payout, than dividends when facing with stronger employment protection laws. It is consistent with the argument that firms typically prefer to use share buybacks to dividends to maintain financial flexibility (Bonaimé, Hankins, and Harford, 2014). This finding further helps explain the above non-result for dividends and total payout.

5. Underlying Mechanisms

5.1 Financial Constraints

We first examine whether the impact of WDLs on share repurchases varies depending on firms' degrees of financial constraints. To the extent that firms increase their stock buybacks due to lower unemployment risk, we expect the impact of these laws to be weaker for financially unconstrained firms. The reason is that, unconstrained firms have stronger financial health and thus are less exposed to unemployment risk *ex ante*. Hence, these firms have less incentive to adopt more aggressive repurchase policies after the passage of WDLs. If, on the other hand, the increase in share repurchases is driven by shareholders' stronger demand for payout to avoid rent capture by better protected employees, the effect of WDLs should be more pronounced for

unconstrained firms. These firms are likely to have greater internal funds and cash flows, thus making it easier for managers to increase share repurchases to accommodate the demand by shareholders.

To test those alternative arguments, we use three common alternative measures of financial constraints: the Rajan and Zingales' (1998) external finance dependence (EFD) index; the Hadlock and Pierce (HP) (2010) size and age index; and the Whited and Wu (WW) (2006) index; see Appendix A for the definitions of those indices.[9] Based on Eq. (1), we specify an augmented model that includes a financial constraint indicator: *High EFD*, *High HP*, or *High WP*, which is set equal to one if the firm is constrained, that is, when it has a higher than median EFD index score, HP index score, or WW index score, respectively. Importantly, the model includes the interaction term between each financial constraint dummy and *good faith*, our variable of interest.[10]

The results in Table 11 show that the coefficient on the interaction term between the good faith indicator and the financial constraint dummy is significant and negative. This finding is robust to the three measures of financial constraints. Following the passage of WDLs, firms are less (more) likely to increase their share repurchases when they are more (less) financially constrained. Overall, our analysis provides some evidence in support of the rent capture channel,

[9] We refrain from using two commonly measures of financial constraints, namely the Kaplan and Zingales (1997) index and dividend paying status, as they are constructed based on dividends and thus are likely to be endogenous.

[10] The model controls for the same structure of fixed effects as in our baseline regression. However, the results (untabulated) are qualitatively unchanged if we follow Bai, Fairhurst, and Serfling's (2017) approach and use the state of location-by-year fixed effects (in place of the state of incorporation-by-year fixed effects) in regressions with interaction terms with *good faith*.

i.e., managers have an incentive to distribute more cash flows to shareholders to avoid rent extraction from employees that become harder to dismiss.

5.2 Governance

To the extent that the adoption WDLs leads to higher share repurchases because managers use share buybacks to pay out to shareholders in response to possible rent capture behavior by better protected employees, the relation between those laws and repurchases should be stronger when the interests of managers and shareholders are more aligned. To investigate this mechanism, we examine firms with stronger versus weaker governance structures. We follow prior research and use institutional ownership as a measure of governance. We define *High IO* as a dummy variable equal to one if the firm has a higher than median level of institutional ownership. Firms with a higher fraction of institutional ownership face stronger monitoring by institutional investors. These firms tend to have stronger governance mechanisms, which make managers and shareholders better aligned.

As in the above section, we estimate an augmented model of Eq. (1) with two additional variables: *High IO* and *Good faith*×*HighIO*. The results reported in Table 12 indicate that the coefficient on the interaction term is significantly positive, while that on the good faith exception is statistically and economically insignificant. This finding suggests that the positive effect of WDLs on share buybacks is concentrated among firms with higher institutional ownership. Consistent with our expectation, as managers and shareholders have stronger alignment of interests, the former distribute more cash flows to the latter via stock buybacks. This course of action prevents rent capture by employees who become better protected following the adoption of WDLs.

[Insert Table 12 about here]

6. Conclusions

Previous studies document that higher LACs lead to more conservative corporate investment and financial policies. In this study, we use the staggered adoption of WDLs by state courts as a quasi-natural experiment to investigate the relation between employee firing costs and corporate payout policy. WDLs include three exceptions to "at-will employment", namely the good faith exception, implied contract exception, and public policy exception. The enactment of these laws leads to plausibly exogenous increases in employment protection and firing costs. We find robust results that, among the three exceptions, the adoption of the good faith exception leads to an increase in firms' stock repurchases but insignificant changes in their dividends or total payout. On the other hand, the other two exceptions do not have significant effects on corporate payout policy, consistent with our conjecture.

Our subsample analyses indicate that the positive relation between employee firing costs and stock repurchases is more pronounced for financially unconstrained firms and firms characterized by good investor protection. Our findings indicate that, as the adoption of WDLs increases employee firing costs that potentially lead to wealth transfer from shareholders to workers, firms increase stock repurchases to discourage workers' wrongful termination lawsuits and mitigate their rent extraction.

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Figure 1. Histogram of T-statistics in Placebo Tests

This figure plots the histogram of the distribution of the t-statistics of the coefficient on the good faith exception (*good faith*) dummy from 1,000 placebo tests. In each iteration we use the empirical distribution of the years in which the good faith exception was adopted and randomly assign states that never adopted this exception into each of those years (without replacement). The dashed vertical line represents the true t-statistic from our regression of share repurchases on the good faith dummy variable (GF) and the controls.

