

Firing Costs and Capital Structure Decisions

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ABSTRACT

I exploit the adoption of state-level labor protection laws as an exogenous increase in employee firing costs to examine how the costs associated with discharging workers affect capital structure decisions. I find that firms reduce debt ratios following the adoption of these laws, with this result stronger for firms that experience larger increases in firing costs. I also document that, following the adoption of these laws, a firm's degree of operating leverage rises, earnings variability increases, and employment becomes more rigid. Overall, these results are consistent with higher firing costs crowding out financial leverage via increasing financial distress costs.

Keywords: Capital structure, Firing costs, Employment protection, Financial distress costs

JEL Classifications: G32, G33, J63, K3

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How do the costs associated with discharging workers (i.e., “firing costs”) affect capital structure decisions? Prior work finds that firms with higher financial leverage ratios and those in technical default are more likely to discharge workers (e.g., Sharpe (1994), Hanka (1998), Falato and Liang (2015)).¹ Yet, discharging workers can involve hidden costs, such as those arising from collective bargaining agreements, discrimination claims, and wrongful termination lawsuits. These employee firing costs create frictions that constrain a firm’s ability to discharge workers. While it is well recognized in economics that firing costs can have a significant effect on real outcomes such as wages, employment, and investment, it is less clear how these costs relate to financing decisions.

From a theoretical perspective, the effect of firing costs on capital structure decisions is ambiguous. On the one hand, an increase in firing costs could lead to higher debt ratios by lowering employees’ risk of unemployment. Titman (1984) and Agrawal and Matsa (2013) suggest that employees demand a wage premium for bearing the increased risk of unemployment that arises from using financial leverage. Thus, if higher firing costs reduce an employee’s risk of dismissal, this wage premium shrinks and firms could increase debt ratios so as to capture a larger share of the tax benefits of debt.

On the other hand, higher firing costs could lower optimal debt ratios by increasing financial distress costs. First, because distressed firms are often forced to discharge workers to cover cash flow shortfalls (e.g., Ofek (1993), Kang and Shivdasani (1997)), the additional firing costs incurred as a result of discharging workers increase the total costs of distress. Second, higher firing costs make it more difficult to reduce employment when firms need to do so, such as during economic downturns (e.g., Bentolila and Bertola (1990), Autor, Donohue, and Schwab (2006), Messina and Vallanti (2007)). This effect can make labor costs more fixed in nature, which can raise operating leverage and increase a firm’s risk of becoming distressed (e.g., Mandelker and Rhee (1984), Mauer and Triantis (1994), Kahl, Lunn, and Nilsson (2014)).

To test these competing predictions, one needs to overcome the endogeneity problem associated with the fact that a firm’s firing costs are determined in part by its financial leverage. In particular, firing costs are a function of the expected costs from firing employees combined with the

probability of firing such employees, which increases with a firm's debt ratio. A second concern is that firing costs are unobservable. However, even if they were observable, it is likely impossible to control for all factors that affect both leverage and firing costs. In this paper, I attempt to identify the causal effect of firing costs on capital structures by exploiting the quasi-natural experiment created by the adoption of Wrongful Discharge Laws (WDLs) by U.S. state courts over the period 1967 to 1995.

WDLs matured into three common law exceptions to "at-will employment" in an effort to protect against wrongful termination.² I focus on the effect of the adoption of one particular WDL—the good faith exception. In its broadest sense, this exception applies when a court determines that an employer discharged a worker out of bad faith, malice, or retaliation. In these cases, employees can recover contractual losses and punitive damages. This exception represents the largest deviation from at-will employment and is arguably the most far reaching of the three (e.g., Dertouzos and Karoly (1992), Kugler and Saint-Paul (2004)).

Importantly, WDLs increase firing costs. Jung (1997) finds that plaintiffs won \$1.29 million on average in 1996, and Boxold (2008) documents average (maximum) awards of \$0.59 million (\$5.4 million) over the 2001 to 2007 period. While these average awards are arguably small for large firms, firms can be subject to several lawsuits at any point in time, which can substantially raise their legal liability.³ Also, the fear of very large settlements could alter the behavior of risk-averse managers (Dertouzos, Holland, and Ebener (1988)), as 46% of surveyed public firms' managers express concerns regarding losses arising from such lawsuits.⁴ Consistent with these laws increasing firing costs, prior research finds that employment levels, employment volatility, and firm entry decrease following the adoption of the good faith exception (Dertouzos and Karoly (1992), Autor, Kerr, and Kugler (2007)). I also find that firms experience cumulative abnormal stock returns of -1.05% to -1.22% and a reduction in the market value of equity of \$4.3 to \$5.0 million when their state adopts this law, suggesting that the law's adoption is not only costly but also partially unanticipated.

To test for the effect on capital structure decisions, I use a difference-in-differences approach in which the treatment and control groups consist of firms headquartered in states that

have and have not adopted the good faith exception, respectively. The regressions control for firm and year fixed effects and firm characteristics known to affect capital structure decisions. I also control for several state-level variables to help ensure that economic and political conditions do not spuriously drive the results. I find that, following the adoption of the good faith exception, book and market leverage ratios decrease by 1.5 and 1.0 percentage points, respectively. Compared to their respective sample means, book and market leverage decrease by 6.1% and 3.6%. These results continue to hold when I use alternative measures of leverage, such as leverage net of cash holdings.

The key identifying assumption central to a causal interpretation of the results is that, in the absence of the treatment, the average change in debt ratios would have been the same for both treatment and control firms. Several features of WDLs and results from a variety of robustness tests suggest that this parallel trends assumption is satisfied. First, because WDLs are based in common law, a judge's decision to adopt the good faith exception is more likely due to the merits of the case than political and economic factors (e.g., Walsh and Schwarz (1996)). In line with this view, I find that out of a large set of economic and political variables that potentially affect whether courts adopt the good faith exception, almost none of them are correlated with the law's adoption. Second, due to the staggered adoption of the good faith exception, firms can be in both the treatment group and the control group at different times, which helps alleviate concerns about large differences between treatment and control firms. Third, I find that changes in leverage appear only after the passage of this law and that this result is robust to controlling for state-specific time trends in leverage. The results are also robust to matching treatment to control firms based on year, industry, and firm characteristics.

I also exploit two sources of cross-sectional variation in industry characteristics and estimate triple-difference regression models. These tests not only shed further light on the mechanisms underlying my results but also address econometric concerns by controlling for state \times year fixed effects that difference out all confounding factors that vary at the state-year level. First, because firms that discharge workers more frequently face a higher risk of wrongful termination lawsuits, these firms should reduce debt more following the adoption of the good faith exception. Second,

WDLs do not generally pertain to temporary or unionized employees (Miles (2000), Autor (2003)), and thus firms operating in industries that employ a smaller fraction of these workers should also reduce debt more following the law's adoption. Consistent with these predictions, declines in leverage following the adoption of the good faith exception are larger for firms operating in industries and states that have more employee discharges, volatile earnings, full-time workers, or nonunionized workers.

Overall, my findings are consistent with higher firing costs affecting debt ratios via increased financial distress costs. While I am unable to test how much of this effect is due to an increase in financial losses once a firm becomes distressed, I show that higher firing costs affect risk and employment decisions in ways that are consistent with increased operating leverage. In particular, I find that the sensitivity of changes in earnings to changes in sales (i.e., degree of operating leverage) is higher following the adoption of the good faith exception. Earnings are also less persistent and display greater year-over-year variation. Further, firms are less likely to discharge workers after a decline in earnings, employment flows are less volatile, and variation in employment is less sensitive to variation in earnings.

In sum, this paper documents that firms reduce financial leverage when it becomes more expensive to discharge workers, and that the effect of employee firing costs on capital structure works largely through traditional trade-off theory mechanisms rather than pure labor mechanisms, such as workers' unemployment risk (e.g., Agrawal and Matsa (2013), Kim (2013)). This study thus broadly contributes to the literature examining the determinants of capital structure decisions (e.g., Titman and Wessels (1988), Rajan and Zingales (1995), Lemmon, Roberts, and Zender (2008)).

My paper is not the first to document results consistent with less flexible labor markets crowding out financial leverage by increasing operating leverage. In a closely related paper, Simintzi, Vig, and Volpin (2015) find a negative relation between the debt ratios of firms in a number of countries and an index of country-level employment protection legislation. My study is distinct, however, in that I provide empirical evidence that an increase in firing costs raises operating leverage. Further, I study the effect of an individual employment law on firms in a single country (the

U.S.), which is advantageous because it allows me to employ a relatively homogeneous sample in terms of legal structure, financial and economic development, infrastructure, etc. In addition, because the U.S. has substantially less rigid labor protection laws compared to European countries, there is much less research on the effects of these laws in the U.S. Notwithstanding, a significant number of firms in the U.S. are subject to employment protection legislation and lawsuits (wrongful termination lawsuits rose 260% over a recent 20-year period and 40.7% between 2005 and 2010 (Boxold (2008), Haider and Plancich (2012)).⁵ My study therefore provides insights on how U.S. employment protection laws can also shape corporate financing behaviors.

In other related work, Schmalz (2015) shows that unionization, which increases labor adjustment costs, causes financially unconstrained U.S. firms to decrease debt ratios and increase cash holdings, and Kuzmina (2013) documents that lower operating flexibility associated with employing more full-time workers results in Spanish manufacturing firms lowering financial leverage. While these papers and my own have similarities, each study has a different research design, including sample construction (e.g., U.S. vs. international and manufacturing vs. all industrial), estimation strategy (differences-in-differences, regression discontinuity, and instrumental variables), and worker focus (unionized vs. nonunionized and temporary vs. full-time). Thus, while each study has strengths and weaknesses with respect to external and internal validity, the consistent results across the studies complement one another.

My study also extends prior work that focuses largely on the real effects of WDLs, such as employment outcomes (e.g., Dertouzos and Karoly (1992), Autor (2003), Autor, Donohue, and Schwab (2006)), capital expenditures and productivity (Autor, Kerr, and Kugler (2007)), and innovation (Acharya, Baghai, and Subramanian (2014)). One implication of my findings is that some of the results in these papers could be related to the effect of WDLs on firms' financing decisions.

The remainder of the paper is organized as follows. Section I develops the theoretical link between firing costs and capital structure decisions. Section II discusses institutional background, identification, and the endogeneity of WDLs. Section III describes the data and empirical methodology. Section IV reports the main empirical results. Section V discusses alternative

explanations. Section VI concludes.

I. Theoretical Link between Firing Costs and Capital Structure

Why would higher employee firing costs affect capital structure decisions? On the one hand, an increase in firing costs could lead to higher debt ratios by lowering employees' unemployment risk. Titman (1984) and Agrawal and Matsa (2013) suggest that firms trade off the benefits of debt financing against higher wages demanded by workers in compensation for bearing higher unemployment risk in the event the firm goes bankrupt or experiences severe financial distress. In particular, because the use of more debt financing increases the likelihood that the firm will become distressed and discharge workers, employees demand a wage premium for bearing the higher risk of unemployment. These wage premiums offset the benefits of debt financing, resulting in a negative relation between debt ratios and workers' unemployment risk. Thus, if higher firing costs reduce the risk that employees will be discharged, higher firing costs could decrease unemployment risk, leading to lower wage premiums and higher financial leverage ratios.

On the other hand, higher firing costs could lead to lower optimal debt ratios by increasing a firm's financial losses in the event of distress as well as its risk of becoming distressed. Under the traditional trade-off theory of capital structure, firms weigh the benefits from tax shields against financial distress costs (e.g., Kraus and Litzenberger (1973), Scott (1976), Bradley, Jarrell, and Kim (1984), Graham (2003)). Several studies suggest that distressed firms often discharge workers to meet outstanding debt obligations by reducing costs (e.g., John, Lang, and Netter (1992), Ofek (1993), Kang and Shivdasani (1997), Denis and Kruse (2000)). Thus, when firms incur the firing costs associated with discharging workers, these additional costs increase the losses associated with financial distress. Consequently, if firing costs increase, this effect should directly offset the tax benefits of debt and result in lower optimal financial leverage ratios.

In addition, if it is costlier to dismiss employees, firms are less likely to adjust their labor force in response to prevailing economic conditions, for instance, by discharging workers during economic downturns. This result has been documented in international settings. For example, Messina and Vallanti (2007) show that more stringent European firing laws significantly weaken the

relation between layoffs and the business cycle, and Kugler and Pica (2008) find that higher dismissal costs in Italy reduce worker separation rates.⁶ This lower sensitivity of workforce reductions to economic downturns makes labor costs more fixed in nature, resulting in greater operating leverage. Thus, higher firing costs can also reduce optimal financial leverage ratios by increasing a firm's likelihood of becoming financially distressed through higher operating leverage (e.g., Van Horne (1974), Mandelker and Rhee (1984), Mauer and Triantis (1994), Kahl, Lunn, and Nilsson (2014)).

II. Wrongful Discharge Laws

A. Institutional Background

To help protect employees from unfair dismissal practices, many states began recognizing exceptions to the terminate at-will rule in the 1970s typically known as WDLs. These WDLs matured into three common law exceptions. Some states recognize all three exceptions, while others recognize two, one, or none at all. Unlike federal laws that aim to protect a particular class of workers, such as union members, racial minorities, women, or the aged, these exceptions pertain to workers not already covered by federal legislation or by explicit contractual agreements (Miles (2000)).

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. This exception requires employers to treat workers in a fair manner (i.e., in good faith) and not take actions that would deprive employees of the benefit of employment without just cause. For example, employers cannot discharge workers out of bad faith, malice, or retaliation. Employers also violate this law if they discharge workers before pensions vest or employees receive bonuses or commissions that they are entitled to. However, in its broadest sense, this exception has been interpreted to imply that dismissal decisions are subject to a "just cause" standard, which can vastly expand the set of situations under which employees can sue.⁷

The implied contract exception protects workers from termination when an employer has implicitly promised employees that they will not be discharged without good cause. These promises may be oral; if written in a handbook, they do not need to be negotiated with employees

individually. Courts 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.

