Employer WARN-ing: Mandatory Advance Disclosure of Employment Loss and Corporate Innovation

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Abstract: I use the adoption of state-level Worker Adjustment and Retraining Notification (WARN) laws to study the effect of mandatory disclosure on corporate innovation. WARN laws mandate advance disclosure of employment loss to displaced employees and thereby reduce information asymmetry between employers and employees. I find that, following the adoption of these laws, the number of patent filings and citations decrease for affected employers. These findings are consistent with mandatory disclosure imposing constraints on employers and in turn discouraging the pursuit of innovative activities with higher returns but also higher risk. In support of this negative real effects (i.e., innovation hinderance) channel, I find evidence that employers take on less risky patents that have lower scientific and market value. I use state-level survey and firm-level employment data to provide confirmatory evidence that mandatory disclosure leads to an increase in labor dismissal costs as reflected by a decrease in layoffs.

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"For two centuries, America's free market has not only been the source of dazzling ideas and path-breaking products, it has also been the greatest force for prosperity the world has ever known. We have preserved freedom of commerce while applying those rules and regulations necessary to protect the public against threats to our health and safety and to safeguard people and businesses from abuse. Sometimes, those rules have gotten out of balance, placing unreasonable burdens on business—burdens that have stifled innovation."

- President Barack Obama, The Wall Street Journal (January 18, 2011)

1. Introduction

Mandatory disclosure regulation has gained increasing popularity in recent years, extending beyond its traditional focus on financial disclosures for shareholders (Benston 1973) towards nonfinancial disclosures aimed at a broader set of stakeholders (Christensen et al. 2017; Chen et al. 2018; She 2022; Rauter 2023; Tomar 2023). These mandated disclosures emphasize areas where firms' real actions create negative stakeholder externalities. The goal is to generate an action cycle, whereby disclosure affects the actions of stakeholders (e.g., public pressure), with firms anticipating these actions and altering their real activities (Weil et al. 2013). Despite realized intended benefits for stakeholders, mandated disclosures are not without costs, and can have unintended effects (i.e., negative real effects) for firms (Leuz and Wysocki 2016).

Extant research examines capital market benefits and proprietary costs of mandated disclosures for stakeholders in capital (e.g., shareholders) and product (e.g., competitors, suppliers, and customers) markets but offers limited evidence for stakeholders in labor markets. In this study, I examine the innovation consequences of labor dismissal laws that mandate advance disclosure of employment loss due to mass layoffs and plant closures to displaced employees. These mandated disclosures have benefits, in the form of reduced information asymmetry between employers and employees (Malik 2022), as well as costs, in the form of increased downward labor adjustment (i.e., dismissal) costs (Lazear 1990; Kuhn 1992). Theory provides conflicting predictions on how labor laws that increase dismissal costs impact corporate innovation, a real activity central to long-run productivity and economic growth (Solow 1957).

On the one hand, higher dismissal costs for employers increase job security for employees and in turn incentivize employees to invest in firm-specific human capital and exert greater effort in innovative activities (henceforth "employee incentive hypothesis"). The 20th century industrial U.S. economy provides a powerful setting to test this hypothesis as it operated primarily as a flexible labor market with very few firing constraints. Exploiting the adoption of wrongful discharge laws (WDLs) by U.S. states between 1970-1999 that provide exceptions to "employment-at-will", Acharya et al. (2014) find that WDLs limit employers' ability to hold up innovating employees, thereby providing employees with job security, and encouraging greater employee effort in innovative activities. Acharya et al. (2013) find similar evidence using the adoption of the federal Worker Adjustment and Retraining Notification (WARN) Act of 1989, which mandates at least 60-day advance disclosure of employment loss to displaced employees.

On the other hand, higher dismissal costs increase the adjustment costs employers face in terminating employment contracts and in turn render it more for difficult for employers to pursue innovative activities (henceforth "employer constraint hypothesis"). The labor landscape in some European countries provides a powerful setting to test this hypothesis as it encompasses heavily regulated labor markets with significant firing constraints. When labor laws already provide employee welfare, further laws exhibit diminishing marginal returns in incentivizing employees, while imposing additional constraints on employers (Belot et al. 2007). Using tax return data for French firms from 1994-2007 and exploiting the fact that "most labor regulations" in France are based on employee size thresholds (i.e., 50 employees), Aghion et al. (2023) find that labor laws lead to lower innovation among employers above the regulatory threshold. Garcia-Vega et al. (2021) focus on Spain and find that an exogenous decrease in dismissal costs brought on by a 2012 labor dismissal law change (i.e. "longer trial (probationary periods") increased innovation.

Extant research examines labor dismissal laws that were enacted between 1974 and 1998 while the U.S. was an industrial economy that offered limited employee protection. The 21st century landscape is different, as WDL and WARN laws enacted in the 20th century have provided a minimum floor on employee welfare and restricted the ability of employers to hold up employees.[1] The economic landscape is also different as the prominence of manufacturing firms has diminished while that of high-tech firms has risen. In today's landscape, high-skill employees most intimately involved in innovation activities routinely job hop, suggesting that job security may no longer be as important a consideration in motivating employees (Groysberg et al. 2008; Tambe and Hitt 2014; Ganco et al. 2015). Thus, it is ultimately an empirical question whether the employee incentive effects of labor dismissal laws mandating advance disclosure of will continue to dominate the employer constraint effects in a contemporaneous U.S setting.[2]

I use the staggered adoption of U.S. state-level Worker Adjustment and Retraining Notification laws ("mini-WARN laws") between 2003 and 2015 to examine the impact of labor dismissal laws on innovation. These state mandates were preceded by the federal WARN Act, Mini-WARN laws differ in that they (1) lower the employee size threshold; (2) lower the employment loss threshold; (3) remove circumstances in which WARN laws don't apply; and (4) provide localized support to employers and employees (Ye 2018; Malik 2022). As noted by a U.S. Chamber of Commerce report, mini-WARN laws play a central role in eroding the employment-at-will doctrine and shaping labor dismissal costs for employers (UCC 2011). My analyses employ a sample of 6,585 U.S. firms from 2000 to 2018. My difference-in-differences (DID) tests

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[1] The OECD (1997) notes, "Labor rules are necessary to ensure the well-being of workers. However, sometimes labor laws can discourage innovation by making it more difficult to introduce new technologies or new approaches."

[2] For the period 1999-2017, U.S. federal labor dismissal laws relating to mass layoffs were above average among OECD countries, rivaling countries such as Australia and the U.K, slightly below France, and significantly lower than Spain (Edmans et al. 2023). In contrast, for the period, 1970-1999, U.S. federal labor dismissal laws relating to mass layoffs were below average and significantly lower than almost all OECD counties (Acharya et al. 2013).

use the staggered adoption of mini-WARN laws. By comparing the post-law change in innovation of firms headquartered in states with mini-WARN laws (henceforth "treated firms") with that of firms headquartered in states without mini-WARN laws (henceforth "control firms"), I can identify the causal effect of labor dismissal laws mandating advance disclosure.[3] I use two innovation measures. The first, number of patents filed by a firm, focuses on the quantity of innovation. The second, number of citations that a firm receives on its patents, focuses on the quality of innovation. I find that after the adoption of mini-WARN laws, treated firms experience an average decrease in the number of patents filed (citations) of 4.4% (6.7%).[4]

I observe no differences in innovation between treated and control firms in the two years leading up to the effective date of mini-WARN laws. The decrease in innovation of treated firms occurs only after the effective date. The setting is not immune from criticisms raised about DID research designs using two-way fixed effects models with staggered treatment timing and heterogeneous treatment effects. I re-estimate the baseline model using an alternative estimator and find evidence of mini-WARN laws negatively impacting innovation (Borusyak et al. 2022).[5]