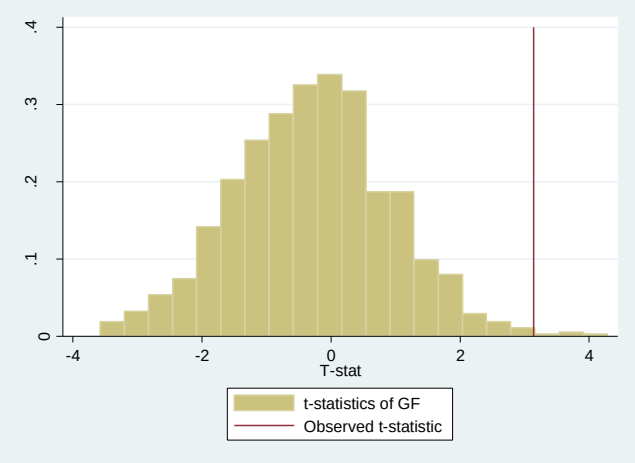

Table 1. State-Level Staggered Adoption of WDLs

This table summarizes the month and year of adoption of three WDL exceptions, namely the good faith, implied contract, and public policy exceptions, to the employment at-will rule.

State	Good faith	Implied contract	Public policy
Alabama		07/1987	
Alaska	05/1983	05/1983	02/1986
Arizona	06/1985	06/1983 (Reversed 04/1984)	06/1985
Arkansas		06/1984	03/1980
California	10/1980	03/1972	09/1959
Colorado		10/1983	09/1985
Connecticut	06/1980	10/1985	01/1980
Delaware	04/1992		03/1992
Florida			
Georgia			
Hawaii		08/1986	10/1982
Idaho	08/1989	04/1977	04/1977
Illinois		12/1974	12/1978
Indiana		08/1987	05/1973
Iowa		11/1987	07/1985
Kansas		08/1984	06/1981
Kentucky		08/1983	11/1983
Louisiana	01/1998		
Maine		11/1977	
Maryland		01/1985	07/1981
Massachusetts	07/1977	05/1988	05/1980
Michigan		06/1980	06/1976
Minnesota		04/1983	11/1986
Mississippi		06/1992	07/1987
Missouri		01/1983 (Reversed 02/1988)	11/1985
Montana	01/1982	06/1987	01/1980
Nebraska		11/1983	11/1987
Nevada	02/1987	08/1983	01/1984
New Hampshire	02/1974 (Reversed 05/1980)	08/1988	02/1974
New Jersey		05/1985	07/1980
New Mexico		02/1980	07/1983
New York		11/1982	
North Carolina			05/1985
North Dakota		02/1984	11/1987
Ohio		04/1982	03/1990
Oklahoma	05/1985 (Reversed 02/1989)	12/1976	02/1989
Oregon		03/1978	06/1975
Pennsylvania			03/1974
Rhode Island			
South Carolina		06/1987	11/1985
South Dakota		04/1983	12/1988
Tennessee		11/1981	08/1984
Texas		04/1985	06/1984
Utah	03/1989	05/1986	03/1989
Vermont		08/1985	09/1986
Virginia		09/1983	06/1985
Washington		08/1977	07/1984
West Virginia		04/1986	07/1978
Wisconsin		06/1985	01/1980
Wyoming	01/1994	08/1985	07/1989

Table 2. Summary Statistics

This table reports the summary statistics (including the mean, median, standard deviation (Std. dev), minimum (Min), and maximum (Max)) of the variables used in the paper. See Appendix A for variable definitions.

	Obs.	Mean	Median	Std. dev.	Min	Max
Payout variables						
Repurchases over total assets	62,518	0.0057	0.0000	0.0163	0.0000	0.0855
Dummy repurchases	62,518	0.2968	0.0000	0.4569	0.0000	1.0000
Repurchases over market cap.	62,518	0.0083	0.0000	0.0233	0.0000	0.1234
Repurchases over book equity	60,476	0.0117	0.0000	0.0334	0.0000	0.1779
Repurchases over sales	62,043	0.0054	0.0000	0.0162	0.0000	0.0873
Repurchases over earnings	45,888	0.1294	0.0000	0.3596	0.0000	1.9442
Repurchases over cash flow	52,467	0.0467	0.0000	0.1300	0.0000	0.6929
Total payout over total assets	62,518	0.0157	0.0032	0.0256	0.0000	0.1216
Dividends over total assets	62,518	0.0092	0.0000	0.0143	0.0000	0.0595
Wrongful discharge laws (WDL) variables						
Good faith	62,518	0.1870	0.0000	0.3899	0.0000	1.0000
Implied contract	62,518	0.5925	1.0000	0.4914	0.0000	1.0000
Public policy	62,518	0.6036	1.0000	0.4891	0.0000	1.0000
Control variables						
Cash flow	62,518	0.0937	0.1267	0.1836	-0.8875	0.4014
Negative earnings	62,518	0.2154	0.0000	0.4111	0.0000	1.0000
Market-to-book	62,518	1.6760	1.2134	1.4384	0.5393	10.0391
NYSE percentile	62,518	0.2286	0.0938	0.2773	0.0000	1.0000
Total liabilities	62,518	0.5006	0.4975	0.2280	0.0508	1.2558
Retained earnings	62,518	0.0171	0.2133	0.8010	-4.7049	0.7906
Cash holdings	62,518	0.1260	0.0633	0.1575	0.0004	0.7845
Idiosyncratic risk	62,518	0.0348	0.0291	0.0214	0.0090	0.1256
Systematic risk	62,518	0.0061	0.0050	0.0047	0.0001	0.0228
Log of firm age	62,518	2.2873	2.3026	0.8447	0.6931	4.2627
Other state-level control variables						
GDP per capita	62,518	32,055	31,753	5,765	15,918	71,804
GDP growth	62,518	7.6952	7.5000	3.7048	-13.8000	30.2000
Political balance	62,499	0.6072	0.6000	0.1604	0.0000	1.0000
Union membership	62,518	0.1963	0.2020	0.0839	0.0330	0.4240
Union coverage	54,757	0.2106	0.2170	0.0795	0.0530	0.3990
RTW laws	62,518	0.2124	0.0000	0.4090	0.0000	1.0000