Last, the public policy exception protects employees from termination for refusing to violate an established public policy or commit an illegal act, such as reporting an employer's wrongdoing, refusing to commit perjury, filing a worker's compensation claim, or performing jury duty. The underlying motivation behind the public policy exception is to protect employees from being discharged for performing a public service even if the action is not in the employer's interest.

Based on findings in prior work, it is possible that firms will adjust leverage ratios following the adoption of any (or all) of the WDLs. However, the good faith exception is arguably the most far-reaching of the three WDLs and hence likely to have the greatest impact on corporate outcomes, for several reasons. First, this exception represents the largest deviation from at-will employment because it can imply that termination must always be for just cause (e.g., Dertouzos and Karoly (1992), Kugler and Saint-Paul (2004)). Second, unlike the implied contract exception but similar to the public policy exception, employees have both a contract and tort cause of action under the good faith exception. This means that an employee can recover not only contractual losses but also compensation for emotional distress and punitive damages. Importantly, emotional and punitive damages can substantially increase an employer's liability. Moreover, because punitive and emotional damages are determined by a jury without a clear formula, there is greater uncertainty associated with settlement amounts. Third, Miles (2000) and Autor, Kerr, and Kugler (2007) suggest that firms can largely prevent lawsuits arising from the implied contract exception by including disclaimers in their personnel manuals and employees' handbooks stating that employment contracts are always at-will. Last, Autor, Kerr, and Kugler (2007) also note that the public policy exception generally does not impose substantial constraints on employer behavior because courts typically limit recovery to dismissals in which the employer violated or encouraged the violation of an identifiable statute or constitutional provision. Given that I find that the adoption of only the good faith exception affects leverage ratios, I focus my analyses and discussions on this exception and treat the adoption of the implied contract and public policy exceptions as additional control

variables.

B. Workforce Reductions, EPLI, and Wrongful Termination Lawsuits?

In assuming that the adoption of WDLs increases employee firing costs, two questions arise. First, can workforce reductions lead to wrongful termination lawsuits? And second, can firms insure against wrongful termination claims?

Under ordinary circumstances, economically motivated workforce reductions do not give rise to a claim of wrongful termination (e.g., Strong (1989)). Employers could be subject to wrongful termination claims, however, if they use layoffs as a pretext to fire certain employees for reasons unrelated to the economic necessity of the dismissals (see Appendix A for a discussion of two relevant cases). Thus, while layoffs may reduce the risk of wrongful termination lawsuits, they do not eliminate this risk.

A claim of wrongful termination under the good faith exception can also arise in the context of plant closings (Rhine (1986)). In general, a claim arises when employers, who have an informational and economical advantage over employees, harm employee welfare by concealing information about closings. For example, if an employer knows that it will close despite increased employee effort or productivity, any false assurances of job security could be deemed in violation of the good faith exception.⁸

Today, firms can purchase Employment Practices Liability Insurance (EPLI) to partially or completely offset losses related to wrongful termination claims (see Appendix B for a more in-depth discussion of EPLI). Over the period 1967 to 1995, however, the majority of sample firms were severely limited in their ability to insure against such claims because the market for EPLI did not start developing until the early 1990s, and was still in early stages of development by the mid-1990s. Further, the nearly universal consensus of firms and brokers was that early EPLI coverage was poor and pricing too high. Policies also contained exclusions related to downsizing, retaliation, and punitive damages.⁹ Moreover, prior to the introduction of EPLI, firms were rarely, if ever, able to recover wrongful termination damages under traditional forms of insurance, such as commercial

general liability insurance and directors and officers liability insurance. Nevertheless, to the extent that insurance coverage reduces the risks and costs associated with employee litigation, the presence of insurance should only reduce the effect of the good faith exception on capital structures.¹⁰

C. The Passage of Wrongful Discharge Laws by State Courts

To identify precedent-setting court cases signaling that a state has passed a particular WDL, I largely follow Autor, Donohue, and Schwab (2006), who search for the first major appellate court decision that indicates the sustained adoption of a particular employment at-will exception. If a lower court's decision to adopt an exception is reversed on appeal, this state is not coded as passing the exception, whereas a state Supreme Court decision or a lower court decision that is not later reversed is coded as the passage of the exception. This coding is done separately for the good faith, implied contract, and public policy exceptions. However, in contrast to Autor, Donohue, and Schwab (2006), I code Utah as recognizing the good faith exception in 1989, as done by Walsh and Schwarz (1996) and Littler (2009). I next match these laws to the state in which each firm is headquartered (see, for example, Matsa (2010), Agrawal and Matsa (2013), Acharya, Baghai, and Subramanian (2014), Dougal, Parsons, and Titman (2015)).^{11,12}

For each exception type, Table I summarizes the dates when exceptions were passed by state, and Figure 1 shows the number of states that had passed exceptions by year. The table and figure show that there is substantial variation in the passage of all three types of exceptions both across states and over time. The majority of states adopted WDLs between the early 1970s and the early 1990s. For example, while one, two, and five states had respectively passed good faith, implied contract, and public policy exceptions as of 1975, by 1995 11, 41, and 43 states had passed these three types of exceptions.

[Table I and Figure 1 about here]

D. Endogeneity and Predictability of the Adoption of the Good Faith Exception

My identification strategy assumes that absent the passage of the good faith exception, the financial leverage ratios of firms headquartered in states that do and do not adopt this law would

have evolved in the same way. This parallel trends assumption would be violated if the law's adoption systematically coincides with state-level political or economic factors and the regression models do not adequately control for these factors. In the following subsections, I discuss the extent to which the adoption of the good faith exception is a function of political and economic characteristics as well as the extent to which its adoption is predictable by firms and market participants.

D.1. Endogeneity of the Adoption of the Good Faith Exception

Two common sources of endogeneity could affect interpretation of the results. First, lobbying activities may influence courts' decisions to recognize stronger employment laws. This concern is unlikely a large problem because, unlike other corporate laws such as business combination laws, the enactment of WDLs is based on judicial rather than legislative decisions. Because judicial decisions are more likely driven by the merits of the case than political economy considerations, lobbying activities are not likely to influence the enactment of these laws (Autor (2003), Acharya, Baghai, and Subramanian (2014)).

Second, changes in financial leverage and the adoption of the good faith exception could be spuriously correlated with underlying economic and political conditions. Walsh and Schwarz (1996) analyze published court decisions and find that judges cite the following reasons for adopting WDLs: (1) enhancing fairness in employment relationships, (2) assuring consistency with established principles of contract law, and (3) following other states that have already passed WDLs. These reasons do not appear to be related to factors that could lead firms to change their leverage. Moreover, these rationales imply that judges were not adopting the good faith exception with the intention of altering firms' financial policies or profitability. These findings therefore provide initial evidence that the adoption of this law likely represents an exogenous shock with respect to capital structure decisions and that any effect of its adoption on leverage ratios is an unintended consequence.

Nevertheless, it is possible that judges are directly or indirectly motivated by factors that

they do not cite. Accordingly, I also explore whether several state-level variables that have been hypothesized to impact a court's decision to adopt WDLs are correlated with the passage of the good faith exception. This set of economic variables draws largely on the discussion from Dertouzos and Karoly (1992).

Many states adopted the good faith exception in the 1980s. The adoption of this law could have coincided with several economic features. First, the fraction of unionized workers protected by collective bargaining agreements declined over time. Because this law protects nonunionized workers, the decline in union protection could have increased demand for its adoption. Second, the recession in the 1980s coincided with high unemployment rates, and courts may have been more likely to recognize the good faith exception when the unemployment rate in their state was higher, when a larger fraction of workers would benefit from protection. Third, ideological positions and attitudes towards labor could be a contributing factor.

To test these predictions, I follow Acharya, Baghai, and Subramanian (2014) and estimate a Cox proportional hazard model, where a failure event represents the adoption of the good faith exception. Table II presents the results. The sample spans the period 1967 to 1995, and states are excluded from the sample after they adopt this law. All predictor variables are measured as of year $t-1$ relative to the law's adoption. Also, all independent variables, except indicator variables, are standardized to have a mean of zero and a standard deviation of one. Column 1 includes the following state-level explanatory variables, which are calculated each year: the fraction of unionized workers, the unemployment rate, GDP per capita, the fraction of a state's Congress members in the U.S. House of Representatives that belong to the Democratic Party, an indicator variable for whether the state has right-to-work laws, and indicator variables for whether the state has already adopted the implied contract and public policy exceptions.¹³ Column 2 further includes the change in the unionization rate, the change in the unemployment rate, and the percentage change in the state's GDP. Both columns show that only GDP per capita is positively related to the adoption of the good faith exception.

[Table II about here]

Dertouzos and Karoly (1992) highlight potential regional patterns in the adoption of WDLs and suggest that a court's decision to adopt WDLs could be influenced by whether neighboring states have already enacted these laws. Bird and Smythe (2008) show that this spillover effect is driven by whether other states in the same federal circuit region have already adopted WDLs. To test for such an effect, column 3 also includes the fraction of states in the firm's federal circuit region that have already passed good faith, implied contact, and public policy exceptions. The results show a positive correlation between the adoption of the good faith exception and the fraction of states in the same federal circuit region that have already adopted this law.

Column 4 shows that the adoption of the good faith exception is unrelated to pre-existing state-level financial leverage (measured as the sales-based weighted average of book leverage per state-year), which provides initial evidence ruling out reverse causality.¹⁴ Last, column 5 adds year dummy variables. Both columns show that, after controlling for state-level leverage, GDP per capita no longer predicts the adoption of the good faith exception. However, the adoption of this law remains significantly correlated with the fraction of states in the same federal circuit region that have already adopted the law.

Overall, Table II shows that the adoption of the good faith exception is correlated with only GDP per capita and the fraction of other states in the firm's federal circuit region that have already adopted this law. These findings are consistent with the notion that political and economic factors do not significantly influence whether judges adopt this law.

D.2. Predictability of the Adoption of the Good Faith Exception

There is little empirical evidence on whether the adoption of the good faith exception was anticipated. Given that the adoption of this law relates to two economic characteristics, firms and investors likely had some idea of whether a given state would eventually enact the law. However, as noted by Autor (2003, p.16), "because a court's issuance of a new precedent is an idiosyncratic function of its docket and the disposition of its justices, the timing of a change to the common law is likely to be in part unanticipated." In Section IV.B.1, I provide evidence in support of this conclusion by showing that debt ratios do not decline until after the passage of the law.

I next test whether capital markets fully anticipated the adoption of the good faith exception

by examining stock price reactions around the dates when courts ruled on precedent-setting cases.¹⁵ Because all firms in a state are subject to the same announcement date, events are not independent and standard errors are biased due to correlation across firms in the same state. To address this issue, I create state-level portfolios and analyze announcement returns for the 12 states that have adopted this law (MacKinlay (1997)).¹⁶ I construct daily portfolio returns by equally weighting the daily stock returns of firms headquartered in a given state. I then calculate cumulative abnormal returns (CARs) using either the market model (equally weighted CRSP market portfolio) or the Fama and French (1993) three-factor model. Parameters are estimated over trading days [-280,-61] relative to the announcement date ($t=0$).

Table III presents the results. I find that, in the pre-announcement periods (days [-21,-4] and [-3,-2]), CARs are negative and statistically insignificant. However, over the event window (days [-1,3]), CARs are negative and statistically significant, ranging from -1.05% to -1.22%. Across the portfolios, the average firm's market capitalization is \$406 million on day $t=-20$. Thus, the average firm loses \$4.3 to \$5.0 million when its state adopts the good faith exception. This result could be interpreted as the amount investors expect this law to cost the average firm. Overall, these findings suggest that the adoption of this law was at least partially unanticipated and had an economically meaningful effect on firm value.¹⁷

[Table III about here]

III. Data and Empirical Methodology

A. Sample Selection

The main sample includes 88,997 firm-years for industrial firms (utilities and financial firms are excluded) that have publicly traded stock over the 1967 to 1995 period, are incorporated in the U.S., and have nonmissing data for the main variables of interest. The sample period starts five years before the second-earliest enactment of a WDL when California passed the implied contract exception in 1972. Data limitations prevent the sample from encompassing the first event, California's passage of the public policy exception in 1959.¹⁸ The sample period ends five years after Ohio passed the public policy exception in 1990. I select this year as my cutoff rather than extending

the analysis to cover the last event (Louisiana's passage of the good faith exception in 1998) because few additional observations enter the treatment group when Delaware, Louisiana, Mississippi, and Wyoming pass a WDL (1.19% of firms are headquartered in these states during these later years), and using the extended sample period may create noise around identification of the effect that the adoption of these laws has on capital structures.¹⁹

B. Empirical Methodology

I adopt a difference-in-differences research design to examine the relation between the passage of the good faith exception and financial leverage at the firm-year level. Specifically, I estimate the following panel regression model:

$$Debt_{ist} = \alpha_1 GF_{st} + \alpha_2 IC_{st} + \alpha_3 PP_{st} + X_{ist}\beta + \nu_i + \omega_t + \varepsilon_{ist}, \quad (1)$$

where $Debt_{ist}$ is a specific measure of financial leverage at firm i in state s and year t , and GF_{st} , IC_{st} , and PP_{st} are indicator variables for whether the state in which a firm is headquartered has adopted the good faith, implied contract, and public policy exceptions as of year t , respectively. The regression model also includes a set of control variables X_{ist} , firm fixed effects ν_i , and year fixed effects ω_t . The firm fixed effects control for time-invariant omitted firm characteristics and ensure that estimates of α_1 reflect average within-firm changes in leverage over time rather than simple cross-sectional correlations. The year fixed effects account for transitory nation-wide factors such as macroeconomic conditions that could affect debt ratios and the likelihood that a state adopts the good faith exception.