Acharya et al. (2014) find that labor dismissal laws (i.e., WDLs) limit employers' ability to hold up employees after the innovation is successful. By reducing the possibility of employee hold up, labor dismissal laws enhance employees' innovative efforts, leading to greater innovation. Employees can hold up employers by commercializing innovation developed inside the firm outside the firm (Fulghieri and Sevilir 2011). By reducing the possibility of employer hold up,

[3] Headquarter MSA accounts for 50-75% of R&D employment (Derrien et al. 2023) and 50% of R&D facilities are within 60 miles of headquarters (Glaesar et al. 2023). Glaesar and Lang (2023, p. 25) note, "Most research assumes laws affect firms based on their headquarters state, which is a reasonable approximation because top executives work at headquarters and because firms have strong incentives to operate R&D facilities in their headquarters state."
[4] These economic magnitudes are lower than those documented in Acharya et al. (2014) for the good-faith (12.2% for patents and 18.8% for citations) and public-policy (6.7% for patents and 8.2% for citations) WDL exceptions.
[5] In untabulated analysis, I find similar results using fixed-effects Poisson models rather than estimating linear regressions of the natural logarithm of one plus the number of patents and forward citations (Cohn et al. 2022).

labor mobility laws (i.e., inevitable disclosure doctrine (IDD) laws and strict enforcement of non-compete agreements) reduce employees' innovative efforts, leading to lower innovation (Contigiani et al. 2018). The employer constraint hypothesis should dominate the employee incentive hypothesis in instances where employers cannot easily holdup employees (i.e., firms in states with WDL laws) while employees can holdup employers (i.e., firms in states without strict enforcement of non-compete agreements or IDD laws). Consistent with expectations, I find that the negative effects of mini-WARN laws on innovation are concentrated among firms where the bargaining power to holdup lies with employees rather than employers.

Mandated disclosures can affect the actions (e.g., public pressure) of information users (e.g., employees) with disclosing firms (e.g., employers) anticipating these actions and altering their real activity. I next substantiate that mini-WARN laws increase employers' labor dismissal costs by examining layoff activity using state-level survey and firm-level employment data. Lower layoff activity implies greater labor dismissal costs. First, I find that the adoption of mini-WARN laws leads to a decrease in the level of layoffs and discharges, defined by the BLS Job Openings and Labor Turnover Survey (JOLTS) as involuntary separations initiated by the employer, among employers in treated states (Davis et al. 2013). Second, I find that the adoption of mini-WARN laws leads to a decrease in corporate layoffs, measured as an indicator variable equal to one if a firm reduces employment by at least 5% year-over-year among treated firms (Serfling 2016; Karolyi 2018). Third, I find that the adoption of mini-WARN laws leads to a decrease in corporate layoffs, measured as an indicator variable equal to one if a firm reduces employment by at least the number of employees threshold specified within mini-WARN laws year-over-year (i.e., 25 in Iowa, 50 in California and Vermont), among treated firms in states with unambiguous thresholds (Landier et al. 2009; Malik 2022). These results are concentrated in states where the bargaining power to holdup lies with employees rather than with employers. In additional cross-sectional tests, I find that the negative effects of mini-WARN laws on innovation are concentrated among high-tech firms as well as firms with high-skill employees. High-skill employees tend to be complementary to technology adoption and capital-for-labor substitution is less feasible in high-tech firms. For completeness, I assess the possibility that firms shift to less labor-intensive and more capital-intensive technologies. I observe no change in capital expenditures nor a change in the capital-labor ratio after the passage of mini-WARN laws. Bena et al. (2022) note that substitution of capital for labor is facilitated by process, but not non-process, innovation. I find that the adoption of mini-WARN laws leads to a decrease in both process and non-process innovation, inconsistent with pursuit of capital-for-labor substitution.

Mandated disclosures can inhibit firms' ability/incentives to innovate and prompt changes to their innovation strategy (Williams and Williams 2021; Allen et al. 2022; Breuer et al. 2022). I find that treated firms pursue less risky patents that have lower scientific and market value. These findings are consistent with advance disclosure, which makes it difficult to adjust employment, discouraging firms from pursuing innovations with higher returns but higher risk.

This study contributes to the literature in three ways. First, the findings contribute to the accounting literature on the real effects of mandatory disclosure. This literature examines the impact of mandated financial disclosures (e.g., financial reporting mandates) on innovation (Zhong 2018; Brown and Martinsson 2019; Jayaraman and Wu 2019; Williams and Williams 2021; Allen et al. 2022; Breuer et al. 2022). These studies emphasize the role mandated disclosures play in shaping information flow between managers and stakeholders in capital and product markets. My focus on mandated nonfinancial disclosures emphasizing advance notice of employment loss by employers to employees extends this line of research to stakeholders in labor markets (Bova et al.

2015). Advance notice represents an important labor adjustment cost for employers (Lazear, 1990; Kuhn 1992), yet the real effects of this novel form of mandated nonfinancial disclosure have received little attention in the accounting literature.

Second, the findings contribute to the financial economics literature on labor dismissal laws and innovation. The context examined in the literature is manufacturing firms during the 1970-1999 period operating in an industrial economy with flexible labor markets (Acharya et al. 2013; 2014; Bena et al. 2022). Since then, we have witnessed a shift towards a knowledge-based economy yet have limited evidence on how evolution in labor laws impact U.S. firms in the 21st century. This study suggests that as labor regulation evolves over time to afford employees basic protections, further regulation exhibits diminishing marginal benefits in incentivizing employees while imposing additional costs on constrained employers (Belot et al. 2007; Adams et al. 2019).

This study also contributes to the labor economics literature examining the implications of WARN laws. The federal WARN Act is viewed as being ineffective in achieving its objectives of protecting employees, their families, and local communities (Ehrenberg and Jakubson 1993; Addison and Blackburn 1994; GAO 2003) while having negative firm value and leverage implications for employers (Alexander and Spivey 1997; Siminitzi et al. 2015). On the other hand, state-level mini-WARN laws have been demonstrated to benefit displaced employees by aiding them in avoiding short-term joblessness and achieving long-term reemployment (Ye 2018; Malik 2022). As a result of the economic implications of the global pandemic, layoffs and WARN legislation are once again in the spotlight, with several states implementing or modifying mini-WARN laws (SHRM 2022). My evidence on the costs of mini-WARN laws for employers complements evidence of benefits for employees and should be of interest to policymakers. [6]

[6] WARN covered layoffs impact brick-and-mortar sectors but also tech and biotech sectors (Industry Insider, 2023). For example, in November 2022, Twitter abruptly laid off 983 employees in three offices across California (i.e., San

2. Hypothesis development

2.1. Mandated disclosures and corporate innovation

Mandated disclosures provide capital market benefits for firms as transparency reduces information asymmetry between managers and shareholders, resulting in improved liquidity, lower cost of capital, and higher asset prices. At the same time, mandatory disclosures impose proprietary costs on firms as transparency increases information leakage to stakeholders in product markets (i.e.,competitors, suppliers, and customers). This tension between capital market benefits and proprietary costs of mandated disclosures is heightened for innovative activities, which are intangible in nature, highly uncertain, and associated with material firm-specific information. Extant research rigorously examines the innovation consequences of mandated financial disclosures in the U.S. (Jayaraman and Wu 2019; Williams and Williams 2021; Allen et al. 2022), European (Breuer et al. 2022), and cross-country contexts (Zhong 2018; Brown and Martinsson 2019). Related research extends this line of inquiry to mandated nonfinancial patent and clinical trial results disclosures (Kim and Valentine 2021; Aghamolla and Thakor 2022).