Table 3. Effect of WDLs on Share Repurchases – Baseline Regression Results

This table reports the regression results on the effect of WDLs on share repurchases based on Eq. (1). *Good faith*, *implied contract*, and *public policy* are the WDL dummy variables equal to one if the state in which a firm is headquartered has adopted the good faith, implied contract, or public policy exceptions, respectively. Our models include firm fixed effects, industry-by-year fixed effects, as well as region-by-year fixed effects and/or state of incorporation-by-year fixed effects. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Good faith	0.0017***	0.0010***	0.0017***	0.0016***	0.0010***	0.0016***
	(3.038)	(3.141)	(3.147)	(3.027)	(3.011)	(3.143)
Implied contract	-0.0001	0.0002	5.37×10^{-5}	-0.0001	0.0001	8.58×10^{-6}
	(-0.362)	(0.474)	(0.162)	(-0.429)	(0.376)	(0.0262)
Public policy	0.0002	-3.26×10^{-5}	0.0003	0.0003	-9.82×10^{-6}	0.0003
	(0.670)	(-0.104)	(0.995)	(0.845)	(-0.0300)	(1.197)
Cash flow				0.0019**	0.0018*	0.0018**
				(2.095)	(1.954)	(2.029)
Negative earnings				-0.0006***	-0.0006***	-0.0006***
				(-3.309)	(-3.557)	(-3.495)
NYSE percentile				-0.0038***	-0.0039***	-0.0038***
				(-5.437)	(-5.323)	(-5.402)
Retained earnings				0.0014***	0.0014***	0.0014***
				(5.961)	(5.676)	(5.692)
Idiosyncratic risk				-0.0449***	-0.0451***	-0.0454***
				(-7.278)	(-8.416)	(-8.449)
Systematic risk				-0.0441***	-0.0380**	-0.0415**
				(-2.720)	(-2.506)	(-2.629)
Log of firm age				0.0007***	0.0006***	0.0006***
				(3.664)	(3.594)	(3.370)
Market-to-book				0.0001	0.0001	0.0001
				(1.063)	(0.990)	(1.029)
Total liabilities				-0.0003	-0.0004	-0.0003
				(-0.606)	(-0.641)	(-0.608)
Cash holdings				0.0031***	0.0031***	0.0031***
				(4.510)	(4.766)	(4.843)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	No	Yes	Yes	No	Yes
StateInc-Year FE	No	Yes	Yes	No	Yes	Yes
Observations	62,518	62,518	62,518	62,518	62,518	62,518
R-squared	0.260	0.272	0.274	0.265	0.277	0.278
State clustering	Yes	Yes	Yes	Yes	Yes	Yes

Table 4. Effect of WDLs on Share Repurchases – Additional Controls

This table reports the regression results on the effect of WDLs on share repurchases, controlling for additional state-level variables, including real GDP per capita, GDP growth rate, union membership density, union coverage density, and the adoption of right-to-work (RTW) laws. *Good faith*, *implied contract*, and *public policy* are the WDL dummy variables, equal to one if the state in which a firm is headquartered has adopted the good faith, implied contract, or public policy exceptions, respectively. Our models include firm fixed effects, industry-by-year fixed effects, region-by-year fixed effects, and state of incorporation-by-year fixed effects. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Good faith	0.0015***	0.0017***	0.0017***	0.0016***	0.0016**	0.0016***	0.0015**
	(3.058)	(3.197)	(3.191)	(3.124)	(2.174)	(3.146)	(2.254)
Implied contract	6.31×10^{-5}	-3.76×10^{-5}	9.13×10^{-6}	3.75×10^{-6}	-2.84×10^{-5}	1.29×10^{-5}	-9.04×10^{-5}
	(0.019)	(-0.114)	(0.028)	(0.012)	(-0.074)	(0.039)	(-0.225)
Public policy	0.0003	0.0003	0.0003	0.0003	0.0005	0.0003	0.0005
	(1.158)	(1.155)	(1.192)	(1.206)	(1.420)	(1.198)	(1.417)
GDP per capita	8.96×10^{-8}						1.26×10^{-7}
	(1.032)						(1.303)
GDP growth		-8.10×10^{-5}*					-0.0001**
		(-1.862)					(-2.424)
Political balance			-0.0004				0.0010
			(-0.464)				(1.058)
Union membership				0.0008			-0.0256
				(0.124)			(-0.879)
Union coverage					-0.0014		0.0193
					(-0.162)		(0.679)
RTW laws						-0.0002	-0.0001
						(-0.287)	(-0.217)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
StateInc-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	62,518	62,518	62,499	62,518	54,709	62,518	54,690
R-squared	0.278	0.278	0.278	0.278	0.294	0.278	0.294
State clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5. Dynamic Effects of WDLs on Share Repurchases – Pre-treatment Trend Analysis