The control variables include variables commonly found in leverage regressions (see, for example, Harris and Raviv (1991), Rajan and Zingales (1995), Frank and Goyal (2008), Lemmon, Roberts, and Zender (2008)). These variables include log assets (a control for firm size), the market-to-book ratio (a proxy for growth opportunities), profitability (a proxy for the availability of internal funds), the proportion of assets that are fixed (a proxy for potential collateral), and an indicator variable for whether the firm paid a common dividend (a proxy for financial constraints). Following recent work examining the effect of labor market frictions on financial leverage (e.g., Matsa (2010), Agrawal and Matsa (2013)), I also include the modified Altman's z-score to control for a firm's

probability of going bankrupt (MacKie-Mason (1990)). Last, to control for local political and economic conditions, I include state-level GDP per capita, the fraction of other states in the same federal circuit region that have passed the good faith exception as of year t , the state-level one-year GDP growth rate, and the fraction of a state's Congress members representing their state in the U.S. House of Representatives that belong to the Democratic Party in a given year.

I correct estimated standard errors in all regressions for heteroskedasticity and clustering at the state level. Given that the variation in the good faith exception is at the state level, this clustering method accounts for potential time-varying correlations in unobserved factors that affect different firms within a given state (Bertrand, Duflo, and Mullainathan (2004)). This methodology also corrects for within-firm error term correlations over time and is therefore more general than firm-level clustering.

Appendix C provides detailed variable definitions. Panel A of Table IV presents summary statistics. Continuous variables, except economic variables, are winsorized at their 1st and 99th percentiles, and dollar values are expressed in 2009 dollars. For this sample, the average ratio of debt to the book value of assets is 24.5%.

[Table IV about here]

Panel B of Table IV compares variable means for firms headquartered in states that eventually adopt the good faith exception against those of firms headquartered in states that do not adopt this law during the sample period. All the variables, except for political balance, are significantly different across the two samples. Ideally, the treatment and control firms would be relatively similar along these dimensions. However, because they are not, in a first pass at controlling for these differences, I include each variable as a control variable. In Section IV.B.2, I address this issue more directly by performing propensity score matching and interacting each control variable with a treatment dummy, which allows the effect of the variables to be different for treatment and control firms.

IV. Empirical Results

A. Wrongful Discharge Laws and Financial Leverage

I create two measures of financial leverage. I use *Book Leverage* (total debt divided by book value of assets) as my primary measure because many managers focus on book leverage rather than market leverage when making capital structure decisions (Graham and Harvey (2002)). Further, Welch (2004) shows that a substantial portion of the variation in market leverage ratios stems from variation in market values rather than changes in debt policies. Nevertheless, in all of the analyses that follow, I also report results using *Market Leverage* (the book value of long-term debt plus debt in current liabilities divided by market value of assets) because this measure is more closely tied to theoretical predictions related to target leverage levels. All of the results are robust to using either measure of leverage as the dependent variable.

Figure 2 presents a graphical analysis of the relation between the adoption of the good faith exception and financial leverage. To create this figure, I follow Acharya, Baghai, and Subramanian (2014) and regress book leverage (or market leverage) on year fixed effects and dummy variables indicating the year relative to the law's adoption, for up to 10 years before and after its adoption. The last variable is set to one if 10 or more years has passed since the adoption of this law. The y-axis plots the estimated coefficients on each indicator variable (Panel A for book leverage and Panel B for market leverage). The x-axis shows the time relative to the adoption of the good faith exception for ± 5 years around its adoption. The dashed lines correspond to the 90% confidence intervals of the coefficient estimates, where confidence intervals are calculated from standard errors clustered by state. Both graphs show that leverage is not statistically different between treated and control firms before the adoption of this law. However, in the years after its adoption, book and market leverage are significantly lower for treated firms.

[Figure 2 about here]

Panel A of Table V presents the results of the analysis examining the effect of the adoption of the good faith exception on book leverage in a multivariate framework. Column 1 includes only year and firm fixed effects as control variables and shows a negative but statistically insignificant relation between its adoption and book leverage. This insignificant relation could be due in part to

changes in other known predictors of leverage following the adoption of the good faith exception. For example, I find that size and asset tangibility increase following the law's adoption, which could be related to firms engaging in capital deepening (Autor, Kerr, and Kugler (2007)). Profitability also declines slightly, but this effect is statistically insignificant. Because size and asset tangibility are positively correlated with leverage, excluding these variables from the regression potentially offsets part of the effect of the adoption of the good faith exception on debt ratios. Consistent with this notion, after controlling for size, profitability, and asset tangibility in column 2, the negative relation between its adoption and leverage becomes statistically significant. The estimated coefficients imply that book leverage declines by 1.1 percentage points following the enactment of this law. Relative to the sample mean of book leverage of 24.5%, this finding represents a reduction in leverage of 4.5% ($=0.011/0.245$).

[Table V about here]

Column 3 further controls for the market-to-book ratio, and column 4 controls for the modified z-score and whether a firm pays dividends.²⁰ Adding these controls increases both the economic and statistical significance of the effect of the passage of the good faith exception. Last, column 5 controls for the four political and economic variables. Including these variables does not affect the economic significance of the effect of the law's adoption. Book leverage declines by 1.5 percentage points or 6.1% relative to its sample mean ($=0.015/0.245$). While controlling for these variables weakens the statistical significance slightly, its effect remains significant at the 5% level. This last model with the full set of control variables is my main specification, which I use throughout the rest of the paper.

Panel B of Table V shows a similar relationship between the adoption of the good faith exception and market leverage. The effect of its adoption becomes negative and statistically significant only after controlling for size, profitability, and asset tangibility. In terms of economic significance, the estimated coefficients in column 2 imply that market leverage declines by 0.9 percentage points or 3.3% relative to its sample mean of 27.5%. Controlling for growth opportunities, dividend payments, and financial distress increases the economic and statistical significance of the effect of the law's adoption. Column 4 shows that market leverage declines by 1.3

percentage points.²¹ Last, controlling for political and economic factors in column 5 reduces the economic significance of the effect of the passage of this law. Market leverage declines by 1.0 percentage points or 3.6% relative to its sample mean. However, this result remains statistically significant at the 1% level.

To establish whether the estimates from Table V are reasonable, I follow an approach similar to Agrawal and Matsa (2013) and calculate the average tax shield loss due to reducing leverage following the adoption of the good faith exception. The present value of the tax shield can be estimated as the value of debt times the marginal tax rate. Assuming that the average firm has a marginal tax rate of 30%, the present value of the incremental loss in the value of the tax shield from permanently decreasing book leverage by 1.5 percentage points following the law's adoption (column 5 of Panel A) is 0.45% of the firm's asset value ($=1.5\% \times 30\%$). Given that the average firm's book value of assets is \$953 million, this reduction in the tax shield amounts to \$4.29 million ($=0.45\% \times \953 million).

If the average wrongful termination award is \$1.29 million (Jung (1997)), this 1.5 percentage point reduction in leverage would have to prevent at least 3.3 lawsuits over the life of the firm ($=\$4.29/\1.29). However, for this value to be meaningful, it needs to be compared to how many wrongful termination claims a firm could face over its lifetime. Unfortunately, deriving such estimates is difficult because the decisions in many wrongful termination cases are never published and are often settled before trial. Therefore, I start with the estimate from Westin and Feliu (1988) that there are 20,000 wrongful termination cases pending in state courts at any point in time. Because WDLs apply to full-time, nonunionized workers, I obtain U.S. employment data from the Bureau of Labor Statistics for 1988 and estimate the number of workers protected by WDLs as 78.2 million. This value equals the total number of nonfarm full-time employees (about 95.2 million) less the number of unionized workers (about 17.0 million). These figures imply that, on average, approximately one out of every 3,910 employees will file a wrongful termination claim in state courts. In my sample, the mean firm has 6,167 employees and could therefore expect 1.56 wrongful termination lawsuits per year. I use the number of years firms in my sample trade as public corporations to proxy for the number of years they could expect to operate. This figure is 14.6 years

based on CRSP data. Thus, the average firm could expect 22.8 ($=14.6 \times 1.56$) wrongful termination lawsuits over its lifetime, suggesting that the reduction in leverage following the adoption of the good faith exception would have to reduce the number of lawsuits by 14.5% ($=3.3/22.8$). Overall, these back-of-the-envelope calculations suggest that the estimates from Table V are reasonable.

B. Econometric Concerns and Robustness Tests

I next address econometric concerns and discuss several robustness tests.

B.1. Pre-Treatment Trends

Figure 2 suggests that treatment and control firms share similar pre-treatment trends. Next, I follow Bertrand and Mullainathan (2003) and conduct an additional test to help alleviate potential endogeneity concerns related to reverse causality and provide further support for the conclusion that my experiment likely satisfies the parallel trends assumption. To do so, I examine the timing of financial leverage changes relative to the timing of the passage of the good faith exception. If reverse causality is an issue or pre-treatment trends exist, then there would be a trend of declining (possibly increasing) leverage before the enactment of this law.

Similar to the regression used to create Figure 2, I replace the variable for whether the state in which a firm is headquartered has adopted the good faith exception as of year t with the following indicator variables: $Good\ Faith^{-1}$, $Good\ Faith^0$, $Good\ Faith^1$, and $Good\ Faith^{2+}$. These four variables are set to one if the firm is headquartered in a state that (1) will pass the law next year, (2) passes the law in the current year, (3) passed the law one year ago, and (4) passed the law two or more years ago.²² The results in Table VI imply that there is no trend of declining book or market leverage before the adoption of the good faith exception and that they decline only after the enactment of this law (columns 1 and 3). These results survive after including state-specific time trends that allow each state to have different trends in leverage that could have coincided with the adoption of this law (columns 2 and 4). Overall, the finding that debt ratios decline only after the enactment of the good faith exception suggests that the relation does not suffer from reverse causality and that there are no pre-treatment trends.

[Table VI about here]

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B.2. The Effect of Covariate Balance

I next examine the robustness of the negative relation between leverage and the adoption of the good faith exception to controlling for differences in firm characteristics between treatment and control firms as shown in Panel B of Table IV. First, I create two matched samples by matching treatment firms to control firms using a propensity score methodology. I start by retaining all observations for treatment and control firms in year $t-1$ relative to the adoption of the good faith exception. I require that both sets of firms have at least one observation in the -5 years and at least one observation in the +5 years around the adoption of the law. I then use a logistic regression to estimate the probability of being a treated firm. For the first match, I estimate propensity scores using *Log Assets*, *Profitability*, *Fixed Assets*, and *Market-to-Book*. For the second match, I also include *Modified Z-Score* as a regressor.

For both matches, I match each treatment firm in year $t-1$ to a control firm (with replacement), matching on year, three-digit SIC industry, and closest propensity score (with a max difference between propensity scores of 0.01). When treatment firms have multiple control firm matches, I retain the control firm with the closest propensity score. I keep all observations in the ± 5 years around the adoption of the good faith exception. For the second match, I also match treatment firms to control firms based on whether they pay dividends. Panel A of Table VII shows a successful matching procedure. The sample means of control variables for matched treated and control firms are not significantly different.

[Table VII about here]

Next, I estimate the effect of the adoption of the good faith exception for the matched samples using the model specification from column 5 of Table V that includes the full set of control variables. Columns 1 to 4 of Panel B continue to show a significant decline in financial leverage ratios for treatment firms relative to control firms after the adoption of this law.

As a last robustness test addressing covariate balance, I estimate regression models using the full sample but interact each control variable with an indicator variable for whether the firm is in the treatment group. This test therefore allows the effect of control variables to be different across treatment and control firms. The results, reported in Internet Appendix Table IAI, are robust to this

model specification.²³

B.3. Additional Robustness Tests of the Main Specification

I conduct several additional robustness tests of the main specification. Table VIII shows that the decline in financial leverage following the adoption of the good faith exception is robust to using alternative definitions of leverage, including (1) the natural logarithm of one plus the total value of debt, to address the concern that the observed decline in debt ratios could be due to an increase in assets, (2) net book and market leverage (debt less cash), to account for firms increasing liquid assets when firing costs increase, and (3) adding the value of capitalized operating leases to debt, to account for potential underestimation of a firm's true degree of financial leverage by ignoring operating lease commitments (Eisfeldt and Rampini (2009), Rauh and Sufi (2012)).²⁴

[Table VIII about here]

In Internet Appendix Tables IAI to IAV, I also document that the negative relation between the adoption of the good faith exception and book and market leverage ratios is robust to the following:

- i. Controlling for all the economic and political factors discussed in Section II.D.1 that could be correlated with the adoption of the good faith exception.
- ii. Including year \times two-digit SIC industry fixed effects to control for industry shocks that could have coincided with a decline in leverage and the adoption of this law.
- iii. Including *Log Assets* \times two-digit SIC industry fixed effects to control in part for the extent to which a firm could have out-of-state operations.
- iv. Controlling for average state-level financial leverage (as measured by the sales-based weighted average of book (or market) leverage per state-year).
- v. Using alternative sample periods. In particular, the results are robust to (1) extending the sample period to 2003, which is five years after the last event (i.e., when Louisiana adopted the good faith exception in 1998), and (2) restricting the sample to the period 1978 to 1999, as in Autor,

Donohue, and Schwab (2006).

- vi. Using the dating schemes and precedent-setting cases employed by Autor, Donohue, and Schwab (2006), Dertouzos and Karoly (1992), and Morriss (1995) to account for subjectivity in identifying precedent-setting cases.
- vii. Restricting the sample to firms that are more likely to have geographically concentrated operations and thus firms for which state-level laws better capture an increase in their firing costs. In particular, the results are robust to (1) excluding firms with international operations as indicated by nonmissing and nonzero foreign income or foreign taxes and (2) excluding firms in industries in which a large percentage of the workforce is likely geographically dispersed, which include retail, wholesale, and transportation (Agrawal and Matsa (2013)).
- viii. Attempting to account for relocations of headquarters. Because Compustat provides only the most recent state in which headquarters are located, I use the programming language PHP to collect reported locations from 10-K filings on the SEC Edgar website. I obtain these data for most firms between 1996 and 2011 and for some firms as early as 1992. The results are robust to excluding all firms identified as having relocated to another state. A limitation of this sample restriction, however, is that it only accounts for firms that relocated to a different state over the 1992 to 2011 period. Thus, following an approach similar to that in Amore, Schneider, and Žaldokas (2013), I also eliminate firms that likely switched locations during the earlier years in my sample. The results are robust to excluding firms that had sales or asset growth of more than 100% in any year when 10-K data are unavailable.²⁵

C. Cross-Sectional Tests of the Effect of the Good Faith Exception on Financial Leverage

I next exploit cross-sectional variation in industry characteristics to estimate difference-in-difference-in-differences (DDD) regression models. These tests serve two purposes. First, they provide evidence on which industries and firms are more affected by higher firing costs and hence provide support for the underlying hypothesized mechanisms. Second, it is possible that unobserved trends in leverage or other unobserved factors affect firms headquartered in states that do and do

not adopt the good faith exception differently. By identifying firms within treated states that are more likely affected by the adoption of this law and comparing groups of firms within the same state, the DDD estimator can help alleviate these concerns.