2.2. Public policy objectives of mandated nonfinancial disclosures

Mandated nonfinancial disclosures are intended to serve specific policy objectives vis-à-vis a broad set of stakeholders, such as enhancing employee safety (Christensen et al. 2017) or fiscal revenue collection from multinational firms in foreign host countries (Rauter 2023) as well as reducing environmental pollution (Chen et al. 2018; Tomar 2023) or suppliers' human rights abuses (She 2022). These disclosures emphasize areas where corporate actions create negative externalities and their goal is to generate an action cycle, whereby disclosure affects the actions of stakeholders (e.g., public protests or shaming), with disclosing firms anticipating these actions and

Franciso, Santa Monica, and San Jose), who in turn initiated a class action lawsuit alleging violations of the federal and state WARN laws (CNBC 2022). The laid off employees were predominately knowledge-based workers.

altering their real behavior accordingly (Weil et al. 2013). However, the disclosures are not without costs, and can result in unintended consequences (i.e., negative real effects) for firms.

2.3. Benefits and costs of advance disclosure of employment loss

As with other forms of mandated disclosure, tensions arise between benefits to certain stakeholders (e.g., employees) and potential costs to others (e.g., employers) of advance notice of employment loss. The benefits accrue to employees and local communities. Advance disclosure of employment loss serves a public policy objective. That is, these mandated disclosures provide displaced employees an opportunity to obtain a new job as quickly as possible and minimize the cost of displacements for employees, their families, and local communities (Malik 2022). Advance disclosure of employment loss also allows local governments to mobilize their resources to assist in the search for alternative employment and aids local communities by giving them time to adjust to the increased demand for social services and loss of tax revenues.

At the same time, advance disclosure of employment loss gives rise to labor adjustment costs for employers as reflected by several implicit opportunity costs (Friesen 2005). First, laid off employees may be less productive after receiving notice of pending layoffs. Second, laid off employees may secure alternative employment prior to their layoff date, thereby reducing the productivity of existing employees who are left to pick up any slack from early departure. Third, retaining employees for the entirety of the notice period may require employers to employ them when their wage exceeds the value of their marginal product. Fourth, other stakeholders may respond to layoff notices in ways that are costly to employers (e.g., cancelled purchase orders).

While implicit opportunity costs are difficult to directly measure, prior research offers two pieces of indirect evidence suggesting that the costs are substantial.[7] First, employers rarely

[7] Prior work identifies capital market benefits to mandated disclosures and direct (i.e., preparation, certification, and litigation) as well as indirect (i.e. proprietary, modified behavior relating to investment and use of resources) costs.

voluntarily provide advance disclosure when not required by law, suggesting it is costly to provide (Addison and Blackburn 1994). Second, it is common for employers to exercise their legal option to provide severance pay in lieu of advance disclosure, suggesting the costs exceed the value of the wage bill for the same period for these employers (Jones and Kuhn 1995).

2.4 Labor dismissal laws and corporate innovation

Labor dismissal laws can have both ex-ante and ex-post effects on innovation. The ex-ante effect, which I term the employee incentive hypothesis, is motivated by the literature on incomplete contracting (Grossman and Hart 1985; Hart and Moore 1990; Hart 1995). In the presence of incomplete employment contracts, labor dismissal laws can provide employers with a commitment mechanism to not punish short-term failures or hold up employees after effort has been exerted and innovation projects have been successful. As such, labor dismissal laws increase job security which incentivizes employees to invest in firm-specific human capital and exert greater effort in innovative activities. Acharya et al. (2013) exploit country-level changes in labor dismissal laws across four countries (i.e., U.S, U.K, Germany, and France) from 1970 to 2002 and find evidence consistent with the employee incentive hypothesis. The authors replicate this result using within U.S. variation stemming from the adoption of the federal WARN Act in 1989. Acharya et al. (2014) exploit state-level WDL changes in labor dismissal laws from 1970 to 1999 and also document evidence consistent with the employee incentive hypothesis.[8]

The ex-post effect, which I term the employer constraint hypothesis, is motivated by the literature on labor adjustment costs. Labor dismissal laws make it more difficult for employers to terminate employment contracts (Lazear 1990). Increased labor dismissal costs lead to

[8] Acharya et al. (2013; 2014) are careful to note that labor dismissal laws can also create incentives for employees to exert less effort in innovative activities (Sauermann and Cohen 2010). Unionization, an alternative mechanism that encourages job security, has been shown to lead to negative employee incentive effects (Bradley et al. 2017).

underinvestment in innovative activities, particularly those that require experimentation with new technologies with higher returns but also higher risk of failure. Aghion et al. (2023) and Garcia-Vega et al. (2021) study innovation and find evidence consistent with the employer constraint hypothesis in France and Spain.[9] Specific to the U.S., Autor et al. (2007) examine WDLs and find evidence that higher labor adjustment costs generate labor rigidity (i.e., less hiring and firing). Bena et al. (2022) find manufacturing firms respond to increased dismissal costs arising from WDLs by pursuing process innovation that replaces labor with capital.

In sum, extant research finds that, within the U.S. context, labor dismissal laws lead to greater corporate innovation among firms during the 20th century, either because employees are better incentivized to pursue innovative activities as they face lower risk of employer hold up (i.e., employee incentive hypothesis) or because employers can offset the negative implications of higher labor adjustment costs through capital deepening (i.e., are able mitigate the employer constraint hypothesis). Conversely, extant research in international contexts finds robust evidence consistent with the employer constraint hypothesis in both the 20th and 21st century.[10]

2.5 Hypothesis development

Adams et al. (2019, p. 4) survey the literature on labor dismissal laws and note, "economic theory does not currently offer a clear answer to the question of whether the economic effects of employment protection laws are generally harmful or beneficial. A given employment protection reform will have different economic effects depending on the overall, preexisting level of protection in a given country."[11] Belot et al. (2007, p. 394) use a theory model to demonstrate that

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[9] Persistently high unemployment rates in certain European counties, such as France and Spain, is termed "Eurosclerosis" and attributed to labor dismissal laws that create very rigid labor markets (Belot et al. 2007).
[10] In a World Bank survey, a key determinant of a firm listing labor laws as a major obstacle was whether the firm was an innovator, especially if a firm was located in a country with stringent labor laws (Pierre and Scarpetta 2006).
[11] Elsewhere, Adams et al. (2019, p. 10) study the global evolution of labor dismissal laws over the last 50 years and make note of, "the overall trend of a steady increase in the level of (employment) protection since the 1970s".

there is a strictly positive (but finite) optimal level of employment protection, "at low levels of employment protection an increase in protection stimulates growth; at high levels of employment protection an increase in protection is harmful to growth." Edmans et al. (2023, p. 8) offer related arguments for firm-specific investments, "motivational benefits of employee satisfaction may be higher in more flexible labor markets. These same reasons imply that these benefits are lower in rigid labor markets, causing a downward shift in the marginal benefit curve, potentially into negative territory. When regulations already ensure that the average firm is offering a certain level of wages, job security, and employee representation, companies with high satisfaction relative to their peers may be in negative territory."