This table reports the regression results on the dynamic effects of WDLs on share repurchases. The good faith indicators equal to one if a firm is headquartered in a state that will adopt the law in one year (*good faith*$^{-1}$), adopted the good faith exception in the current year (*good faith*^{0}), adopted the law one year ago (*good faith*$^{+1}$), and adopted the law two or more years ago (*good faith*$^{>=+2}$). The implied contract indicators are equal to one if a firm is headquartered in a state that will adopt the implied contract exception in one year (*implied contract*$^{-1}$), adopted the law in the current year (*implied contract*^{0}), adopted the law one year ago (*implied contract*$^{+1}$), and adopted the law two or more years ago (*implied contract*$^{>=+2}$). The public policy indicators are equal to one if a firm is headquartered in a state that will adopt the public policy exception in one year (*public policy*$^{-1}$), adopted the law in the current year (*public policy* ^{0}), adopted the law one year ago (*public policy*$^{+1}$), and adopted the law two or more years ago (*public policy*$^{>=+2}$). The control variables are the same as specified in Table 3. All models include firm fixed effects, industry-by-year fixed effects, region-by-year fixed effects, and state of incorporation-by-year fixed effects. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Good faith^{-1}	0.0012	0.0008	0.0013*	0.0012	0.0008	0.0013
	(1.425)	(1.358)	(1.727)	(1.414)	(1.312)	(1.666)
Good faith^{0}	0.0023***	0.0015***	0.0023***	0.0023***	0.0014**	0.0022***
	(2.993)	(2.911)	(3.214)	(2.938)	(2.652)	(3.043)
Good faith^{+1}	0.0021***	0.0014**	0.0021**	0.0020**	0.0014**	0.0020**
	(2.778)	(2.481)	(2.567)	(2.610)	(2.338)	(2.467)
Good faith$^{>=+2}$	0.0018**	0.0012***	0.0020***	0.0017**	0.0011**	0.0019***
	(2.467)	(2.730)	(2.751)	(2.484)	(2.636)	(2.783)
Implied contract^{-1}	0.0002	0.0002	-0.0001	0.0002	0.0003	-5.87×10^{-5}
	(0.498)	(0.591)	(-0.230)	(0.558)	(0.668)	(-0.133)
Implied contract^{0}	-8.72×10^{-6}	-9.60×10^{-5}	-0.0002	-1.64×10^{-5}	-0.0001	-0.0002
	(-0.020)	(-0.216)	(-0.485)	(-0.038)	(-0.231)	(-0.504)
Implied contract^{+1}	3.16×10^{-6}	6.36×10^{-5}	-0.0002	-1.14×10^{-5}	3.32×10^{-5}	-0.0002
	(0.007)	(0.134)	(-0.320)	(-0.026)	(0.067)	(-0.359)
Implied contract$^{>=+2}$	-0.0001	0.0004	0.0002	-0.0002	0.0004	0.0001
	(-0.274)	(0.854)	(0.370)	(-0.327)	(0.782)	(0.257)
Public policy^{-1}	-4.87×10^{-5}	-0.0002	8.42×10^{-5}	-3.79×10^{-5}	-0.0002	-3.79×10^{-5}
	(-0.087)	(-0.281)	(0.136)	(-0.067)	(-0.291)	(0.116)
Public policy^{0}	0.0002	-0.0005	-0.0002	0.0002	-0.0005	-0.0001
	(0.406)	(-1.499)	(-0.471)	(0.495)	(-1.481)	(-0.417)
Public policy^{+1}	0.0003	1.58×10^{-5}	0.0004	0.0004	3.41×10^{-5}	0.0005
	(0.559)	(0.028)	(0.860)	(0.636)	(0.057)	(0.926)
Public policy$^{>=+2}$	0.0002	-1.13×10^{-5}	0.0004	0.0003	1.77×10^{-5}	0.0005
	(0.428)	(-0.025)	(0.923)	(0.596)	(0.038)	(1.127)
Controls	No	No	No	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	No	Yes	Yes	No	Yes
StateInc-Year FE	No	Yes	Yes	No	Yes	Yes
Observations	62,518	62,518	62,518	62,518	62,518	62,518
R-squared	0.260	0.272	0.274	0.265	0.277	0.278
State clustering	Yes	Yes	Yes	Yes	Yes	Yes

Table 6. Effect of WDLs on Share Repurchases – Propensity Score Matching Analysis

This table reports the results from our propensity score matching analysis. We match control and treatment firms in year *t*-1 on industry (using the same 3-digit SIC code) and all control variables specified in our baseline model (Eq. (1)). The treatment group consists of firms headquartered in states that adopt the good faith exception in year t. The control group consists of (i) firms headquartered in states that never adopt the good faith exception and (ii) those headquartered in states that adopt the exception outside of the test window, which is specified to be ±3 years around year *t*. Both treatment and control firms must have data available in at least one year in both the pre- and post-treatment years. Panel A reports the means of the variables for the treatment and control firms in year *t*-1. *t*-stat represents the *t*-statistics of the test of equal means. Panel B reports the regression results on the effect of WDLs on share repurchases using the propensity-score-matched samples described above. *Treated* is a dummy variable equal to one if the firm belongs to the treatment group, i.e., if the state in which the firm is headquarter has recognized the good faith exception in year t and zero otherwise. *Post* is a dummy variable equal to one in the period after the recognition of the exception and zero otherwise. The control variables are the same as specified in Table 3. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Control and treatment firms

	Control group		Treatment group		
	N	Mean	N	Mean	t-stat
Cash flow	396	0.1414	396	0.1411	0.036
Negative earnings	396	0.1162	396	0.1263	-0.435
NYSE percentile	396	0.2091	396	0.2041	0.263
Retained earnings	396	0.2045	396	0.1839	0.672
Idiosyncratic risk	396	0.0284	396	0.0298	-1.346
Systematic risk	396	0.0067	396	0.0068	-0.448
Log of firm age	396	2.2266	396	2.1879	0.727
Market-to-book	396	1.5815	396	1.5512	0.307
Total liabilities	396	0.4981	396	0.4886	0.666
Cash holdings	396	0.1044	396	0.1062	-0.201