The first set of cross sections is based on industry cash flow volatility and labor turnover rates. Firms operating in industries with more volatile cash flows are more likely to need to adjust employment in response to cash flow fluctuations (e.g., Cuñat and Melitz (2012)). Similarly, firms operating in industries that discharge workers more frequently likely rely on layoffs to make staffing changes and manage costs. Thus, the adoption of the good faith exception should lead to a larger increase in expected firing costs for firms in these industries and hence have a larger effect on their capital structure decisions.

I measure industry cash flow volatility as the average cash flow volatility across all firms in the same three-digit NAICS industry, state, and year. A firm's cash flow volatility is the standard deviation of the ratio of income before extraordinary items plus depreciation and amortization to book assets over the preceding 10 years. Firms must have at least three years of data to enter the industry average calculation. I measure each industry's employee discharge rate in two ways. For the first proxy, I calculate the fraction of firms in each firm's three-digit NAICS industry, state, and year that reduce their number of employees by at least 5% and average this measure over the previous 10 years.²⁶ The second proxy is similar to that in Agrawal and Matsa (2013). For each three-digit NAICS industry, state, and year, I collect data on the total number of employee separations from the Bureau of Labor Statistics' "Mass Layoff Statistics" and the number of total employees from the Bureau of Economic Analysis over the years 1998 to 2006.²⁷ I then average separation rates (number of separations divided by number of employees) for each industry and state over the entire 1998 to 2006 period and use this variable as a single industry measure for the entire sample period. For all three industry measures, I create an indicator variable that is set to one if its value is above the sample median and zero otherwise.

Columns 1 to 3 of Panels A and B in Table IX show that the adoption of the good faith exception has little to no effect on the leverage ratios of firms operating in industries with below-median cash flow volatility or separation rates. For firms with above-median values of each measure,

however, both book and market leverage significantly decline following the adoption of this law.

[Table IX about here]

The second set of cross-sectional tests exploits the fact that WDLs apply to some workers but not others. WDLs do not typically apply to temporary workers and employees who are already covered by a collective bargaining agreement (e.g., Miles (2000), Autor (2003)). Thus, the adoption of the good faith exception should also have greater influence on the capital structures of firms operating in industries with a larger fraction of full-time or nonunionized workers.

I measure the fraction of full-time workers, who are employees that work at least 40 hours a week, for each three-digit NAICS industry, state, and year using data from the IPUMS-CPS database.²⁸ I also use data from the IPUMS-CPS database to calculate the fraction of workers in each three-digit NAICS industry, state, and year that are nonunionized. I consider workers unionized if they are a member of a union or covered by a union but not a member. These data are available beginning in 1990. Thus, I create a single industry measure that is used for the entire sample period by averaging the fraction of nonunionized workers over the years 1990 to 1995. For both industry measures, I create an indicator variable that is set to one if its value is above the sample median and zero otherwise.

Columns 4 and 5 of Panels A and B show that the adoption of the good faith exception does not affect the leverage ratios of firms operating in industries with a below-median fraction of full-time or nonunionized workers. However, for firms with above-median values of each measure, both book and market leverage significantly decline following its adoption.

Panels C and D repeat the analysis in Panels A and B but include the interaction of state and year fixed effects in each model. These models therefore compare the effect of the adoption of the good faith exception across firms with different industry characteristics headquartered in the same state. Effectively, the inclusion of state \times year fixed effects removes all time-varying omitted variables that affect all firms within the same state during a given year by demeaning all variables by state each year. The results are similar to those found in the first two panels and therefore provide further assurance that the finding that the adoption of this law results in lower leverage is robust to controlling for omitted state-level factors.

D. Mechanisms of the Adoption of the Good Faith Exception

The results so far are consistent with higher firing costs influencing capital structures by raising financial distress costs. While I am unable to test how much of this effect is due to higher firing costs increasing a firm's financial losses after it becomes distressed, I next test whether higher firing costs increase the firm's risk of becoming distressed by increasing its operating leverage.

D.1. The Good Faith Exception, Operating Leverage, and Earnings Variability

A firm has greater operating leverage if, for a given percentage change in sales, it has a greater percentage change in earnings. To test whether operating leverage increases following the adoption of the good faith exception, I follow an approach similar to Eisfeldt and Papanikolaou (2013) and estimate the following panel regression model:

$$\Delta \log E_{it} = \alpha_i + \alpha_t + \beta_1 \Delta \log S_{it} + \beta_2 \Delta \log S \times GF_{it} + \beta_3 GF_{it} + \gamma X_{it} + \varepsilon_{it}, \quad (2)$$

where E_{it} is earnings before interest and taxes, S_{it} is firm sales, GF_{it} is an indicator variable set to one if the state has adopted the good faith exception as of year t and zero otherwise, and X_{it} is the same set of control variables as used in the financial leverage regressions. Standard errors are clustered by state.

Column 1 of Table X shows that the sensitivity of changes in earnings to changes in sales is significantly higher after the adoption of the good faith exception. The coefficient estimates imply that before the adoption of this law, a 1% decrease in sales is associated with a 1.37% decrease in earnings. Following its passage, however, a 1% decrease in sales is associated with a 1.58% decrease in earnings, for a relative increase of 15.4% ($=0.206/1.37$). Column 2 shows that this result is robust to controlling for book leverage.

[Table X about here]

Columns 3 to 6 examine the effect of the adoption of the good faith exception on earnings variability. If higher firing costs increase operating leverage, earnings should be more volatile following its passage. Traditional measures of earnings variability, such as cash flow volatility, require using data over several consecutive years and thus have overlapping data. However, it is

problematic to use such measures in the context of my difference-in-differences research design, which measures the average treatment effect from the adoption of this law over time. To overcome this challenge, I first investigate the effect of the adoption of the good faith exception on earnings persistence. This test is motivated by findings in Dichev and Tang (2009) and Frankel and Litov (2009) that firms with more volatile earnings also have less persistent earnings. Therefore, I expect earnings to be less persistent after the law's adoption. To test this prediction, I regress profitability in year $t+1$ on the good faith dummy, contemporaneous profitability, the interaction of the good faith dummy and contemporaneous profitability, and the same set of control variables as used in the main leverage tests. Column 3 shows that after the adoption of this law, earnings are significantly less persistent. Column 4 shows that this finding is also robust to controlling for book leverage.

In a second test, I examine how the passage of the good faith exception affects variation in year-over-year profitability. Similar to the motivation behind the earnings persistence test, if adoption of this law increases earnings variability, it should result in more variation in year-over-year earnings. Columns 5 and 6 support this prediction and show that after enactment of the law, the absolute value of one-year changes in profitability is significantly higher. Overall, the results in Table X are consistent with high firing costs increasing operating leverage.

D.2. The Good Faith Exception and Employment Decisions

I next conduct an indirect test of the operating leverage channel. If operating leverage increases because firms find it more difficult to adjust employment, then I should observe that after the adoption of the good faith exception, firms are less likely to downsize employment following negative cash flow shocks. I should also find lower overall fluctuations in employment.

Table XI presents the results. All dependent and independent variables (except the good faith indicator variable) are standardized to have a standard deviation of one to ease interpretation of coefficient estimates. In columns 1 and 2, I follow Hanka (1998) and define the dependent variable as the percentage decline in the number of firm employees over the previous year, with employment gains (positive percentage changes) set to zero.²⁹ I capture declines in profitability as the change in profitability from year $t-1$ to t . Again, increases in profitability are set to zero.

[Table XI about here]

The positive coefficient of 0.054 on the decline in profitability in column 1 suggests that, before the adoption of the good faith exception, if profitability declines by one standard deviation (about 9.3 percentage points), firms discharge 0.054 of a standard deviation of their workers (about 11.1%). The negative coefficient of -0.032 on the interaction of the good faith dummy and the decline in profitability suggests that adoption of this law more than halves this sensitivity (a 59.3% reduction [$=0.032/0.054$]). Column 2 further controls for book leverage and the interaction of the good faith dummy and book leverage to examine how possible debt reductions affect this relation. The results remain unchanged.

In columns 3 to 5, I follow Autor, Kerr, and Kugler (2007) and test the effect of the adoption of the good faith exception on net employment flows, which is the absolute value of the percentage change in the number of employees. Column 3 shows that employment flows decline by 0.064 standard deviations following the adoption of this law. The sensitivity of net employment flows to the absolute value of the change in profitability is also significantly lower following the adoption of this law (column 4), and this result holds after controlling for book leverage and the interaction of the good faith dummy and book leverage (column 5). Overall, the results in Table XI provide further evidence that is consistent with higher firing costs increasing operating leverage.

V. Alternative Explanations

While my findings are consistent with higher firing costs affecting leverage via increasing financial distress costs, two alternative explanations are possible.

First, the passage of the good faith exception could reduce employment levels. Thus, firms may not need to borrow as much to fund labor expenses, which could also result in lower leverage ratios following the adoption of this law. To explore this possibility, I first check whether employment levels decline for my sample of firms. I regress the natural logarithm of the number of employees on the good faith dummy and the same set of control variables as used in column 5 of Table V. The results show a small decrease in employment of about 1% following the adoption of the good faith exception, but this decrease is not statistically significant (p -value of about 0.50). Thus, this alternative explanation does appear to hold in my sample. As an additional robustness check, I rerun my main leverage tests and include the natural logarithm of the number of employees as a

control variable. The results remain unchanged (see columns 1 to 3 of Table IAVI in the Internet Appendix).

Second, it is possible that firms do not adjust leverage in response to the adoption of the good faith exception, but rather, that the observed decline in leverage is spurious in that it reflects other actions that firms take in response to higher firing costs. For example, firms could employ more temporary workers or shift from relatively more expensive labor inputs to less labor-intensive capital investments to reduce their exposure to litigation and employee firing costs (e.g., Autor (2003), Autor, Kerr, and Kugler (2007)). While true, it is unlikely that these actions result in firms lowering leverage. Indeed, shifting to more flexible temporary workers and increasing capital-intensive assets would increase debt capacity and result in higher financial leverage ratios. Nonetheless, I estimate my main leverage regressions including a proxy for the fraction of a firm's workers who are full-time and the firm's labor-to-asset ratio. The results continue to show that debt ratios decrease following the passage of the good faith exception (see columns 4 to 5 of Table IAVI in the Internet Appendix).

VI. Conclusion

In this paper, I study the relation between employee firing costs and capital structure decisions. To identify the causal effect of firing costs on capital structures, I exploit the adoption of the good faith exception by U.S. state courts. This exception protects workers from termination out of bad faith, malice, or retaliation and raises the costs associated with discharging workers. Using a difference-in-differences research design, I compare changes in the debt ratios of firms headquartered in states that adopt this law to changes in the debt ratios of firms headquartered in states that do not adopt it.

I document a significant decline in both book and market leverage ratios following the adoption of the good faith exception. Results of several robustness tests support a causal interpretation of this finding. I also find that the negative relation between the adoption of this law and financial leverage is stronger for firms operating in industries with a higher likelihood of discharging workers and for firms that employ a larger fraction of workers who are protected by this law.

These findings are consistent with theories predicting that higher firing costs lower optimal financial leverage ratios by increasing a firm's financial losses in the event of distress and, by increasing operating leverage, the likelihood of becoming distressed. I provide evidence of this latter channel by showing that, following the adoption of this law, a firm's degree of operating leverage rises, earnings variability increases, and employment becomes more rigid. In sum, the results emphasize the interdependence of financial policies with labor market frictions and provide insights into how labor regulations and employee firing costs affect capital structure.

Appendix A: Workforce Reductions and Wrongful Termination Lawsuits

While economically motivated layoffs typically do not give rise to wrongful termination claims. In *Coelho v. Posi-Seal International, Inc.*, 208 Conn. 106, 544 A.2d 170, 544 A. 2 (1988) and *Ewers v. Stroh Brewery Company*, 178 Mich. App. 371, 443 N.W.2d 504 (1989), employees were discharged as part of layoffs that the employers claimed were economically motivated. However, the plaintiffs claimed the layoffs were an excuse. In *Posi-Seal*, the plaintiff claimed he was discharged as a result of a dispute with a manager of a different division. In *Ewers*, the plaintiff challenged the economic necessity of the layoff, as there was evidence that Stroh was actually experiencing substantial economic growth and operating at a substantial profit before and after Ewer's discharge.

In both cases, the defendants argued that termination due to a reduction in workforce is, as a matter of law, a just cause. The courts agreed with this premise. However, they concluded that an employer's claim that some employees were terminated as a result of a legitimate reduction in workforce does not necessarily establish that all employees were discharged for the same reason. Further, the courts noted that employers may not use a reduction in workforce as a pretext for discharges that would otherwise be subject to just-cause attack by the employee. In these cases, the courts ruled in favor of the plaintiffs because the evidence indicated that the layoffs were used as a pretext to fire the employees.