I hypothesize that state-level investments in labor dismissal laws could exhibit diminishing marginal returns in incentivizing employees in ways similar to firm-level investments in employee satisfaction. When federal (i.e., WARN) and other state-level labor dismissal laws (i.e., WDL) already ensure a minimum level of employee welfare, particularly in protecting innovating employees from the risk of employer hold up, further labor market regulation at the state-level is likely to exhibit diminishing marginal returns in incentivizing employees. In the absence of significant employee incentive effects, employer constraints are expected to be the primary channel through which labor dismissal laws impact innovation.[12] Employer constraints arise as mandated advance disclosure of employment loss heightens labor adjustment costs and thus can generate negative real effects with respect to innovative activities. That is, labor dismissal laws mandating advance disclosure of employment loss can incentivize employer underinvestment

[12] Belot et al. (2007, p. 387) note, "On the one hand, employment protection stimulates firm-specific training by the employee, which can be welfare-enhancing given that effort is not contractible. On the other hand, firing costs also have a negative effect on welfare, and represent a direct cost at separation. If the costs of effort are convex, the marginal benefit of increasing employment protection in terms of welfare will fall with employment protection. At some point, the loss in terms of expected separation costs will dominate the gain in terms of productivity increase."

in innovative activities that require experimentation with new technologies with higher returns but higher risk of failure.[13] This leads me to predict that state-level mini-WARN laws result in lower firm-level corporate innovation.

H1: State-level mini-WARN laws lead to lower firm-level corporate innovation.

3. Institutional background

The deep recession of the mid 1970's and resulting plant closings and mass layoffs in major manufacturing firms acted as an impetus for dialogue in support of labor dismissal laws. Supporters of legislative efforts to strengthen labor dismissal laws argued that advance disclosure benefits displaced employees through facilitating an easier transition to new jobs and higher replacement earnings. Implicit in these arguments is that bargaining between employers and employees over provision of advance disclosure suffers from a form of market failure (i.e., commitment problem arising from weak reputation effects for firms or large compliance and enforcement costs of private-sector advance disclosure contracts)(Addison and Blackburn 1994).

Opponents of legislative efforts to strengthen labor dismissal laws argued that advance disclosure would significantly increase labor adjustment costs for employers and ultimately contribute to lower levels of employment. Opponents argued that advance disclosure imposes productivity costs on employers by encouraging the most productive employees with outside options to leave and in turn depress employee morale among remaining employees (Kuhn 1992). Opponents also noted that advance notice disclosure impact employers in their dealing with other stakeholders: customers could place fewer orders, investors could supply less capital, suppliers could be reluctant to provide goods and services, and potential acquirers could have diminished

[13] Labor flexibility is key for knowledge-based work. SpaceX referenced "some rebalancing of resources" in relation to a 2014 layoff of 5% of its workforce. Industry analyst Marco Caceres noted, "It wouldn't surprise me if they were testing a technology that didn't work out and they no longer need those employees." (England-Nelson 2014).

interest (Ehrenberg and Jakubson 1993).[14] While legislation calling for mandatory advance disclosure was active in Congress beginning in 1979, it took until 1988 for it become law.[15]

The federal WARN Act went into effect in 1989. It offers protection to employees, their families, and communities by requiring employers to provide notice at least 60 days in advance of covered plant closings and mass layoffs. Advance disclosure is required to be provided to affected employees, to the state-level dislocated worker unit, and local governments. Employers are covered if they have 100 or more full-time employees and employees are covered if they are hourly or salaried employees, inclusive of managerial employees. A plant closing occurs when an employment site is shut down and this shutdown results in employment loss for 50 or more employees during any 30-day period. A mass layoff is defined as an instance in which there is no plant closing but there is employment loss for 500 or more employees (or alternatively at least 33% of the active workforce if the absolute number of employment loss is between 50 and 499).

There are three exceptions to the requirement that an employer provide at least 60 day advance disclosure: (1) faltering company (where disclosure would prevent employers from obtaining new capital to stay open); (2) unforeseeable business circumstances (where business circumstances that were not reasonably foreseeable at the time disclosure would otherwise have been required arise); (3) natural disasters (where the plant closing or mass layoff is the direct result of a natural disaster). Employers bear the burden of proof for exceptions (Levine 2007).

An employer who violates the federal WARN Act requiring advance disclosure to employees is liable to each aggravated employee for an amount including backpay and benefits

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[14] Firm opposition to WARN also stemmed from fears that its passage would facilitate further, more intrusive laws. A 1987 Wall Street Journal (WSJ) article was headlined, "Business is Worried That Bill on Plant Closings Presages More Laws Mandating Worker Benefits". White House economist Beryl Sprinkel noted, "There is a total package up there, which together all works in the direction of imposing rigidities on our labor market. We don't want to repeat the failed experiment of Western Europe resulting from mandated costs on the private sector".

[15] In line with the regulatory literature that argues lobbying occurs by firms most affected by a law (Hochberg et al. 2009), the National Association of Manufacturers actively lobbied against the federal WARN Act (WSJ 1987).

for the period of violation, to a maximum of 60 days. This liability may be reduced by wages paid during the period of violation and unconditional payments made to employees. An employer who violates the federal WARN Act without disclosing to the local government is subject to a civil penalty of $500 for each day of violation. Enforcement occurs through the United States district courts. Employees and governments can file individual or class action lawsuits.[16]

The literature finds mixed evidence on the effectiveness of the federal WARN Act. Addison and Blackburn (1994) find evidence of only marginal increases in the provision of advance disclosure after the law change, which they attribute to the many burdensome requirements (i.e., minimum employee level, minimum employment loss level, many exceptions, etc.) rendering most corporate displacements exempt from the federal WARN Act. A 2003 Government Accountability Office study found similar evidence but also points out that specifics of the law are difficult to interpret for employers and that the Department of Labor plays a limited role in facilitating employer compliance with the law (GAO 2003). Weil et al. (2006) compare the federal WARN act to other transparency initiatives and note that it is ineffective as the mandated disclosure is not embedded in everyday decision-making. Alexander and Spivey (1997) find evidence of a negative market reaction for small firms (but not large firms), consistent with regulatory burden of compliance falling disproportionately on small firms.[17,18] Siminitzi et al.

[16] The Toledo Blade newspaper noted in a 2007 noted that "A Blade analysis of 226 lawsuits filed in Federal courts across the country since 1989 revealed that judges threw out more than half the cases. In 108 cases, WARN Act lawsuits resulted in settlements or with the courts siding with the displaced workers. But in dozens of those cases, workers received only pennies on the dollar of what they felt they were owed." (Eder and Drew 2007). While federal WARN lawsuits are relatively uncommon, litigation is more frequent in states with mini-WARN laws (Malik 2022).
[17] Large firms argued that federal WARN Act was a non-event as they were already voluntarily complying with its provisions. A 1987 WSJ was headlined, "Many executives say closings law isn't a big problem." 50% of 224 large corporations surveyed by Conference Board indicated they voluntarily provided 90 days' notice of layoffs while less than 20% of small corporations surveyed by GAO indicated they voluntarily provided more than 30 days' notice.
[18] In untabulated tests, when I partition my sample on firm size (i.e., market value), I also find that the negative effects of mini-WARN laws on corporate innovation are concentrated among smaller firms (Breuer et al. 2022).

(2015) find evidence that the federal WARN act increases operating leverage, thereby crowding out financial leverage, particularly in sectors where labor turnover is common.

State-level mini-WARN laws have the same objective as the federal WARN act: to provide displaced employees an opportunity to obtain a new job as quickly as possible and in turn minimize the cost of displacements for employees, their families, and local communities. They were enacted in response to the two main perceived limitations of the federal WARN Act: (1) parameters that narrowed the scope of displacements that were covered under the law and (2) lack of extensive documentation and resources to aid in interpretation of the law and thereby facilitate employer compliance. As state-level mini-WARN laws are more expansive than their federal counterpart, are easier for employers to comply with, and are tailored to the local labor market in each state, the literature views them as being more rigorous (Ye 2018; Malik 2022).