Panel B. Regression results using propensity-score-matched sample

	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.0014	-0.0004	0.0007	0.0008	-0.0006	0.0002
	(0.512)	(-0.149)	(0.264)	(0.317)	(-0.264)	(0.106)
Post	-0.0010	-0.0012	-0.0019**	-0.0011	-0.0011	-0.0019*
	(-0.942)	(-1.345)	(-2.077)	(-0.955)	(-1.239)	(-1.929)
Treated×Post	0.0031**	0.0021**	0.0030**	0.0030**	0.0020**	0.0030**
	(2.528)	(2.216)	(2.056)	(2.609)	(2.051)	(2.116)
Controls	No	No	No	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	No	Yes	Yes	No	Yes
StateInc-Year FE	No	Yes	Yes	No	Yes	Yes
Observations	5,014	4,810	4,810	5,014	4,810	4,810
R-squared	0.406	0.459	0.471	0.410	0.463	0.476
State clustering	Yes	Yes	Yes	Yes	Yes	Yes

Table 7. Placebo Tests

This table reports the regression results on the effect of WDLs on share repurchases in placebo tests. *Good faith*, *implied contract*, and *public policy* are the WDL dummy variables, equal to one if the state in which a firm is headquartered has been randomly assigned to the year in which the good faith was adopted. The model includes firm fixed effects, industry-by-year fixed effects, as well as region-by-year fixed effects and/or state of incorporation-by-year fixed effects. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Good faith	-0.0008	-0.0003	-0.0006	-0.0008	-0.0003	-0.0007
	(-1.567)	(-0.716)	(-1.086)	(-1.512)	(-0.706)	(-1.119)
Cash flow				0.0018**	0.0018*	0.0018**
				(2.074)	(1.953)	(2.033)
Negative earnings				-0.0006***	-0.0006***	-0.0006***
				(-3.326)	(-3.533)	(-3.500)
NYSE percentile				-0.0038***	-0.0038***	-0.0038***
				(-5.477)	(-5.303)	(-5.454)
Retained earnings				0.0014***	0.0014***	0.0014***
				(6.042)	(5.691)	(5.749)
Idiosyncratic risk				-0.0449***	-0.0452***	-0.0455***
				(-7.265)	(-8.446)	(-8.521)
Systematic risk				-0.0445***	-0.0383**	-0.0417**
				(-2.764)	(-2.536)	(-2.639)
Log of firm age				0.0007***	0.0006***	0.0006***
				(3.675)	(3.671)	(3.402)
Market-to-book				0.0001	0.0001	0.0001
				(1.056)	(0.972)	(1.018)
Total liabilities				-0.0003	-0.0004	-0.0003
				(-0.581)	(-0.648)	(-0.592)
Cash holdings				0.0031***	0.0031***	0.0031***
				(4.502)	(4.768)	(4.843)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	No	Yes	Yes	No	Yes
StateInc-Year FE	No	Yes	Yes	No	Yes	Yes
Observations	62,518	62,518	62,518	62,518	62,518	62,518
R-squared	0.260	0.272	0.274	0.265	0.277	0.278
State clustering	Yes	Yes	Yes	Yes	Yes	Yes

Table 8. Propensity of Share Repurchases

This table reports the regression results on the effect of WDLs on the propensity of share repurchases. *Good faith*, *implied contract*, and *public policy* are the WDL dummy variables equal to one if the state in which a firm is headquartered has adopted the good faith, implied contract, or public policy exceptions, respectively. The model includes firm fixed effects, industry-by-year fixed effects, as well as region-by-year fixed effects and/or state of incorporation-by-year fixed effects. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Good faith	0.0397***	0.0271*	0.0322**	0.0381***	0.0258*	0.0306**
	(3.284)	(1.976)	(2.314)	(3.071)	(1.895)	(2.129)
Implied contract	0.0159	0.0251*	0.0240*	0.0148	0.0234*	0.0218*
	(1.655)	(1.946)	(1.886)	(1.474)	(1.790)	(1.703)
Public policy	-0.0081	-0.0046	0.0030	-0.0046	-0.0013	0.0072
	(-1.076)	(-0.377)	(0.267)	(-0.630)	(-0.118)	(0.682)
Cash flow				-0.0791***	-0.0783***	-0.0792***
				(-4.587)	(-4.125)	(-4.228)
Negative earnings				-0.0280***	-0.0286***	-0.0285***
				(-5.272)	(-5.168)	(-5.075)
NYSE percentile				-0.0228	-0.0269	-0.0262
				(-0.751)	(-0.868)	(-0.844)
Retained earnings				0.0494***	0.0489***	0.0495***
				(8.570)	(7.615)	(7.742)
Idiosyncratic risk				-1.4090***	-1.4220***	-1.4270***
				(-10.420)	(-10.730)	(-10.840)
Systematic risk				-0.3950	-0.4500	-0.4460
				(-0.688)	(-0.836)	(-0.835)
Log of firm age				0.0150**	0.0149**	0.0146**
				(2.330)	(2.082)	(2.136)
Market-to-book				-0.0130***	-0.0132***	-0.0130***
				(-4.951)	(-4.729)	(-4.630)
Total liabilities				-0.0951***	-0.0920***	-0.0921***
				(-5.235)	(-5.143)	(-5.131)
Cash holdings				0.0175	0.0224	0.0210
				(0.723)	(0.943)	(0.861)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	No	Yes	Yes	No	Yes
StateInc-Year FE	No	Yes	Yes	No	Yes	Yes
Observations	62,518	62,518	62,518	62,518	62,518	62,518
R-squared	0.377	0.388	0.389	0.384	0.395	0.396
State clustering	Yes	Yes	Yes	Yes	Yes	Yes