Appendix B: Employment Practices Liability Insurance

The following discussion is based largely on Klenk (1999) and Daris and Gassman (2013). In deciding whether to offer EPLI and the premium to charge, insurers consider a number of factors including information on employee turnover and the frequency and dollar volume of prior claims. Insurers also consider the firm's employment practices, such as whether the firm has an employee manual, has someone dedicated to addressing employment-related issues, has a progressive discipline policy, and requires harassment training. Insurers may also require firms' financial statements because their financial condition can be a predictor of future involuntary turnovers or layoffs.³⁰

EPLI largely emerged in the 1990s due to an onslaught of employment litigation following the enactment of several federal and state laws that raised the cost and awareness of employee litigation, such as the Civil Rights Act of 1991, WDLs, the Federal Whistleblower Protection Act of 1989, the Family and Medical Leave Act of 1993, and the Americans with Disabilities Act of 1990. Prior to the introduction of EPLI, firms tried to recover losses under traditional forms of insurance, including commercial general liability insurance and directors and officers liability insurance. However, insurers often denied coverage based on policy terms and conditions. Commercial general liability policies provide coverage for claims arising from accidents and contain provisions stating that accidents cannot be expected or intended from the standpoint of the insured, which essentially barred coverage for wrongful termination practices (Machson and Monteleone (1994)).³¹ Further, insurers often modified these policies to exclude coverage for wrongful termination. Firms also had difficulty recovering claims under directors and officers liability insurance because employment claims are typically asserted against corporate entities and not individuals. Moreover, if individuals are sued, they are not typically directors or officers and therefore not covered.

Early EPLI policies also had a number of problems. Firms and brokers contended that EPLI coverage was poor and pricing too high. Early EPLI policies often contained "intentional acts" exclusions as well as exclusions related to downsizing, retaliation, and punitive damages, which rendered coverage for wrongful termination illusory at times. As a result of these issues, demand for

these early policies was low. However, once the market developed and EPLI coverage expanded, demand increased. For instance, while 22% of surveyed employers had EPLI in 1997, 50 to 60% of surveyed employers carried EPLI in 2012.³²

Appendix C: Variable Definitions

Variable	Description (variable definitions in parentheses refer to Compustat designations where appropriate)
Assets	The value of total book assets (<i>at</i>) in millions.
Book Leverage	The book value of long-term debt (<i>dltt</i>) plus debt in current liabilities (<i>dlc</i>) divided by book value of assets (<i>at</i>).
Circuit States' Good Faith	The fraction of other states in the same federal circuit region as the firm's headquarters state that have passed the good faith exception by year <i>t</i> .
Circuit States' Implied Contract	The fraction of other states in the same federal circuit region as the firm's headquarters state that have passed the implied contract exception by year <i>t</i> .
Circuit States' Public Policy	The fraction of other states in the same federal circuit region as the firm's headquarters state that have passed the public policy exception by year <i>t</i> .
Dividend Payer	An indicator variable set to one if a firm pays a common dividend (<i>dvc</i>) during a fiscal year and zero otherwise.
Fixed Assets	The ratio of property, plant, and equipment (<i>ppent</i>) to book value of assets (<i>at</i>).
Full-Time	The fraction of workers in each three-digit NAICS industry, state, and year that work at least 40 hours per week.
Good Faith	An indicator variable set to one if the state in which a firm is headquartered has adopted the good faith exception by year <i>t</i> and zero otherwise.
Implied Contract	An indicator variable set to one if the state in which a firm is headquartered has adopted the implied contract exception by year <i>t</i> and zero otherwise.
Ind. Volatility	The average cash flow volatility across all firms in the same three-digit NAICS industry, state, and year, where cash flow volatility is the standard deviation of the ratio of income before extraordinary items plus depreciation and amortization to book assets ($(ib+dp)/at$) over the preceding 10 years (firms must have at least three years of data to enter the industry average calculation).
Market-to-Book	The market value of assets (book value of assets (<i>at</i>) plus market value of equity ($prcc_f * csho$) minus book value of equity (<i>ceq</i>)) divided by book value of assets (<i>at</i>).
Market Leverage	The book value of long-term debt (<i>dltt</i>) plus debt in current liabilities (<i>dlc</i>) divided by market value of debt and equity (long-term debt (<i>dltt</i>) plus debt in current liabilities (<i>dlc</i>) plus market value of equity ($prcc_f * csho$)).
Modified Z-Score	The modified Altman's z-score ($1.2 * (wcap/at) + 1.4 * (re/at) + 3.3 * (ebit/at) + (sale/at)$).
Non-Union	The average annual fraction of workers in each three-digit NAICS industry and state that are not unionized. Union data are available beginning in 1990. To create a single industry measure that can be used for the entire sample period, I average

	the fraction of nonunionized workers over the years 1990 to 1995.
Political Balance	The fraction of a state's Congress members in the U.S. House of Representatives that belong to the Democratic Party in a given year.
Profitability	Income before extraordinary items (<i>ib</i>) plus depreciation and amortization (<i>dp</i>) divided by book value of assets (<i>at</i>).
Public Policy	An indicator variable set to one if the state in which a firm is headquartered has adopted the public policy exception by year <i>t</i> and zero otherwise.
Right-to-Work Laws	An indicator variable set one if a firm is headquartered in a state that has passed right-to-work laws by year <i>t</i> and zero otherwise.
Sales-Weighted State Book Leverage	The sales-based weighted average book leverage of all firms headquartered in a state.
Sep. Rate	The fraction of firms in a firm's three-digit NAICS industry, state, and year that reduce their number of employees by at least 5%. This measure is averaged over the previous 10 years to determine the industry's separation rate.
Sep. Rate BLS	The average annual fraction of workers separated from work as part of a mass layoff for each three-digit NAICS industry and state over the years 1998 to 2006.
State GDP Growth	The state-level GDP growth rate over the fiscal year.
State Per Capita GDP	A state's GDP (in thousands) divided by its total population.
State Unemployment Rate	The fraction of workers within a state that are in the labor force but unemployed.
State Union Membership	The fraction of each state's nonagricultural wage and salary employees who are covered by a collective bargaining agreement.

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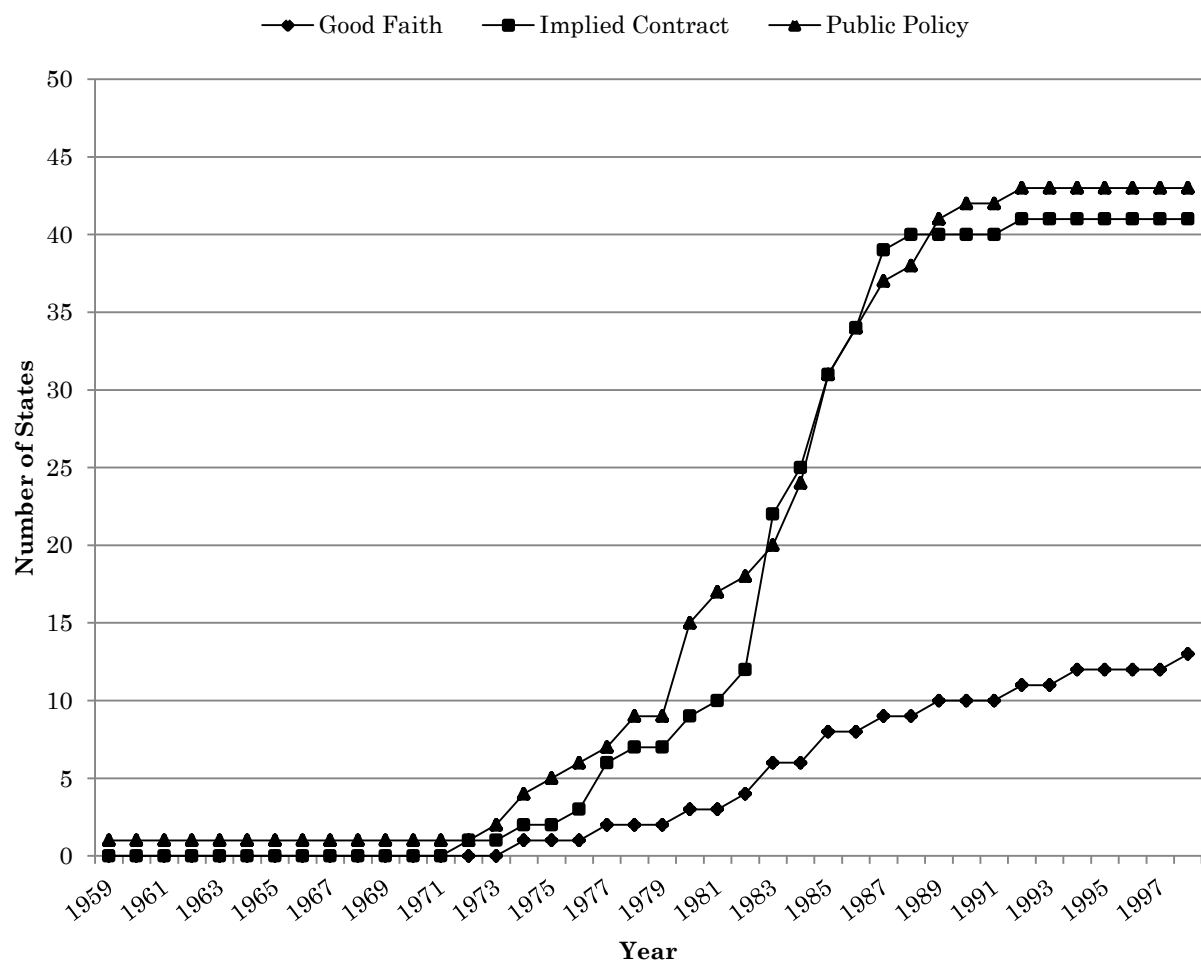


Figure 1. Number of states adopting wrongful discharge laws. This figure shows the number of states that have passed good faith, implied contract, and public policy exceptions to the traditional employment at-will rule in each year between 1959 and 1998.

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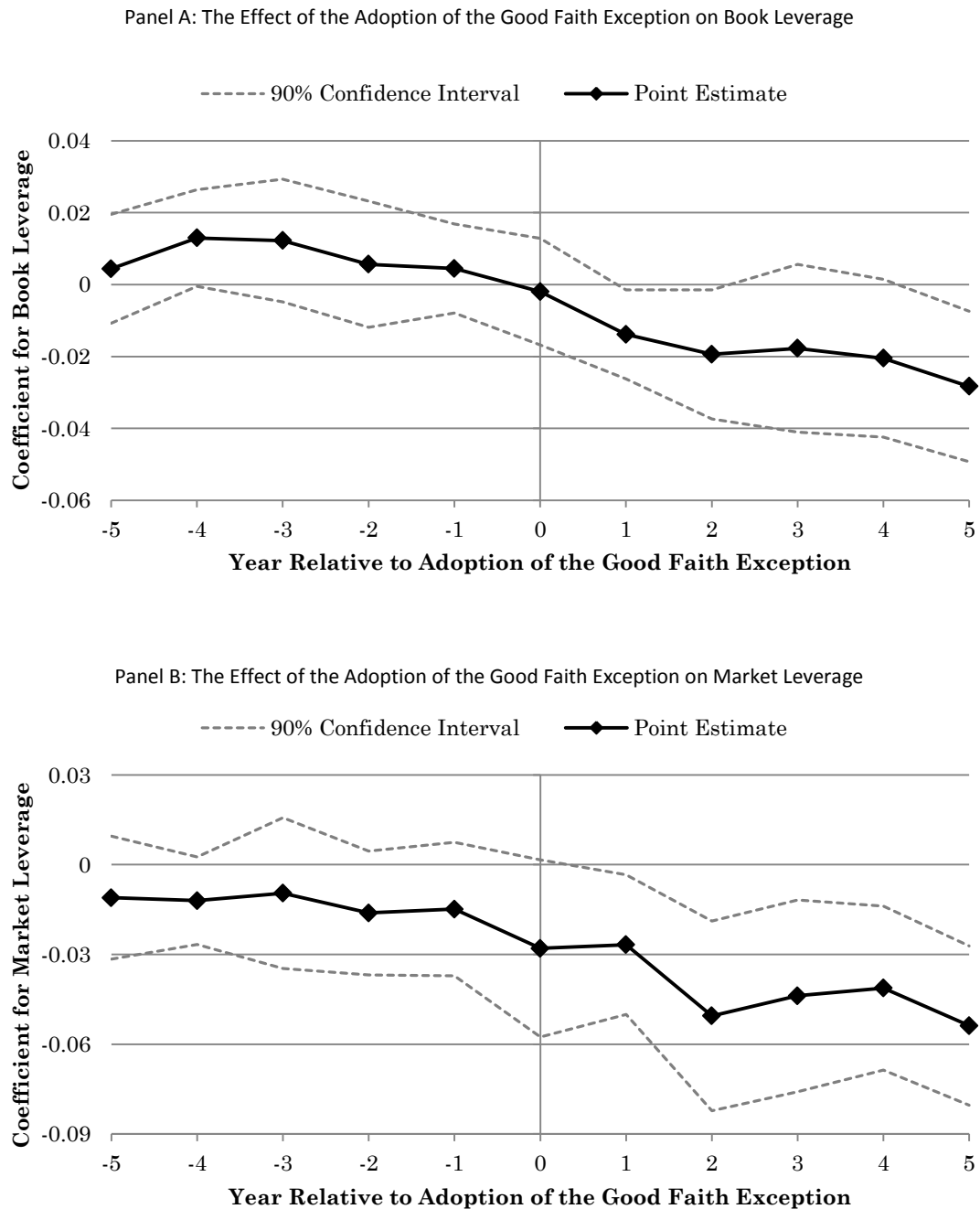


Figure 2. The effect of the adoption of the good faith exception on financial leverage. This figure shows the effect of the adoption of the good faith exception on book leverage (Panel A) and market leverage (Panel B). On the y-axis, the graph plots the coefficient estimates from regressing book leverage (or market leverage) on year fixed effects and dummy variables indicating the year relative to the adoption of the good faith exception. I create dummies for up to 10 years before and after its adoption. The last variable is set to one if it has been 10 or more years after the adoption of this law and zero otherwise. The x-axis shows the time relative to the adoption of the good faith exception. The dashed lines correspond to the 90% confidence intervals of the coefficient estimates. Confidence intervals are calculated from standard errors clustered by state. The sample period is from 1967 to 1995 and consists of 88,404 firm-year observations.