State-level mini-WARN laws apply to employees working at employer establishments within state boundaries. As summarized in Table 1, over the period 2003-2015, 7 U.S. states passed mini-WARN laws, beginning with California in 2003 and ending with Vermont in 2015 (Malik 2022).[19] Some states lower the employee threshold at which the law applies to employers (e.g., 75 in California) while others lower the threshold at which the law applies to employment losses (e.g., 25 in Iowa). Some states increase the advance disclosure period (e.g., 90 days in New York) while others decrease the advance disclosure period but offer broader protections elsewhere within their mini-WARN law (e.g., 30 days in Vermont but the law applies to employers with 50 or more employees). Some states remove the exceptional circumstances in which WARN laws don't apply (e.g., California does not recognize the unforeseen business circumstances exception) while others

[19] Delaware (Maryland) implemented a mini-warn law effective January 7, 2019 (October 1, 2020). I exclude these states and end the sample period in 2018 to avoid the potential confounding effects of the global pandemic. Other states (e.g., New Jersey 2023, New York 2023) have only very recently modified pre-existing mini-WARN laws.

add relocations as additional (to plant closings and mass layoffs) circumstances to which WARN laws do apply (e.g., Illinois). Some states increase the fines and penalties for violation of their mini-WARN law (e.g., one week pay for each full year of employment when the employer violates the act in New Jersey). Finally, some states provide for public enforcement through an administrative agency (New York Commission of Labor has enforcement power) in addition to private enforcement through litigation. Overall, while there is some variation across states in the rules around mini-WARN laws, there is consensus around the idea that they represent more stringent labor dismissal laws than the federal WARN Act.

4. Sample, variable measurement, and research design

4.1. Data and sample

I obtain financial data from Compustat and patent data from Kogan et al. (2017). The patent database reports patent number, CRSP PERMNO, issue date, filing date, forward citations of a patent, and the value of a patent. Patent applications are included in the database only if they are eventually granted by the USPTO, with an average lag of two to three years between patent filing and patent grant. My sample period begins in 2000, which is three years before the first state (i.e., California 2003) adopted a mini-WARN law. My sample period ends in 2018, which is three years after the last state (i.e., Vermont 2015) adopted a mini-WARN law. My sample firms consist of all firms in the merged CRSP/Compustat database. I exclude firms headquartered outside the U.S. I exclude firms in the financial industry (Standard Industrial Classification (SIC) codes 6000-6999) and utilities industry (SIC 4900-4999) because of the differences in regulatory oversight for these industries. My final sample consists of 54,607 firm-year observations (6,585 unique firms) from 2000 to 2018 with all the financial and patent data.[20]

[20] For comparability with Acharya et al. (2014), I do not impose the sample restriction that sample firms have at least one patent over the sample period. In untabulated analysis, I find similar results when imposing this restriction.

4.2. *Variable measurement*

4.2.1 *Identifying treated and control firms*

To identify firm-year observations impacted by the adoption of state-level mini WARN laws, I construct an indicator variable *MW*. *MW* equals 1 if a firm's headquarter state adopts a law in the adoption year and all the subsequent years.[21] *MW* equals 0 in years prior to a firm's headquarter state adopting a law or in all years if a firm's headquarter state does not adopt a law.

4.2.2 *Measuring innovation*

I use natural logarithms because of the right skewness of patent data. *LN_NPAT* is measured as the natural logarithm of one plus the number of patents filed one year after the year in which the key independent variable *MW* is measured. This variable counts the number of patent applications filed in a year that are eventually granted. The relevant year is the filing year since it is closer to the time of innovation relative to the grant year. The number of patents is subject to a truncation problem because patents appear in the database only after they are granted. I include year fixed effects to mitigate this. *LN_NCITE* is measured as the natural logarithm of one plus the number of forward citations received on patents that are filed one year after the year in which the key independent variable *MW* is measured. To mitigate the truncation problem with citations, I employ year fixed effects and adjust patent citations by scaling citations in a given year by the average number of citations per patent in the same industry-year.

4.2.3. *Control variables*

I control for firm and industry characteristics that can affect corporate innovation. Firm-level controls include firm size (*FSIZE)*, cash holdings (*CASH)*, leverage (*LEV)*, capital expenditures (*CAPEX)*, return on assets (*ROA*), market-to-book (*MTB)*, R&D (*RD)*, and fixed

[21] I use historical headquarter data (sourced from 10-K header data) rather than Compustat current headquarter data.

assets (*FIXED)*. Innovative firms are usually larger, profitable firms with available cash, higher growth opportunities, lower leverage, and higher asset tangibility. Industry-level controls include industry concentration (*HI)* and squared industry concentration (*HISQ)* to account for nonlinear effects of product market competition. To minimize effects of outliers, I winsorize all variables at the 1st and 99th percentiles. Detailed variable definitions are provided in Appendix A.

4.3. Research Design

I test my hypothesis of the effect of state-level labor dismissal laws on corporate innovation using the following OLS models (1) and (2). All the subscripts are suppressed, as all independent variables are measured in the same time period - year *t*.

$$LN\_NPAT = b_0 + b_1MW + b_2CASH + b_3FSIZE + b_4LEV + b_5CAPEX + b_6ROA + b_7MTB + b_8FIXED + b_9HI + b_{10}HISQ + b_{11}RD + FIRMFE + YEARFE \quad (1)$$

$$LN\_NCITE = b_0 + b_1MW + b_2CASH + b_3FSIZE + b_4LEV + b_5CAPEX + b_6ROA + b_7MTB + b_8FIXED + b_9HI + b_{10}HISQ + b_{11}RD + FIRMFE + YEARFE \quad (2)$$

In both models, the key independent variable is *MW*, which equals 1 if a firm is headquartered in a state with a mini-WARN law in year *t*. The coefficient on *MW* indicates how the corporate innovation of treated firms changes after mini-WARN laws compared with that of control firms. My hypothesis predicts a significant and negative coefficient on *MW*.

To estimate generalized DID regressions, my models need to include a set of group and time fixed effects. I include firm and year fixed effects. The firm fixed effects allow me to control for time-invariant differences in patenting across firms. The year fixed effects enable me to control for intertemporal macroeconomic shocks, as well as the fact that citations to patents applied for in later years will be lower than those in earlier years.[22] These fixed effects lead to b_1 being estimated as the within-state differences before and after the law change as opposed to similar before–after

[22] In untabulated analysis, I find similar results when using census region-by-year and industry-by-year fixed effects.

differences in states that did not experience a change during the same period. As my treatment is defined at the headquarter state, I cluster standard errors by headquarter state.

5. Results

5.1. Descriptive statistics

Descriptive statistics are presented in Table 2. 24% of firm-year observations in the sample have *MW* = 1. The mean of *NPAT* (the number of patents filed by a firm in year *t+1*) is 8.293, and the mean of *NCITE* (the number of forward citations received on patents filed by a firm in year *t+1*) is 8.766. Sample firms have a median cash to total assets ratio of 13.12%. Sample firms are moderately levered with a median book leverage ratio of 16.46% and fixed assets account for 15.83% of total assets in the average firm. Sample firms perform well with a median *ROA* of 9.99% and have moderate growth opportunities with *MTB* of 1.56%.