Table 9. Alternative Measures of Share Repurchases

This table reports the regression results on the effect of WDLs on alternative measures of share repurchases based on Eq. (1). *Good faith*, *implied contract*, and *public policy* are the WDL dummy variables equal to one if the state in which a firm is headquartered has adopted the good faith, implied contract, or public policy exceptions, respectively. All models include firm fixed effects, industry-by-year fixed effects, region-by-year fixed effects and state of incorporation-by-year fixed effects. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Repurchases over market capitalization	(2) Repurchases over market capitalization	(3) Repurchases over book equity	(4) Repurchases over book equity	(5) Repurchases over cash flow	(6) Repurchases over cash flow	(7) Repurchases over sales	(8) Repurchases over sales
Good faith	0.0032***	0.0029***	0.0037***	0.0033**	0.0172***	0.0146**	0.0017**	0.0016**
	(3.563)	(3.187)	(2.855)	(2.646)	(3.035)	(2.604)	(2.656)	(2.514)
Implied contract	0.0006	0.0006	6.72×10^{-6}	0.0001	0.0007	0.0012	-0.0001	-0.0001
	(1.157)	(1.189)	(0.009)	(0.192)	(0.233)	(0.428)	(-0.372)	(-0.439)
Public policy	0.0002	0.0003	0.0008	0.0008	-0.0019	-0.0003	0.0004	0.0005*
	(0.364)	(0.594)	(1.236)	(1.218)	(-0.634)	(-0.085)	(1.545)	(1.867)
Cash flow		-0.0004		0.0062**		-0.1210***		-0.0012
		(-0.415)		(2.578)		(-8.590)		(-1.471)
Negative earnings		-0.0006*		-0.0008*		0.0129***		-0.0005**
		(-1.697)		(-1.855)		(5.370)		(-2.640)
NYSE percentile		-0.0147***		-0.0091***		-0.0450***		-0.0029***
		(-15.430)		(-5.835)		(-5.966)		(-3.191)
Retained earnings		0.0023***		0.0033***		0.0400***		0.0017***
		(7.465)		(5.642)		(7.894)		(6.093)
Idiosyncratic risk		-0.0727***		-0.1040***		-0.4450***		-0.0474***
		(-8.361)		(-8.803)		(-6.538)		(-7.634)
Systematic risk		-0.0648***		-0.0792**		-0.6640***		-0.0387**
		(-2.759)		(-2.308)		(-4.724)		(-2.556)
Log of firm age		0.0016***		0.0008*		0.0032**		0.0002
		(5.419)		(1.911)		(2.336)		(0.870)
Market-to-book		-0.0007***		0.0004*		-0.0012		-0.0001
		(-5.393)		(1.818)		(-0.994)		(-1.124)
Total liabilities		0.0021***		0.0158***		0.0163***		0.0002
		(3.208)		(12.150)		(2.756)		(0.331)
Cash holdings		0.0030***		0.0068***		0.0470***		0.0066***
		(3.093)		(5.333)		(5.896)		(8.894)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
StateInc-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	62,518	62,518	60,331	60,331	52,030	52,030	62,004	62,004
R-squared	0.244	0.251	0.273	0.278	0.264	0.274	0.291	0.296
State clustering	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 10. Dividends, Total Payout, and Payout Flexibility

This table reports the regression results on the effect of WDLs on different measures of payout, namely dividends scaled by total assets, total payout scaled by total assets, and repurchases scaled by total payout. *Good faith*, *implied contract*, and *public policy* are the WDL dummy variables equal to one if the state in which a firm is headquartered has adopted the good faith, implied contract, or public policy exceptions, respectively. All models include firm fixed effects, industry-by-year fixed effects, region-by-year fixed effects, and state of incorporation-by-year fixed effects. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Dividends over total assets	(2) Dividends over total assets	(3) Total payout over total assets	(4) Total payout over total assets	(5) Repurchases over total payout	(6) Repurchases over total payout
Good faith	-0.0006	-0.0003	0.0011	0.0013	0.0456***	0.0361**
	(-0.979)	(-0.580)	(1.161)	(1.443)	(2.872)	(2.374)
Implied contract	0.0002	-1.68×10^{-5}	0.0004	0.0001	-0.0106	-0.0061
	(0.814)	(-0.067)	(0.788)	(0.264)	(-0.870)	(-0.499)
Public policy	-0.0002	-0.0001	4.74×10^{-5}	0.0002	0.0036	0.0003
	(-0.602)	(-0.362)	(0.083)	(0.353)	(0.411)	(0.034)
Cash flow		0.0025***		0.0057***		0.2030***
		(3.582)		(3.376)		(6.381)
Negative earnings		9.54×10^{-5}		-0.0005		-0.0015
		(0.669)		(-1.564)		(-0.166)
NYSE percentile		0.0111***		0.0069***		-0.3050***
		(10.230)		(4.014)		(-10.390)
Retained earnings		3.83×10^{-5}		0.0014***		-0.0085
		(0.367)		(4.476)		(-0.469)
Idiosyncratic risk		-0.0148***		-0.0647***		2.4310***
		(-5.701)		(-8.686)		(9.630)
Systematic risk		-0.0942***		-0.1610***		-2.3530***
		(-7.505)		(-5.939)		(-3.726)
Log of firm age		0.0019***		0.0027***		-0.0470***
		(9.634)		(9.473)		(-4.040)
Market-to-book		0.0001***		0.0004***		-0.0031
		(2.751)		(2.799)		(-0.701)
Total liabilities		-0.0065***		-0.0067***		0.1030***
		(-7.053)		(-5.117)		(3.093)
Cash holdings		0.0017**		0.0066***		0.0120
		(2.363)		(4.277)		(0.465)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
StateInc-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	62,518	62,518	62,518	62,518	33,937	33,937
R-squared	0.812	0.825	0.521	0.531	0.684	0.693
State clustering	Yes	Yes	Yes	Yes	Yes	Yes