Table I

State-Level Wrongful Discharge Laws

This table reports the month and year when each state passed good faith, implied contract, and public policy exceptions to the traditional employment at-will rule.

State	Month/Year Good Faith Exception Passed	Month/Year Implied Contract Exception Passed	Month/Year Public Policy Exception Passed
Alabama		7/1987	
Alaska	5/1983	5/1983	2/1986
Arizona	6/1985	6/1983 (Reversed 4/1984)	6/1985
Arkansas		6/1984	3/1980
California	10/1980	3/1972	9/1959
Colorado		10/1983	9/1985
Connecticut	6/1980	10/1985	1/1980
Delaware	4/1992		3/1992
Florida			
Georgia			
Hawaii		8/1986	10/1982
Idaho	8/1989	4/1977	4/1977
Illinois		12/1974	12/1978
Indiana		8/1987	5/1973
Iowa		11/1987	7/1985
Kansas		8/1984	6/1981
Kentucky		8/1983	11/1983
Louisiana	1/1998		
Maine		11/1977	
Maryland		1/1985	7/1981
Massachusetts	7/1977	5/1988	5/1980
Michigan		6/1980	6/1976

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Minnesota		4/1983	11/1986
Mississippi		6/1992	7/1987
Missouri		1/1983 (Reversed 2/1988)	11/1985
Montana	1/1982	6/1987	1/1980
Nebraska		11/1983	11/1987
Nevada	2/1987	8/1983	1/1984
New Hampshire	2/1974 (Reversed 5/1980)	8/1988	2/1974
New Jersey		5/1985	7/1980
New Mexico		2/1980	7/1983
New York		11/1982	
North Carolina			5/1985
North Dakota		2/1984	11/1987
Ohio		4/1982	3/1990
Oklahoma	5/1985 (Reversed 2/1989)	12/1976	2/1989
Oregon		3/1978	6/1975
Pennsylvania			3/1974
Rhode Island			
South Carolina		6/1987	11/1985
South Dakota		4/1983	12/1988
Tennessee		11/1981	8/1984
Texas		4/1985	6/1984
Utah	3/1989	5/1986	3/1989
Vermont		8/1985	9/1986
Virginia		9/1983	6/1985
Washington		8/1977	7/1984
West Virginia		4/1986	7/1978
Wisconsin		6/1985	1/1980
Wyoming	1/1994	8/1985	7/1989

Table II

Determinants of Adopting the Good Faith Exception

This table reports results from a Cox proportional hazard model analyzing the hazard of a state court adopting the good faith exception. The sample period is from 1967 to 1995. A “failure event” is the adoption of the good faith exception in a given state. States are excluded from the sample after they adopt this law. Explanatory variables are measured as of year $t-1$. Appendix C provides variable definitions. *Change in State Union Membership* and *Change in State Unemployment Rate* are the one-year changes in *State Union Membership* and *State Unemployment Rate*, respectively. All independent variables, except indicator variables, are standardized to have a mean of zero and a standard deviation of one. Dollar values are expressed in 2009 dollars. Standard errors are clustered at the state level (t -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
State Union Membership	-0.495 (-1.29)	-0.460 (-1.23)	-0.518 (-1.26)	-0.448 (-1.04)	-0.705 (-1.39)
State Unemployment Rate	0.212 (0.89)	0.319 (1.12)	0.314 (1.03)	0.243 (0.75)	0.136 (0.31)
Log(State Per Capita GDP)	0.586*** (3.62)	0.527*** (2.68)	0.466** (2.47)	0.293 (0.94)	0.450 (1.09)
Political Balance	-0.099 (-0.32)	-0.093 (-0.28)	-0.047 (-0.15)	0.011 (0.03)	0.056 (0.17)
Right-to-Work Laws	-0.234 (-0.30)	-0.118 (-0.15)	0.099 (0.12)	-0.001 (-0.00)	-0.157 (-0.21)
Implied Contract	0.341 (0.41)	0.356 (0.50)	-0.103 (-0.12)	-0.164 (-0.20)	-0.073 (-0.09)
Public Policy	-0.370 (-0.39)	-0.306 (-0.34)	-0.452 (-0.43)	-0.325 (-0.30)	-0.097 (-0.09)
Change in State Union Membership		-0.386 (-1.19)	-0.375 (-1.21)	-0.402 (-1.32)	-0.444 (-1.55)
Change in State Unemployment Rate		-0.503 (-1.58)	-0.462 (-1.40)	-0.476 (-1.40)	-0.361 (-0.94)
State GDP Growth		0.093 (0.62)	0.158 (1.01)	0.184 (1.13)	-0.015 (-0.06)
Circuit States’ Good Faith			0.444** (2.08)	0.431** (2.03)	0.378* (1.92)
Circuit States’ Implied Contract			0.063 (0.21)	0.080 (0.24)	0.465 (1.03)
Circuit States’ Public Policy			0.101 (0.24)	0.135 (0.32)	0.653 (1.46)
Sales-Weighted State Book Leverage				0.290 (0.86)	0.256 (0.66)
Year Fixed Effects	No	No	No	No	Yes
Observations	1,299	1,299	1,299	1,251	1,251
Pseudo R ²	0.060	0.094	0.117	0.122	0.264

Table III

CARs around the Adoption of the Good Faith Exception

This table reports results investigating stock price reactions to the announcement that a state court adopts the good faith exception. These dates are the dates when a good faith exception case is ruled on. The sample includes all firms headquartered in one of the adopting states over the 1967 to 1995 period that have nonmissing and positive book assets and sales. Financial and utility firms are excluded. Due to data availability, this sample is restricted to the following 12 adopting states: AZ, CA, CT, DE, ID, MA, MT, NH, NV, OK, UT, and WY. In columns 1 and 2, an observation is an equally weighted state-level portfolio. Cumulative abnormal returns (CARs) are calculated over the pre-event windows (trading days [-21,-4] and [-3,-2]) and event window (trading days [-1,3]), where $t=0$ is the date the court adopts the good faith exception. In column 1, abnormal returns are calculated from the market model using CRSP equally weighted market returns. In column 2, abnormal returns are calculated from the three-factor model, in which firm returns are regressed on value-weighted market returns as well as the returns to zero-investment long-short portfolios formed from small cap stocks minus large cap stocks and high book-to-market stocks minus low book-to-market stocks. The parameters for the market and three-factor models are estimated over the [-280,-61] trading days relative to the announcement date. t -statistics are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Equally Weighted State-Level Portfolios		
(Obs. = 12)		
	Market Model CARs	3-Factor CARs
	(1)	(2)
<i>CAR Window</i>		
[-21,-4]	-1.511%	-1.471%
	(-1.52)	(-1.74)
[-3,-2]	-0.058%	-0.045%
	(-0.14)	(-0.12)
[-1,3]	-1.045%**	-1.219%***
	(-3.01)	(-3.59)

Table IV
Summary Statistics

This table reports summary statistics for the main variables in the regression models. Panel A presents summary statistics for the full sample. Panel B reports univariate results comparing the mean values of variables for treatment (firms headquartered in states that eventually adopt the good faith exception) and control firms (firms headquartered in states that never adopt this law). Standard deviations of each variable are reported in parentheses below the corresponding mean value. In Panel B, *, **, and *** in the column labeled Treatment indicate significance at the 10%, 5%, and 1% levels, respectively, for a t-test of whether the two samples have equal means. The sample consists of Compustat industrial firms (excluding financials and utilities) over the 1967 to 1995 period and includes 88,997 firm-year observations. Continuous variables, except economic variables, are winsorized at their 1st and 99th percentiles, and dollar values are expressed in 2009 dollars. Appendix C provides variable definitions.

Panel A: Summary Statistics for Full Sample					
	Mean	Std. Dev.	P25	Median	P75
<i>Dependent Variables</i>					
Book Leverage	0.245	0.191	0.086	0.225	0.362
Market Leverage	0.275	0.241	0.059	0.222	0.441
<i>Main Explanatory Variable</i>					
Good Faith	0.172	0.378	0.000	0.000	0.000
<i>Control Variables</i>					
Implied Contract	0.525	0.499	0.000	1.000	1.000
Public Policy	0.542	0.498	0.000	1.000	1.000
Assets	953.1	2625.5	35.95	131.3	516.5
Market-to-Book	1.736	1.477	0.956	1.242	1.871
Profitability	0.042	0.184	0.036	0.084	0.125
Fixed Assets	0.323	0.209	0.162	0.281	0.444
Dividend Payer	0.454	0.498	0.000	0.000	1.000
Modified Z-Score	1.900	2.127	1.247	2.261	3.055
State Per Capita GDP	34.82	5.583	30.85	34.58	38.45
Circuit States' Good Faith	0.119	0.022	0.000	0.000	0.125
State GDP Growth	0.079	0.037	0.055	0.077	0.100
Political Balance	0.603	0.168	0.519	0.600	0.674

Table IV – (Continued)

Panel B: Comparing Sample Means for Treatment and Control Firms		
	Treatment Sample (Obs. = 23,271)	Control Sample (Obs. = 65,726)
<i>Dependent Variables</i>		
Book Leverage	0.226*** (0.196)	0.252 (0.190)
Market Leverage	0.240*** (0.237)	0.287 (0.241)
<i>Control Variables</i>		
Implied Contract	0.680*** (0.467)	0.470 (0.499)
Public Policy	0.795*** (0.404)	0.453 (0.498)
Assets	709.6*** (2,203)	1,039 (2,754)
Market-to-Book	1.958*** (1.709)	1.658 (1.378)
Profitability	0.020*** (0.215)	0.049 (0.171)
Fixed Assets	0.297*** (0.209)	0.332 (0.209)
Dividend Payer	0.338*** (0.473)	0.495 (0.500)
Modified Z-Score	1.556*** (2.356)	2.022 (2.025)
State Per Capita GDP	37.23*** (5.938)	33.97 (5.190)

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Circuit States' Good Faith	0.228*** (0.263)	0.080 (0.184)
State GDP Growth	0.081*** (0.040)	0.078 (0.036)
Political Balance	0.603 (0.199)	0.603 (0.155)

Table V

The Good Faith Exception and Financial Leverage

This table reports results from OLS regressions relating financial leverage to the enactment of the good faith exception for Compustat industrial firms from 1967 to 1995. The dependent variables in Panels A and B are *Book Leverage* and *Market Leverage*, respectively. *Good Faith* is an indicator variable set to one if the state in which a firm is headquartered has passed the good faith exception by year t and zero otherwise. Appendix C provides variable definitions. Continuous variables are winsorized at their 1st and 99th percentiles, and dollar values are expressed in 2009 dollars. Standard errors are clustered at the state level (t -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. The critical t -values (two-tailed with 49 degrees of freedom) for significance at the 10%, 5%, and 1% level are 1.68, 2.01, and 2.68, respectively.

Panel A: Dependent Variable is Book Leverage					
	Book Leverage				
	(1)	(2)	(3)	(4)	(5)
Good Faith	-0.006 (-1.18)	-0.011** (-2.19)	-0.012** (-2.31)	-0.015*** (-2.80)	-0.015** (-2.56)
Implied Contract	-0.003 (-0.89)	-0.003 (-1.09)	-0.004 (-1.18)	-0.004 (-1.56)	-0.004 (-1.57)
Public Policy	0.002 (0.60)	0.004 (1.23)	0.004 (1.33)	0.002 (0.80)	0.002 (0.52)
Log Assets		0.035*** (11.64)	0.032*** (10.49)	0.049*** (17.02)	0.048*** (17.05)
Profitability		-0.277*** (-16.31)	-0.276*** (-16.35)	0.054*** (3.55)	0.054*** (3.55)
Fixed Assets		0.262*** (19.25)	0.257*** (18.52)	0.183*** (12.37)	0.183*** (12.43)
Market-to-Book			-0.008*** (-9.15)	-0.008*** (-8.77)	-0.008*** (-8.82)
Dividend Payer				-0.049*** (-14.66)	-0.049*** (-14.67)
Modified Z-Score				-0.051*** (-22.95)	-0.051*** (-23.01)
Log(State Per Capita GDP)					0.016 (0.61)
Circuit States' Good Faith					-0.007 (-0.80)
State GDP Growth					0.024 (1.03)
Political Balance					0.001 (0.10)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	88,997	88,997	88,997	88,997	88,997
Adjusted R ²	0.611	0.660	0.662	0.696	0.696

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Table V – (Continued)

Panel B: Dependent Variable is Market Leverage					
	Market Leverage				
	(1)	(2)	(3)	(4)	(5)
Good Faith	-0.002 (-0.55)	-0.009* (-1.88)	-0.012*** (-3.03)	-0.013*** (-3.44)	-0.010*** (-2.74)
Implied Contract	-0.002 (-0.25)	-0.002 (-0.36)	-0.003 (-0.67)	-0.003 (-0.84)	-0.004 (-1.19)
Public Policy	0.002 (0.33)	0.005 (0.76)	0.006 (1.20)	0.004 (0.86)	0.002 (0.51)
Log Assets		0.053*** (13.95)	0.039*** (10.58)	0.058*** (16.67)	0.058*** (16.91)
Profitability		-0.340*** (-11.26)	-0.333*** (-11.53)	-0.026 (-1.12)	-0.025 (-1.09)
Fixed Assets		0.258*** (20.56)	0.233*** (17.81)	0.165*** (12.04)	0.164*** (11.99)
Market-to-Book			-0.039*** (-14.69)	-0.038*** (-14.48)	-0.038*** (-14.79)
Dividend Payer				-0.077*** (-21.48)	-0.077*** (-21.44)
Modified Z-Score				-0.047*** (-24.68)	-0.047*** (-24.66)
Log(State Per Capita GDP)					-0.006 (-0.28)
Circuit States' Good Faith					-0.013 (-1.21)
State GDP Growth					-0.227*** (-5.26)
Political Balance					-0.001 (-0.09)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	88,997	88,997	88,997	88,997	88,997
Adjusted R ²	0.637	0.682	0.703	0.726	0.727