5.2. Primary results

Table 3 reports the difference-in-differences estimates of the impact of the adoption of state-level mini-WARN laws on the innovation outcomes of treated firms. Models (1) and (2) (Models (3) and (4)) report the results for the natural logarithm of the number of patents (the natural logarithm of the number of patent citations). In Models (1) and (3), I include the *MW* indicator as well as firm and year fixed effects. For Models (2) and (4), I also add the controls.

The adoption of labor dismissal laws has a negative and statistically significant impact on the innovation outcomes of treated firms. The estimated coefficients in Models (2) and (4) in Table 3 imply that, after the implementation of state-level mini-WARN laws, the number of patents (patent citations) of treated firms decreases by 4.37% (6.65%), equivalent to a decrease of 0.36 patents (=8.29 x 4.37%) and 0.58 patent citations (=8.77 x 6.65%).[23]

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[23] When a coefficient in a log-linear regression is small, it approximately corresponds to the percentage change in the dependent variable if the independent variable increases by 1.

With regards to control variables across both models (2) and (4), large firms, firms with greater cash holdings, firms with high R&D expenditures, firm with high asset tangibility, firms with higher growth potential, as well as firms with lower leverage are more innovative.[24]

5.3. *Pre-treatment trends*

The validity of a DID estimation depends on the parallel trends assumption: absent the mini-WARN laws, treated firms' innovation would have evolved in the same way as that of control firms. This assumption is inherently untestable because I do not observe the treated firms in the absence of treatment. However, I can obtain suggestive evidence by examining pre-treatment trends. In Table 4, I employ an event-time specification where the two dependent variables are measured contemporaneously and the key variables of interest are MW^{-2}, MW^{-1}, MW^{+0}, MW^{+1}, MW^{+2}, *and* MW^{3+} which are equal to one if the firm is headquartered in a state that will adopt mini-WARN law in two years, in one year, the current year, adopted one year ago, two years ago, and three or more years ago, and zero otherwise. Across all four columns, I find that none of the coefficients in pre-treatment period are statistically significant.

5.4. *Potential bias arising from staggered treatment timing and heterogenous treatment effects*

With staggered treatment timing and heterogeneous treatment effects, two-way fixed effects (TWFE) estimation can introduce a "forbidden comparisons" problem by comparing later treated firms to earlier treated firms as a control, yielding biased estimates of treatment effects. This occurs as the estimator is a weighted average of each possible two-by-two DID comparison combination, some of which use earlier treated firms as controls for later treated firms. If later adopting states learn from earlier adopting states and produce more effective laws, staggered treatment timing will bias the estimator towards zero as the design pools earlier and later adopting

[24] For brevity, the coefficients on control variables are not reported in subsequent tables.

states. If the impact of laws grows over time, heterogenous treatment effects will bias the estimator towards zero as the design will use some earlier treated firms as controls for later treated firms. In Table 5, I use an alternative estimator that imputes counterfactuals and compares treatment effects using only untreated observations. It estimates fixed effects among the untreated observations only, imputes untreated outcomes for treated observations, and then forms treatment-effect estimates as weighted averages over the differences between actual and imputed outcomes (Borusyak et al. 2022). Across all columns, I find that none of the coefficients in pre-treatment period are statistically significant. The coefficients on MW^{+0}, MW^{+1}, MW^{+2} *, and* MW^{+3} are negative and statistically significant, at the 5 percent level, across all four columns.[25]

5.5. Cross-sectional tests: Interactions in labor laws governing employer-employee relations

The employer constraint hypothesis should dominate the employee incentive hypothesis where employers cannot easily holdup employees while employees can holdup employers. I split out sample firms into two groups: (1) firms headquartered in WDL states with the good-faith and/or public-policy exceptions to the employment-at-will doctrine (*WDL*) as well as firms with lax enforcement of non-compete agreements (*LNC*) (i.e., below median on the Marx 2022 enforcement index) and (2) firms headquartered in all other states *(NWDL* and/or *HNC*).[26] In Panel A of Table 6, I re-estimate models (1) and (2) on the two subsamples. The coefficient on *MW* is

[25] Sun and Abraham (2021) show that, in the presence of staggered treatment timing and treatment effect heterogeneity, TWFE DiD dynamic effect estimates for one relative-time period can be contaminated by the causal effects of other relative-time periods in the estimation sample. The theoretical framework and statistical package of Borusyak et al. (2022) that I employ in Table 5 explicitly avoid this contamination of dynamic effect estimates. Sant'Anna and Zhao (2020) outline a very stringent set of assumptions under which inclusion of time-varying control variables in TWFE DiD regressions produce consistent estimates of the average treatment effect on the treated (ATT). Following the recommendations in Baker et al. (2022), I have included a variant of the TWFE DiD without control variables in Tables 4 and 5 to assess the impact of time-varying control variables. The theoretical framework and statistical package of Borusyak et al. (2022) that I employ in Table 5 explicitly incorporate time-varying control variables. The collective evidence from columns (1) and (3) in Table 4 and 5, as well as the more appropriate (relative to Table 4, columns (2) and (4)), estimation in Table 5, columns (2) and (4), all suggest that the impact of state-level WARN acts on patenting occur immediately and persists in ensuing years.

[26] Marx (2022) constructs a state-level, time-varying index based on changes in judicial/legislative decisions. Lax enforcement facilitates employee mobility and increases the ability of employees to potentially hold up employers.

insignificant for firms headquartered in all other states in columns (2) and (4). In contrast, the coefficient on *MW* is -0.1161 and -0.1530 for firms where the power to holdup lies with employees not employers in columns (1) and (3), significant at the 1 percent level. In Panel B of Table 6, I split out sample firms into two groups: (1) firms headquartered in WDL states with the good-faith and/or public-policy exceptions to the employment-at-will doctrine (*WDL*) as well as firms without inevitable disclosure doctrine (IDD) laws *(NID*) and (2) firms headquartered in all other states *(NWDL* and/or *ID*).[27] In Panel B of Table 6, I re-estimate models (1) and (2) on the two subsamples. The coefficient on *MW* is insignificant for firms headquartered in all other states in column (2) and significant at the 10 percent level in column (4). In contrast, the coefficient on *MW* is -0.0919 and -0.1173 for firms where the power to holdup lies with employees not employers in columns (1) and (3), significant at the 1 percent level.

5.6. Directly validating the employer constraint hypothesis

By reducing the information asymmetry between employers and employees regarding employment loss, mandated disclosure can affect the actions of information users with disclosing firms anticipating these actions and altering their behavior (Weil et al. 2006; Christensen et al. 2021). The employer constraint hypothesis posits that labor dismissal laws raise employers' adjustment costs of labor, incentivizing them to fire less, resulting in lower levels of layoffs. To directly validate this hypothesis, I conduct three additional tests in Table 7. As each individual test has limitations (i.e., data availability, level of aggregation, precision of measurement), I triangulate across three distinct tests to assess the impact of mini-WARN laws on layoff activity.