Table 11. The Role of Financial Constraints

This table reports the regression results on the effect of WDLs on share repurchases conditional on the degree of financial constraint. *Good faith*, *implied contract*, and *public policy* are the WDL dummy variables equal to one if the state in which a firm is headquartered has adopted the good faith, implied contract, or public policy exceptions, respectively. *High EFD*, *High HP*, and *High WP* are dummy variables equal to one if the firm is financially more constrained because it has a high external finance dependence index score (Rajan and Zingales, 1998), a high Hadlock and Pierce (2010) size and age index score, or a high Whited and Wu (2006) index score, respectively. All models include firm fixed effects, industry-by-year fixed effects, region-by-year fixed effects, and state of incorporation-by-year fixed effects. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Repurchases over total assets	(2) Repurchases over total assets	(3) Repurchases over total assets	(4) Repurchases over total assets	(5) Repurchases over total assets	(7) Repurchases over total assets
Good faith	0.0026***	0.0024***	0.0025***	0.0024***	0.0027***	0.0026***
	(4.324)	(4.160)	(3.421)	(3.468)	(4.101)	(4.231)
Implied contract	0.0001	8.96×10^{-5}	6.21×10^{-5}	1.60×10^{-5}	8.18×10^{-5}	3.87×10^{-55}
	(0.345)	(0.271)	(0.190)	(0.049)	(0.250)	(0.120)
Public policy	0.0003	0.0004	0.0003	0.0003	0.0002	0.0003
	(1.233)	(1.450)	(1.011)	(1.187)	(0.885)	(1.033)
High EFD	-0.0014***	-0.0011***				
	(-8.474)	(-6.743)				
Good faith×High EFD	-0.0015***	-0.0013**				
	(-2.984)	(-2.588)				
High HP			0.0008**	0.0012***		
			(2.284)	(3.106)		
Good faith×High HP			-0.0023***	-0.0022***		
			(-4.062)	(-4.113)		
High WW					-0.0001	0.0003
					(-0.497)	(0.977)
Good faith×High WW					-0.0027***	-0.0026***
					(-6.227)	(-6.611)
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
StateInc-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	60,427	60,427	62,518	62,518	60,706	60,706
R-squared	0.279	0.283	0.274	0.278	0.277	0.282
State clustering	Yes	Yes	Yes	Yes	Yes	Yes

Table 12. The Role of Governance – Institutional Ownership

This table reports the regression results on the effect of WDLs on share repurchases conditional on the quality of firm governance. *Good faith*, *implied contract*, and *public policy* are the WDL dummy variables equal to one if the state in which a firm is headquartered has adopted the good faith, implied contract, or public policy exceptions, respectively. *High IO* is a dummy variable equal to one if the firm has a high fraction of institutional ownership and thus a better governance mechanism. All models include firm fixed effects, industry-by-year fixed effects, region-by-year fixed effects, and state of incorporation-by-year fixed effects. See Appendix A for variable definitions. *T*-statistics reported in parentheses are based on robust standard errors clustered at the state of location level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Repurchases over total assets	(2) Repurchases over total assets
Good faith	0.0007	0.0008
	(0.319)	(0.338)
Implied contract	0.0003	0.0002
	(0.442)	(0.301)
Public policy	0.0013***	0.0012**
	(2.686)	(2.381)
High IO	-0.0005	-0.0011**
	(-1.024)	(-2.215)
Good faith × High IO	0.0018**	0.0016**
	(2.460)	(2.362)
Controls	Yes	Yes
Firm FE	Yes	Yes
Industry-Year FE	Yes	Yes
Region-Year FE	Yes	Yes
StateInc-Year FE	Yes	Yes
Observations	31,631	31,631
R-squared	0.333	0.337
State clustering	Yes	Yes

Appendix A. Variable Definitions

Variable name	Definition (Compustat item names are reported in italics)
Payout variables	
Repurchases over total assets	Purchase of common and preferred stock (*PRSTKC*) minus the reduction in the book value of preferred stock (*PSTKRV*), all scaled by total assets (*AT)*.
Dummy repurchases	Binary variable that is equal to one for positive stock repurchases, otherwise zero.
Repurchases over market capitalization	Purchase of common and preferred stock (*PRSTKC)* minus the reduction in the book value of preferred stock (*PSTKRV*), all scaled by market capitalization (*PRCC* times *CSHO*).
Repurchases over book equity	Purchase of common and preferred stock (*PRSTKC*) minus the reduction in the book value of preferred stock (*PSTKRV*), all scaled by book equity. Book equity is stockholders' equity (*SEQ*) or book common equity (*CEQ*) plus book preferred stock (*PSTK*) or total assets (*AT)* minus total liabilities (*LT*), minus preferred stock (defined below), plus deferred taxes and investment tax credit (*TXDITC*), if available, minus the postretirement benefit asset (*PRBA*), if available. Preferred stock is the liquidating value of preferred stock (*PSTKL*) or the redemption value of preferred stock (*PSTKRV*) or the par value of preferred stock (*PSTK*). If items *PSTKL*, *PSTKRV*, and *PSTV* are not available, preferred stock is set to zero.
Repurchases over cash flow	Purchase of common and preferred stock (*PRSTKC*) minus the reduction in the book value of preferred stock (*PSTKRV*), all scaled by operating income before depreciation (*OIBDP*).
Repurchases over sales	Purchase of common and preferred stock (*PRSTKC*) minus the reduction in the book value of preferred stock (*PSTKRV*), all scaled by sales (*SALE*).
Dividends over total assets	Common dividends (*DVC*) over total assets (*AT*).
Total payout over total assets	Sum of repurchases (*PRSTKC* minus the reduction in *PSTKRV*) and dividends (*DVC*), all scaled total assets (*AT*)
Repurchases over total payout	Repurchases (*PRSTKC* minus the reduction in *PSTKRV*) over the sum of repurchases and dividends (*DVC*).
Wrongful discharge laws (WDL) variables	
Good faith	Binary variable that is set to one if the state of the firm's headquarters has adopted the good faith exception by the end of the current fiscal year, otherwise zero.
Implied contract	Binary variable that is set to one if the state of the firm's headquarters has adopted the implied contract exception by the end of the current fiscal year, otherwise zero.
Public policy	Binary variable that is set to one if the state of the firm's headquarters has adopted the public policy exception by the end of the current fiscal year, otherwise zero.
Firm-specific controls	
Cash flow	Operating income before depreciation (*OIBDP*) over total assets (*AT*).