Table VI

The Good Faith Exception and the Timing of Capital Structure Changes

This table reports results from OLS regressions relating financial leverage to the enactment of the good faith exception for Compustat industrial firms from 1967 to 1995. The dependent variable in columns 1 and 2 is *Book Leverage*. The dependent variable in columns 3 and 4 is *Market Leverage*. *Good Faith*⁻¹ is an indicator variable set to one if a firm is headquartered in a state that will pass this law in one year and zero otherwise. *Good Faith*⁰ is an indicator variable set to one if a firm is headquartered in a state that passes this law in the current year and zero otherwise. *Good Faith*¹ is an indicator variable set to one if a firm is headquartered in a state that passed this law one year ago and zero otherwise. *Good Faith*²⁺ is an indicator variable set to one if a firm is headquartered in a state that passed this law two or more years ago and zero otherwise. Columns 2 and 4 include state-specific time trends. The control variables include *Implied Contract*, *Public Policy*, *Log Assets*, *Market-to-Book*, *Profitability*, *Fixed Assets*, *Dividend Payer*, *Modified Z-Score*, *Log(State Per Capita GDP)*, *Circuit States' Good Faith*, *State GDP Growth*, and *Political Balance*. Appendix C provides variable definitions. Continuous variables are winsorized at their 1st and 99th percentiles, and dollar values are expressed in 2009 dollars. Standard errors are clustered at the state level (t-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Book Leverage		Market Leverage	
	(1)	(2)	(3)	(4)
Good Faith ⁻¹	0.003 (0.64)	0.005 (1.24)	0.006 (0.81)	0.007 (0.90)
Good Faith ⁰	-0.004 (-1.02)	-0.002 (-0.43)	-0.006 (-0.84)	-0.006 (-0.75)
Good Faith ¹	-0.015** (-2.18)	-0.012* (-1.71)	-0.007 (-1.32)	-0.006 (-1.03)
Good Faith ²⁺	-0.018** (-2.64)	-0.015** (-2.27)	-0.010** (-2.12)	-0.010** (-2.27)
State-Time Trends	No	Yes	No	Yes
Control Variables	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	88,404	88,404	88,404	88,404
Adjusted R ²	0.696	0.698	0.727	0.728

Table VII

The Effect of Using Propensity Score Matched Samples

This table explores the impact of the adoption of the good faith exception on firms' financial leverage using propensity score matched samples over the window ± 5 years around the adoption of this law. The treatment and control groups consist of firms headquartered in states that adopt and do not adopt the good faith exception, respectively. For the first match, I estimate propensity scores using *Log Assets*, *Profitability*, *Fixed Assets*, and *Market-to-Book*. For the second match, I also include *Modified Z-Score* as a regressor. For both matches, I match each treatment firm in year $t-1$ to a control firm (with replacement), matching on year, three-digit SIC industry, and closest propensity score (with a max difference between propensity scores of 0.01). For the second match, I also match on *Dividend Payer*. When treatment firms have multiple control firm matches, I retain the control firm with the closest propensity score. Panel A tabulates the means of the matched variables and propensity scores for the treatment and control groups in year $t-1$ (the control variables are not statistically different across groups at the 10% significance level). Panel B presents the results examining the impact of the adoption of the good faith exception on firms' leverage in columns 1 to 4. *Treatment* is an indicator variable set to one if the firm is headquartered in a state that adopts this law. *Post* is an indicator variable set to one in the five years after the adoption of this law (same for matched control firms). I exclude the year in which a state adopts this law from the regressions. Control variables in Panel B include *Implied Contract*, *Public Policy*, *Log Assets*, *Market-to-Book*, *Profitability*, *Fixed Assets*, *Dividend Payer*, *Modified Z-Score*, *Log(State Per Capita GDP)*, *Circuit States' Good Faith*, *State GDP Growth*, and *Political Balance*. Appendix C provides variable definitions. Standard errors in Panel B are clustered at the state level (t -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Comparison of Means across Matched Samples in Year $t-1$				
	Matched Sample 1		Matched Sample 2	
	Treatment Group (Obs. = 596)	Control Group (Obs. = 596)	Treatment Group (Obs. = 567)	Control Group (Obs. = 567)
Propensity Score	0.029	0.029	0.029	0.029
Log Assets	4.931	5.069	4.932	4.983
Profitability	0.073	0.084	0.072	0.080
Fixed Assets	0.328	0.340	0.328	0.330
Market-to-Book	1.487	1.447	1.481	1.430
Dividend Payer			0.506	0.506
Modified Z-Score			2.290	2.330
Panel B: Adoption of the Good Faith Exception and Financial Leverage				
	Matched Sample 1		Matched Sample 2	
	Book Leverage (1)	Market Leverage (2)	Book Leverage (3)	Market Leverage (4)
Treatment \times Post	-0.021** (-2.50)	-0.016** (-2.33)	-0.021*** (-2.82)	-0.018*** (-2.73)
Post	-0.001 (-0.16)	0.001 (0.20)	0.004 (0.42)	0.005 (0.59)
Control Variables	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	10,008	10,008	9,484	9,484
Adjusted R^2	0.763	0.782	0.748	0.770

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Table VIII

Alternative Measures of Financial Leverage

This table reports results from OLS regressions relating alternative measures of financial leverage to the enactment of the good faith exception for Compustat industrial firms from 1967 to 1995. The dependent variables in columns 1 to 5 are as follows: *Log(1+Total Debt)* is the natural logarithm of one plus the book value of long-term debt plus debt in current liabilities and is expressed in 2009 dollars; *Net Book (Market) Leverage* is the book value of long-term debt plus debt in current liabilities less the book value of cash and short-term investments divided by the book value of assets (market value of debt and equity); *Book (Market) Leverage with Leases* is the book value of long-term debt plus debt in current liabilities plus the value of leases ($xrent*10$) divided by the book value of assets plus the value of leases (market value of debt and equity plus the value of leases). In columns 4 and 5, all control variables are adjusted to account for the value of leases by adding the value of leases to the book value of debt and/or assets. The value of leases is obtained by capitalizing annual rental expenses at a 10% discount rate. *Good Faith* is an indicator variable set to one if the state in which a firm is headquartered has adopted the good faith exception by year t and zero otherwise. The control variables include *Implied Contract*, *Public Policy*, *Log Assets*, *Market-to-Book*, *Profitability*, *Fixed Assets*, *Dividend Payer*, *Modified Z-Score*, *Log(State Per Capita GDP)*, *Circuit States' Good Faith*, *State GDP Growth*, and *Political Balance*. Appendix C provides variable definitions. Continuous variables are winsorized at their 1st and 99th percentiles, and dollar values are expressed in 2009 dollars. Standard errors are clustered at the state level (t -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Log(1+ Total Debt)	Net Book Leverage	Net Market Leverage	Book Leverage with Leases	Market Leverage with Leases
	(1)	(2)	(3)	(4)	(5)
Good Faith	-0.102*** (-2.82)	-0.027*** (-4.03)	-0.020*** (-3.52)	-0.013** (-2.27)	-0.008* (-1.94)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	88,997	88,997	88,997	88,997	88,997
Adjusted R ²	0.924	0.751	0.678	0.761	0.783

Table IX

The Effect of Cross-Sectional Variation in Industry Characteristics

This table reports results from OLS regressions relating financial leverage to the enactment of the good faith exception for Compustat industrial firms from 1967 to 1995. The dependent variable in Panels A and C is *Book Leverage*, and the dependent variable in Panels B and D is *Market Leverage*. *Good Faith* is an indicator variable set to one if the state in which a firm is headquartered has adopted the good faith exception by year t and zero otherwise. For the continuous measures *Ind. Volatility*, *Sep. Rate*, *Sep. Rate BLS*, *Full-Time*, and *Non-Union*, I create an indicator variable (*High*) that is set to one if the value of the particular measure is above the sample median and zero otherwise. Each regression includes the interaction term and the main effect of each cross-section, but these variables are not tabulated for brevity. The control variables in Panels A and B include *Implied Contract*, *Public Policy*, *Log Assets*, *Market-to-Book*, *Profitability*, *Fixed Assets*, *Dividend Payer*, *Modified Z-Score*, *Log(State Per Capita GDP)*, *Circuit States' Good Faith*, *State GDP Growth*, and *Political Balance*. The control variables used in Panels C and D are the same as those used in Panels A and B. However, because the models in Panels C and D include state \times year fixed effects, these models exclude state-level variables (i.e., *Good Faith*, *Log(State Per Capita GDP)*, *Circuit States' Good Faith*, *State GDP Growth*, and *Political Balance*). Appendix C provides variable definitions. Continuous variables are winsorized at their 1st and 99th percentiles, and dollar values are expressed in 2009 dollars. Standard errors are clustered at the state level (t -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Baseline Cross-Sectional Effects on Book Leverage					
	Book Leverage				
	(1)	(2)	(3)	(4)	(5)
Good Faith	-0.003 (-0.43)	-0.011* (-1.81)	0.013 (1.26)	-0.010 (-1.40)	-0.003 (-0.36)
Good Faith \times High Ind. Volatility	-0.021*** (-5.57)				
Good Faith \times High Sep. Rate		-0.009** (-2.46)			
Good Faith \times High Sep. Rate BLS			-0.034*** (-2.79)		
Good Faith \times High Full-Time				-0.016*** (-3.35)	
Good Faith \times High Non-Union					-0.025*** (-4.20)
Control Variables	Yes	Yes	Yes	Yes	Yes

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Year and Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	79,101	81,914	51,135	77,653	82,560
Adjusted R ²	0.692	0.692	0.690	0.698	0.696
Panel B: Baseline Cross-Sectional Effects on Market Leverage					
	Market Leverage				
	(1)	(2)	(3)	(4)	(5)
Good Faith	-0.001 (-0.08)	-0.005 (-0.98)	0.014 (1.67)	-0.007 (-1.40)	-0.003 (-0.61)
Good Faith × High Ind. Volatility	-0.018*** (-3.54)				
Good Faith × High Sep. Rate		-0.015*** (-3.52)			
Good Faith × High Sep. Rate BLS			-0.032** (-2.52)		
Good Faith × High Full-Time				-0.012** (-2.07)	
Good Faith × High Non-Union					-0.016** (-2.63)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year and Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	79,101	81,914	51,135	77,653	82,560
Adjusted R ²	0.728	0.726	0.729	0.733	0.729

Table IX – (Continued)

Panel C: Effect of Inclusion of State × Year Fixed Effects on Book Leverage					
	Book Leverage				
	(1)	(2)	(3)	(4)	(5)
Good Faith × High Ind. Volatility	-0.021*** (-5.90)				
Good Faith × High Sep. Rate		-0.007** (-2.35)			
Good Faith × High Sep. Rate BLS			-0.039*** (-3.69)		
Good Faith × High Full-Time				-0.017*** (-2.95)	
Good Faith × High Non-Union					-0.024*** (-4.22)
Control Variables	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	79,101	81,914	51,135	77,653	82,560
Adjusted R ²	0.695	0.694	0.693	0.700	0.700
Panel D: Effect of Inclusion of State × Year Fixed Effects on Market Leverage					
	Market Leverage				
	(1)	(2)	(3)	(4)	(5)
Good Faith × High Ind. Volatility	-0.018*** (-3.02)				
Good Faith × High Sep. Rate		-0.015*** (-4.32)			

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Good Faith × High Sep. Rate BLS			-0.040***		
			(-3.39)		
Good Faith × High Full-Time				-0.012*	
				(-1.69)	
Good Faith × High Non-Union					-0.015***
					(-3.04)
Control Variables	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	79,101	81,914	51,135	77,653	82,560
Adjusted R ²	0.730	0.728	0.764	0.734	0.732

Table X

The Good Faith Exception, Operating Leverage, and Earnings Variability

This table reports results from OLS regressions relating operating leverage and measures of earnings variability to the enactment of the good faith exception for Compustat industrial firms from 1967 to 1995. The dependent variables in columns 1 to 6 are as follows: *Change in Log(EBIT)* is the one-year change in the natural logarithm of earnings before interest and taxes [$\ln(ebit_t) - \ln(ebit_{t-1})$]; *Profitability* is income before extraordinary items (*ib*) plus depreciation and amortization (*dp*) divided by book value of assets (*at*); *abs(Change in Profitability)* is the absolute value of the one-year change in *Profitability*. *Good Faith* is an indicator variable set to one if the state in which a firm is headquartered has passed the good faith exception by year *t* and zero otherwise. $\Delta \text{Log}(\text{Sales})$ is the one-year change in the natural logarithm of total sales [$\ln(sale_t) - \ln(sale_{t-1})$]. *abs(Change in Profitability)* and *Book Leverage* in columns 5 and 6 are standardized to have a standard deviation of one to ease interpretation of the coefficient estimates. Control variables include *Implied Contract*, *Public Policy*, *Log Assets*, *Market-to-Book*, *Profitability*, *Fixed Assets*, *Dividend Payer*, *Modified Z-Score*, *Log(State Per Capita GDP)*, *Circuit States' Good Faith*, *State GDP Growth*, and *Political Balance*. Appendix C provides variable definitions. Continuous variables are winsorized at their 1st and 99th percentiles, and dollar values are expressed in 2009 dollars. Standard errors are clustered at the state level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Change in Log(EBIT) _t		Profitability _{t+1}		abs(Change in Profitability) _{t+1}	
	(1)	(2)	(3)	(4)	(5)	(6)
Good Faith	0.002	0.004	0.003	0.002	0.035**	0.035**
	(0.16)	(0.35)	(1.11)	(0.88)	(2.23)	(2.27)
$\Delta \text{Log}(\text{Sales})$	1.371***	1.353***				
	(44.33)	(44.22)				
Good Faith $\times \Delta \text{Log}(\text{Sales})$	0.206***	0.204***				
	(5.39)	(5.07)				
Profitability			0.282***	0.283***		
			(12.63)	(12.77)		
Good Faith \times Profitability			-0.068***	-0.066***		
			(-3.46)	(-3.33)		
Book Leverage		0.357***		-0.030***		0.003
		(8.26)		(-4.01)		(0.32)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

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Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	60,521	60,521	84,617	84,617	83,074	83,074
Adjusted R ²	0.289	0.291	0.614	0.614	0.546	0.546