First, I use BLS data on layoffs (Davis et al. 2013). The JOLTS program collects data from 2001 onwards where 21,000 establishments indicate their number of layoffs and discharges. The

[27] IDD laws limit mobility of knowledge workers (i.e., innovating employees). The absence of these laws increases the ability of employees to potentially hold up employers (Contigiani et al. 2018 Glaeser 2018; Chen et al. 2021).

strength of this data is that it provides an objective measure of mass layoffs that correspond approximately to WARN layoffs (i.e., plant closings, downsizing layoffs, and discharges).[28]The limitation of this data is that it includes both private and public sector employers. To capture layoffs, I use *JOLTS_LAYOFF*, measured as the natural logarithm of the number of involuntary separations for state *s* in year *t*, where an involuntary separation includes layoffs with no intent to rehire, discharges because positions were eliminated, discharges resulting from mergers, downsizing, or plant closings, firings or other discharges for cause, terminations of seasonal employees, and layoffs (suspensions from pay status) lasting or expected to last more than 7 days. I estimate a model with *JOLTS_LAYOFF* as the dependent variable and *MW* as the independent variable of interest. I include natural logarithms of several measures as controls, including population density (*POP),* vote share for the democratic party (*VOTE*), unemployment rate (*UR*), median household income (*HI*), and GDP growth *(GDP*).[29] The model also includes year and state fixed effects. The results are reported in Panel A of Table 7. The coefficient on *MW* is insignificant for employers in all other states in columns (2) and (4). In contrast, the coefficient on *MW* is -0.0446 and -0.0576 for employers where the bargaining power to holdup lies with employees not employers in columns (1) and (3), significant at the 10 percent level.

Second, I use Compustat data on annual percentage changes in the number of employees (Serfling 2016). The strength of this data is that it is widely available for sample firms. The limitation of this data is that it captures net changes in employment (i.e., number of hired, fired, and departing employees). To capture corporate layoff events, I use *COMPUSTAT_LAYOFF*,

[28] Krolikowski and Lunsford (2022) collect aggregate state-level WARN layoff disclosure data from 1994 onwards for 33 states with available data from private sector and non-profit employers. The data is only reliably available from 2006 onwards, and even then, only for a subset of states. In states with mini-WARN laws, that data may be confounded by both federal and state layoff disclosure. I obtain firm-year data for approximately 2,000 WARN layoffs for sample firms. Similar to the aggregated WARN data, the disaggregated data is sparsely populated for many relevant years and states. This precludes use of the aggregated or disaggregated WARN data in the analysis.

[29] As GDP growth can have negative values, I define this variable in its raw form without taking natural logarithms.

measured as an indicator variable equal to one if the year-over-year percentage decrease in the number of employees is 5% or higher for firm *i* in year *t*, and zero otherwise.[30] I estimate a model with *COMPUSTAT_LAYOFF* as the dependent variable and *MW* as the independent variable of interest. The control variables are the same as in models (1) and (2). The model includes year and firm fixed effects. The results are reported in Panel B of Table 7. The coefficient on *MW* is 0.0251 and 0.0372 for firms headquartered in all other states in columns (2) and (4), significant at the 10 percent level.[31] In contrast, the coefficient on *MW* is -0.0290 and -0.0277 for firms headquartered in states where the bargaining power to holdup lies with employees rather than employers in columns (1) and (3), significant at the 5 percent level.

Third, I use Compustat data on year-over-year changes in the number of employees (Landier et al. 2009; Malik 2022). The employment loss threshold at which the mini-WARN law applies varies across states and also often within states depending on the employment loss event.[32] However, three states have one unambiguous employment loss threshold at which the law applies to all employment loss events (i.e., Iowa with 25 employees and California and Vermont with 50 employees).The strength of this setting is that it allows me to precisely measure layoffs impacted by mini-WARN laws. The limitation of this setting is that it is not unambiguously available for all treated states. To capture corporate layoff events, I use *25_LAYOFF (50_LAYOFF),* measured as an indicator variable equal to one if the year-over-year decrease in the number of employees is 25 (50) or higher for firm *i* in year *t*, and zero otherwise. I estimate a model with the above two variables as dependent variables and *MW* as the independent variable of interest. The control

[30] Following Serfling (2016), I use 5% but the results are insensitive to using 7.5% or 10% cutoffs (Karolyi 2018).
[31] I do not observe positive and significant coefficients in columns (2) and (4) in Panel A of Table 7 nor when using alternative (untabulated) cutoffs of 7.5% or 10% year-over-year percentage decrease in number of employees.
[32] For example, New Hampshire's mini-WARN law has an employment loss threshold of 50 employees for plant closings and 500 (25) employees for mass layoffs involving less than (more than) one-third of the total workforce.

variables are the same as in models (1) and (2). The model includes year and firm fixed effects. The model is estimated six years around mini-WARN law adoption by each of the three focal states. The results are reported in Panel C of Table 7. In columns (1) and (2), I estimate this only for firms headquartered in Iowa, relative to firms headquartered in states where the bargaining power to holdup lies with employees rather than employers.[33] In columns (3) and (4), I estimate this only for firms headquartered in California and Vermont, relative to firms headquartered in states where the bargaining power to holdup lies with employees rather than employers. The coefficient on *MW* is -0.0527, -0.0665, -0.0158, and -0.0139 in columns (1) through (4), significant at the 10 percent level.

5.7. Cross-sectional tests: Industry-level technology and skill partitions

The negative employer constraint effects on innovation are more difficult to mitigate through capital-for-labor substitution in high-tech relative to traditional low-tech sectors. Thus, I expect the treatment effect to be concentrated in high-tech firms (i.e., computers, electronics, pharmaceutical, and telecommunications). I use the Kile and Phillips (2009) definition to identify high-tech firms (*HT)*. All other sample firms are identified as low-tech firms (*LT)*. In Panel A of Table 8, I re-estimate models (1) and (2) on the two subsamples. The coefficient on *MW* is insignificant for low-tech firms in columns (2) and (4). In contrast, the coefficient on *MW* is -0.0788 and -0.1112 for high-tech firms in columns (1) and (3), significant at the 5 percent level.

The negative employer constraint effects on innovation are more difficult to mitigate through capital-for-labor substitution in firms with a greater proportion of knowledge workers (i.e., complements to technology). Thus, I expect the treatment effect to be concentrated in firms with

[33] Iowa is a WDL state suggesting employers have limited ability to hold up employees. Iowa is also an IDD state with above-average enforcement of non-compete agreements suggesting employees also have limited ability to hold up employers. In untabulated analysis, I find similar results when using all other states as a benchmark for Iowa.

highly skilled employees (Malik 2022). I use median splits of the Belo et al. (2017) measure to identify industries with high-skill (*HS)* and low-skill (*LS*) employees.[34] In Panel B of Table 8, I re-estimate models (1) and (2) on the two subsamples. The coefficient on *MW* is insignificant for low-skill firms in columns (2) and (4). In contrast, the coefficient on *MW* is -0.0630 and -0.0772 for high-skill firms in columns (1) and (3), significant at the 1 percent level.

5.8. Physical capital deepening as a response to mini-WARN laws.

I assess the possibility that firms can mitigate the negative effects of mini-WARN laws by shifting to less labor-intensive and more capital-intensive technologies. In Panel A of Table 9, I repeat the main tests using capital and R&D expenditures scaled by total assets as the dependent variables.[35]These two dependent variables capture investment in physical capital (Acharya et al. 2013).[36] The coefficient on *MW* is -0.0010, -0.0017, and 0.0003 in columns (1) through (3), all insignificant at conventional levels.[37] I also use the capital-to-labor ratio, defined as the natural logarithm of property, plant, and equipment divided by employment (Bena et al. 2022).[38] The coefficient on *MW* is -0.0360 in column (4), insignificant at conventional levels.[39] As physical capital deepening is facilitated by process, but not non-process, innovation, I examine these forms of innovation separately in Panel B of Table 9. To capture process (non-process) innovation, I use *LN_PROCI (LN_NPROCI),* measured as the natural logarithm of one plus the number of process

[34] Belo et al. (2017) use two data sources to construct a variable measuring the skill level of the employees in an industry. First, they use data from the Dictionary of Occupational Titles (DOT). This database provides the amount of time required by a worker to learn and develop skills for each occupation. Second, they use Occupational Employment Statistics (OES) data. The OES data provides the number of employees by occupation in each industry.
[35] In untabulated analysis, I find similar results if I define these variables in their raw form not natural logarithms.
[36] Kim and Valentine (2021) note, "R&D intensity captures the share of resources a firm devotes to developing inventions and capital expenditures represent investments that can increase capacity to produce new products."
[37] Approximately 35% of sample firms report missing R&D. To ensure this is not biasing inferences (Koh and Reeb 2015), I tabulate results both when setting missing values to zero (1) and excluding these observations (2).
[38] Bird and Knopf (2009) and Bena et al. (2022) only use capital investments to assess physical capital deepening. However, Acharya et al. (2013) use both capital investments and R&D to assess physical capital deepening. For completeness, I employ both while acknowledging that R&D likely captures both labor and capital investments.
[39] Models (1) and (2) control for R&D spending at time *t* and thus incorporate innovation input. I exclude this control variable (and analogously capital expenditures at time *t*) when using R&D (Capex) as a dependent variable.