Negative earnings	Binary variable that is equal to one if earnings before interest are negative, otherwise zero. Earnings before interest is income before extraordinary items (*IB*) plus interest and related expenses (*XINT*), if available, plus income statement deferred taxes (*TXDI*), if available.
NYSE percentile	Fraction of NYSE firms with a lower or equal market capitalization in the same year. Market capitalization is stock price times the number of outstanding shares (*PRCC* times *CSHO*).
Retained earnings	Retained earnings (*RE*) over total assets (*AT*).
Idiosyncratic risk	Standard deviation of the residuals from a regression of the daily stock return (source: CRSP) in excess of the risk free rate (from Kenneth French's website) on the market factor based on the value-weighted market return (source: CRSP). Daily returns over the fiscal year are used.
Systematic risk	Standard deviation of the predicted value from a regression of the daily stock return (source: CRSP) in excess of the risk free rate (from Kenneth French's website) on the market factor based on the value-weighted market return (source: CRSP). Daily returns over the fiscal year are used.
Log of firm age	Log of one plus the number of years since the firm's first appearance in CRSP.
Market-to-book	Firm market value over total assets (*AT*). Firm market value is total assets minus book equity plus market capitalization. Book equity is stockholders' equity (*SEQ*) or book common equity (*CEQ*) plus book preferred stock (*PSTK*) or total assets (*AT*) minus total liabilities (*LT*), minus preferred stock (defined below), plus deferred taxes and investment tax credit (*TXDITC*), if available, minus the postretirement benefit asset (*PRBA*), if available. Preferred stock is the liquidating value of preferred stock (*PSTKL*) or the redemption value of preferred stock (*PSTKRV*) or the par value of preferred stock (*PSTK*). If items *PSTKL*, *PSTKRV*, and *PSTV* are not available, preferred stock is set to zero. Market capitalization is stock price times the number of outstanding shares (*PRCC* times *CSHO*).
Total liabilities	Total liabilities (*LT*) over total assets (*AT*).
Cash holdings	Cash and short-term investments (*CHE*) over total assets (*AT*).

State-specific controls

GDP per capita	Real state-level GDP over state-level population (source: Bureau of Economic Analysis). Nominal GDP figures are inflation-adjusted using the GDP implicit price deflator (set to 100 in 2010; source: FRED).
GDP growth	State-level GDP percent change (source: Bureau of Economic Analysis).
Political balance	State-level fraction of the members of the House of Representatives from the Democratic Party in the current year.
Union membership density	State-level percentage of nonagricoltural wage and salary employees who are union members. Source: www.unionstats.com. See Hirsch, Macpherson, and Vroman (2001) for more details.
Union coverage density	State-level percentage of nonagricoltural wage and salary employees who are covered by a collective bargaining agreement. Source: www.unionstats.com. See Hirsch, Macpherson, and Vroman (2001) for more details.
RTW laws	Binary variable that is set to one if the state of the firm's headquarters has passed right-to-work laws by the end of the current fiscal year, otherwise zero.

Conditioning variables

High EFD	Binary variable that is set to one when external financial dependence is greater than or equal to its median value, otherwise zero (Rajan and Zingales, 1998). External financial dependence is capital expenditures (*CAPX*) minus funds from operations, over capital expenditures. If *SCF* is equal to seven, the variable funds from operations is computed as follows: *OANCF-AOLOCH-APALCH-INVCH-RECCH-TXACH*. Otherwise, funds from operations is equal to *FOPT*.
High HP	Binary variable that is set to one when the HP index is greater than or equal to its median value, otherwise zero (Hadlock and Pierce, 2010). The HP index is computed as follows: -0.737 times the log of inflation-adjusted (at 2004 prices) total assets (*AT*), plus 0.043 times the squared value of the log of inflation-adjusted total assets, minus 0.04 times firm age. Firm age is the number of years since the firm's first appearance in Compustat. The variable inflation-adjusted total assets is capped at 4500 while firm age is capped at 37.
High WW	Binary variable that is set to one when the WW index is greater than or equal to its median value, otherwise zero (Whited and Wu, 2006). The WW index is computed as follows: -0.091 times ((*ib*+*dp*)/*at*) minus 0.062 times dummy for positive dividends plus 0.021 times *dltt*/*at* minus 0.044 times log(*at*) plus 0.102 times industry sales growth minus 0.035 times firm sales growth. The dummy for positive dividends is set to one when *DVC* plus *DVP* is larger than zero. Firm sales growth is the relative change in *SALE* while industry sales growth is the average value of the same variable for the three-digit SIC industry to which the firm belongs.
High IO	Binary variable that is set to one when institutional ownership is greater than or equal to its median value, otherwise zero. Institutional ownership is the fraction of common shares outstanding held by institutional investors (source: Thomson Reuters Institutional Holdings database).