Table XI

The Good Faith Exception and Employment

This table reports results from OLS regressions relating employment outcomes to the enactment of the good faith exception for Compustat industrial firms from 1967 to 1995. The dependent variable in columns 1 and 2 is the one-year percentage decline in a firm's number of employees ($emp_t/emp_{t-1} - 1$), with employment gains (positive percentage changes) set to zero. The dependent variable in columns 3 to 5 is $abs(\% \Delta \text{ in } \# \text{ of Employees})$, which is the absolute value of the one-year percentage change in a firm's number of employees [$abs(emp_t/emp_{t-1} - 1)$]. *Decline in Profitability* is the one-year change in *Profitability* [$(ib+dp)/at$], with positive changes in *Profitability* set to zero. $abs(\Delta \text{ Profitability})$ is the absolute value of the one-year change in *Profitability*. *Good Faith* is an indicator variable set to one if the state in which a firm is headquartered has passed the good faith exception by year t and zero otherwise. All dependent and independent variables (except *Good Faith*) are standardized to have a standard deviation of one to ease interpretation of the coefficient estimates. Control variables include *Implied Contract*, *Public Policy*, *Log Assets*, *Market-to-Book*, *Profitability*, *Fixed Assets*, *Dividend Payer*, *Modified Z-Score*, *Log(State Per Capita GDP)*, *Circuit States' Good Faith*, *State GDP Growth*, and *Political Balance*. Appendix C provides variable definitions. Continuous variables are winsorized at their 1st and 99th percentiles, and dollar values are expressed in 2009 dollars. Standard errors are clustered at the state level (t -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	% Decline in		abs(% Δ in # of Employees)		
	# of Employees				
	(1)	(2)	(3)	(4)	(5)
Good Faith	-0.042*	-0.016	-0.064**	-0.043*	-0.039
	(-1.92)	(-0.47)	(-2.48)	(-1.70)	(-1.22)
Decline in Profitability	0.054***	0.055***			
	(3.74)	(3.74)			
Good Faith \times Decline in Profitability	-0.032*	-0.034**			
	(-1.87)	(-2.05)			
abs(Δ Profitability)				0.066***	0.065***
				(6.83)	(6.65)
Good Faith \times abs(Δ Profitability)				-0.046***	-0.045***
				(-3.77)	(-3.68)
Book Leverage		-0.011			0.040***
		(-1.39)			(4.32)
Good Faith \times Book Leverage		-0.021			-0.001
		(-1.03)			(-0.10)

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Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	80,987	80,987	80,987	80,987	80,987
Adjusted R ²	0.242	0.242	0.267	0.268	0.269

¹ Workforce reductions are an especially important cost management tool during economic downturns. For instance, a significant fraction of firms (67% of surveyed U.S. firms and 38% of French, German, and Italian firms, which face substantially more rigid labor markets) cut employment during the recent financial crisis (see “Cost Management: An Aspect of Profit & Cash Optimization,” *Accenture*, January 2, 2011).

² At-will employment refers to a legal environment in which employers are free to terminate any employee for good reason, bad reason, or no reason at all, with or without prior notice, and without risk of legal liability. I discuss the three exceptions to at-will employment in more detail in Section II.A.

³ For example, Lawrence National Security laid off 430 employees in 2008 as the firm dealt with the national recession and a budget deficit. However, 130 of these employees sued the firm, claiming that the budget deficit was a “pretext to get rid of older employees who have higher salaries, larger medical costs, and are closer to collecting their pension.” On May 10, 2013, a jury sided with five employees who were selected to be test cases for the other 125 employees, awarding the plaintiffs a total of \$2.7 million for breach of contract and breach of the implied covenant of good faith and fair dealing. See “Jury Awards \$2.7 M Against Lawrence Livermore Lab for Wrongfully Terminating Long-Time Employees in 2008,” *PRNewswire*, May 13, 2013 and *Andrews, et. al. v. Lawrence Livermore National Security, LLC.*, Case No. RG09453596

⁴ See “U.S. Public Companies’ Perceptions of Risk, and Their Risk Mitigation Strategies,” *Chubb 2012 Public Company Risk Survey*, 2012.

⁵ Specifically, from 2005 to 2010, the number of wrongful termination charges filed increased from 39,102 to 55,019.

⁶ While there is strong international evidence that firms are less likely to discharge workers when firing costs increase, this evidence is weaker for U.S. firms. Autor, Donohue, and Schwab (2006), for instance, find weak evidence that WDLs lower separation rates.

⁷ There is no set definition of what constitutes “just cause,” so this standard can vary according to the facts and circumstances of a specific case (Dilts and Deitsch (1992)). However, as arbitrator in *Enterprise Wire Co.* (46 LA 359, 1966), Carroll R. Daugherty developed a checklist of seven tests to determine whether the employer had a just cause for discipline. Stated succinctly, a firm has a just cause for discipline if the following criteria are satisfied: the employer informed and gave the employee advanced notice of disciplinary consequences, the enforced rule is reasonable, the employer conducted a timely, thorough, and fair investigation of the employee’s conduct, the employer has proof of guilt, the employer has applied its rules and discipline evenly to all employees, and the penalty is fair given the offense.

⁸ Similarly, a violation of the good faith exception could arise if an employer keeps secret its plans to close until an

unreasonably short time before closing, because in doing so the employer gives employees insufficient advance notice to plan for their financial futures. It is worth noting that some of these situations have been greatly reduced by the passage of the Worker Adjustment and Retraining Notification Act (WARN Act) in 1988, which requires that employers give employees at least a 60-day advance notice of plant closings and mass layoffs.

⁹ For example, only one domestic insurance carrier as of January 1996 offered EPLI policies that specifically covered punitive damages in the main policy (Klenk (1999)), which is especially problematic because a large percentage of settlements often consist of punitive damages.

¹⁰ It is worth noting that not all of the costs associated with employee litigation are insurable, such as those resulting from delays or disruptions in production due to bureaucratic discharge procedures, time spent fighting lawsuits, and the loss of reputation as an employer of choice for recruiting and retaining desirable employees.

¹¹ Employment laws typically apply to the state in which an employee is working. An ideal measure that captures the increase in a firm's firing costs following the adoption of the good faith exception would aggregate the number of workers who are protected by this law at each of the firm's locations of operations. However, Compustat, provides only the state of incorporation and headquarters. I also do not have access to detailed plant-level data from the U.S. Census Bureau. Accordingly, I follow an extensive line of research and capture each firm's primary location of operations using its headquarters state. Dertouzos, Holland, and Ebener (1988) also find that plaintiffs in wrongful termination cases tend to hold executive or managerial positions (53%). Because workers in these positions tend to be concentrated at headquarters, using the headquarters state may capture a large portion of the increase in firing costs.

¹² Compustat provides only the most recent headquarters locations. If firms relocate their headquarters to a different state, then these firms would be subject to different employment laws in the earlier periods and the resulting measurement error could bias the results. If a firm is coded as not being located in a state that adopts the good faith exception but was in fact located in such a state, the effect of the adoption of this law on leverage would be reduced, as the firm's leverage changes despite the apparent absence of the adoption of the law. Similarly, if a firm is coded as being located in a state that adopts the good faith exception but was in fact not, the firm would fail to appear responsive to the adoption of the law. Either case, however, would bias the tests in favor of finding no effect of the adoption of the good faith exception on debt ratios.

¹³ Union membership is the fraction of each state's nonagricultural wage and salary employees who are covered by a collective bargaining agreement. Data on state union membership are from Hirsch, Macpherson, and Vroman (2001) and are available online at <http://www.unionstats.com>. The unemployment rate is based on data from the March Current Population Survey (CPS) each year. Specifically, the data come from the Integrated Public Use Microdata Series (IPUMS)-CPS database (King et al. (2010)). The CPS is a monthly U.S. household survey conducted jointly by the U.S. Census Bureau and the Bureau of Labor Statistics. The March survey covers additional topics compared to the surveys conducted in other months and therefore is the most widely used. For missing state-years (early 1970s and late 1960s for a few states), this measure is supplemented with data from the IPUMS-USA database (Ruggles et al. (2010)). The IPUMS-USA database compiles data from the American population censuses every 10 years. I obtain data on state-level GDP from the Bureau of

Economic Analysis and data on Congress members in the House of Representatives from the History, Art & Archives, U.S. House of Representatives. Data on the passage of right-to-work laws come from the Department of Labor and are available at <http://www.dol.gov/whd/state/righttowork.htm>.

¹⁴ The smaller sample size is due to some states (such as Alaska) not having any firms with available Compustat data in the early years. Controlling for state-level market leverage produces similar results.

¹⁵ The sample includes all industrial Compustat firms headquartered in the U.S.

¹⁶ Thirteen states adopted the good faith exception over the 1967 to 1995 period. However, because there are no firms headquartered in Alaska with available Compustat and CRSP data in 1983 when the law was adopted, I am unable to estimate announcement returns for this state.

¹⁷ These findings are consistent with Abraham (2004), who examines stock price reactions to six individual wrongful discharge cases in California and New York. The author finds that cases that increased protection of employees led to negative announcement returns, while cases that affirmed at-will employment led to positive announcement returns.

¹⁸ In particular, the book value of common shareholder equity (Compustat data item CEQ) is not populated until 1962. Also, data on state-level GDP from the Bureau of Economic Analysis are not available until 1963.

¹⁹ The results are robust to using alternative sample periods of 1967 to 2003 and 1978 to 1999 (Section IV.B.3).

²⁰ I include the market-to-book ratio separately to examine how controlling for the mechanical relation between it and market leverage in Panel B impacts the effect of the adoption of the good faith exception on leverage.

²¹ Controlling for the modified z-score in column 4 flips the estimated coefficient on profitability from negative to positive. This effect occurs because a measure of profitability (EBIT/assets) is also a component of the modified z-score. If I remove this component from the z-score's calculation, the estimated coefficient on profitability becomes negative again and the results throughout the paper are robust to using this alternative definition. The results are also robust and the estimated coefficients on control variables are as expected if I follow prior literature (e.g., Rajan and Zingales (1995), Leary and Roberts (2014), Simintzi, Vig, and Volpin (2015)) and control only for size, asset tangibility, the market-to-book ratio, and profitability as firm-level controls.

²² There are two instances in which a state reversed its previous passage of the good faith exception. These reversals are (1) New Hampshire, which reversed the passage of the law in 1980, and (2) Oklahoma, which reversed the passage of the law in 1989. To account for these reversals, I drop all observations for these two states after the date of the reversal, which reduces the sample size from 88,997 observations to 88,404 observations.

²³ The Internet Appendix is available in the online version of this article on the *Journal of Finance* website.

²⁴ In this test, I replace book (market) leverage with debt plus the value of leases divided by book value of assets plus the value of leases (market value of assets plus the value of leases). To calculate the value of leases, I follow Rampini and Viswanathan (2013) and capitalize annual rental expenses at a 10% discount rate for all firms.

²⁵ The finding in Pirinsky and Wang (2006) that mergers and acquisitions drive most headquarters relocations motivates this sample restriction. Because mergers are typically associated with large increases in sales or assets (Almeida, Campello,

and Weisbach (2004)), the exclusion of firms with growth exceeding 100% eliminates firms that may have experienced events that initiated changes in their headquarters locations.

²⁶ I calculate the one-year change in a firm's number of employees using data from Compustat. Since Compustat only provides the aggregate number of employees across a firm's divisions, a decrease in the number of firm employees does not necessarily imply that the firm discharged these employees. For instance, I would observe a decrease in the number of firm employees if the firm sells a division to another firm. If selling a division to another firm does not result in a greater likelihood of discharges or wrongful termination lawsuits, then using Compustat data to calculate employee discharges could be a noisy proxy for the firm's propensity to dismiss workers and the increase in firing costs that the firm faces.

²⁷ Similar to Agrawal and Matsa (2013), I count the number of workers who are separated from their jobs during extended mass layoffs, which is defined by the Bureau of Labor Statistics as occurring when at least 50 initial claims for unemployment insurance are filed against an establishment during a consecutive five-week period and at least 50 workers have been separated from their jobs for more than 30 days. A limitation of this measure is that for a number of states and industries, separations are not reported or not reported in a way that meets the Bureau of Labor Statistics' or state agency's disclosure standards. Such observations are treated as missing data, resulting in the sample shrinking to 51,135 observations.

²⁸ In the CPS database, industry definitions change from 1967 to 1968. For consistency, I use the 1968 fraction of full-time workers for 1967.

²⁹ I calculate the one-year change in each firm's reported number of employees using data from Compustat. This methodology has two empirical limitations. First, I observe only net changes in the number of employees and not gross changes. For example, if a firm discharges 50 employees but subsequently hires 50 employees in the same year, this measure will show no change in the number of employees and hence no employee discharges. Net changes may thus understate the firm's risk of wrongful termination lawsuits. Second, because Compustat only provides the aggregate number of employees across a firm's divisions, I would observe a decrease in the number of firm employees if the firm sells a division to another firm. To address this concern, I rerun the regressions in Table XI including control variables for contemporaneous and lagged changes in a firm's asset base to control for changes in the firm's number of employees due to asset sales. I find quantitatively similar results.

³⁰ In practice, EPLI premiums can vary substantially. For example, based on the responses from 26 EPLI carriers, the typical annual premium for a firm with 5,000 employees was between \$110,000 and \$500,000 in 2005. Based on the responses from five EPLI carriers, the typical annual premium for the same firm ranged from \$80,000 to \$120,000 in 2014. See the 2005 and 2014 Betterley Report, *Betterley Risk Consultants, Inc.*

³¹ Commercial general liability policies also provide coverage for claims alleging bodily injury or property damage, but this coverage does not apply to wrongful termination practices.

³² See "2012 Insurance Coverage Survey Results," *Zywave, Inc.*, 2012, "1997 Employment Litigation Survey," *Society for Human Resource Management*, 1997, and "U.S. Public Companies' Perceptions of Risk, and Their Risk Mitigation Strategies," *Chubb 2012 Public Company Risk Survey*, 2012.