(non-process) claims contained in a firm's patents. The coefficient on *MW* is -0.0999 and -0.0950 in columns (1) through (3), significant at the 5 percent level. [40]

5.9. Changes to innovation strategy

Next, I examine whether treated firms' innovation strategy shifts in response to the increased dismissal costs associated with labor dismissal laws (i.e., "innovation hindrance" in Allen et al. 2022). The international literature suggests that when labor dismissal laws make it more difficult to adjust employment levels, firms will be discouraged from pursuing innovations with higher returns but also higher risk (Griffith and Macartney 2014; Bartelsman et al. 2016).

In Table 10, I re-estimate my main models using four alternative dependent variables (Derrien et al. 2023). To capture the riskiness of innovation activities, I use *LN_VOLCPP*, measured as the natural logarithm of the cross-sectional standard deviation of the number of forward citations per patent. The coefficient on *MW* is -0.0226 in column (1), significant at the 1 percent level. To capture the scientific value of patents, I first use *LN_USP*, measured as natural logarithm of the proportion of the firms' patents that are in both the top 20% of forward citations (most useful patents) and in the bottom 20% of backwards citations (most novel patents) relative to patents in the same year. The coefficient on *MW* is -0.0251 in column (2), significant at the 1 percent level. I also use *LN_CITEPP*, measured as the natural logarithm of one plus the average number of forward citations per patent received. The coefficient on *MW* is -0.0128 in column (3), significant at the 5 percent level. To capture the market value of patents, I use *LN_MVPP*, measured as the natural logarithm of the market value per patent, which is generated based on three-day CARs around grant date and averaged across all patents in a given year. The coefficient on *MW* is -0.0650 in column (4), significant at the 1 percent level.

[40] Process innovation is driven by manufacturing firms where capital can more easily be substituted for labor. It is less prevalent in non-manufacturing firms where capital-for-labor substitution is less feasible (Bena et al. 2022).

5.10. Confounding events, state economic conditions, unobservable differences, time trends

It is possible confounding effects of other policy changes, state economic conditions, other unobservable differences among treated and control firms, and/or time trends correlated with these unobservables are driving the results.[41] To address this, I conduct additional analyses.

First, I examine whether mini-WARN laws coincide with other state law changes that impact employees' contributions to innovation. In Panel A of Table 11, I consider wrongful discharge laws (Acharya et al. 2014), right-to-work laws (Agrawal et al. 2023), constituency statutes (Flammer and Kacperczyk 2016), nondiscrimination acts (Gao and Zhang 2017), and workplace smoking bans (Gao et al. 2020), all of which have been shown to *increase* innovation and could bias against my findings. I also examine inevitable disclosure doctrine laws (Contigiani et al. 2018), which have been shown to *decrease* innovation and could bias in favor of my findings. While unrelated to the employer-employee relationship, I also consider anti-takeover laws (Atannasov 2013) and addback statutes (Li et al. 2021), both of which have been shown to *decrease* innovation and could bias in favor of my findings. I search for concurrent changes within a three-year window (*t-1* to *t+1*) of the year in which mini-WARN laws were adopted. The only overlap is Illinois's 2005 adoption of mini-WARN laws (which coincides with 2006 adoption of non-discrimination acts and addback statutes) and New Jersey's 2007 adoption of mini-WARN laws (which coincides with 2006 adoption of workplace smoking ban laws). In Panel B of Table 11, I re-estimate models (1) and (2) excluding these states. The coefficient on *MW* is -0.0664 and -0.0835 in columns (1) and (2), significant at the 1 percent level.

[41] Adoption appears to be based on idiosyncratic state factors (Malik 2022). The California WARN law was a result of negotiations between the Governor and assembly members with little statistical analysis underlying the outcome. The New Jersey WARN law was spearheaded by then district representatives in response to the closing of Millville Dallas Airmotive Plant, which displayed approximately 400 employes and angered local constituents.

Second, mini-WARN laws could be passed when states' economic conditions warrant them, and these conditions could affect innovation. I examine whether states' decisions to adopt mini-WARN laws are related to economic conditions. I estimate a linear probability model with mini-WARN adoption as the dependent variable. *MW_ADOPT* is an indicator variable that equals 1 if a state has adopted a mini-WARN law in year *t*, and 0 otherwise. I include several measures from year *t-1*: population density, vote share for the democratic party, unemployment rate, median household income, and GDP growth. The model also includes year and state fixed effects in column (2). As shown in Panel A of Table 12, none of the state characteristics have predictive power for explaining the adoption of mini-WARN laws. In Panel B of Table 12, I re-estimate models (1) and (2) and control for these state-level characteristics. The coefficient on *MW* is -0.0345 and -0.0584 in columns (1) and (2), significant at the 5 percent level.

Third, some unobservable regional economic shocks may be associated with both the passage of mini-WARN laws and innovation. In my next test, I augment models (1) and (2) by including *MW_NEIGHBOUR*, which is an indicator variable equal to one if a neighboring state (geographically adjacent to the firm's state of headquarters as reflected in a shared state border) has adopted a mini-WARN law by year *t*, and zero otherwise. As states located in the same geographical region are subject to similar economic conditions but different mini-WARN laws, this specification better controls for unobservable regional economic shocks than the baseline specifications. In Panel C of Table 12, I re-estimate models (1) and (2) and add *MW_NEIGHBOUR*. The coefficient on *MW* is -0.0401 and -0.0686 in columns (1) and (2), significant at the 5 percent level. In contrast, the coefficient on *MW_NEIGHBOUR* is insignificant at conventional levels in both columns (1) and (2).

Fourth, to account for time trend being correlated with unobservable differences between treated and control firms, I conduct a falsification test by assuming a *pseudo-adoption* year that is 10 years prior to the adoption on mini-WARN laws in each state (Roberts and Whited 2013). *MW_PSEUDO* equals 1 if a firm's headquarter state adopts a law 10 years prior to the adoption year and all the subsequent years, and zero otherwise. In Panel D of Table 12, I re-estimate models (1) and (2) for the sample period 1990-2008 and replace *MW* with *MW_PSEUDO*. The coefficient on *MW_PSEUDO* is positive but insignificant at conventional levels.

5.11. Alternative sample composition and definitions of treated and control firms

First, I expect the treatment effect to be concentrated in geographically concentrated firms where employees are more likely to be located within the headquarter state. I use the Dou et al. (2016) definition of geographically dispersed industries (i.e., retail, wholesale, transport) to identify geographically dispersed firms (*GD)*. All other sample firms are identified as geographically concentrated firms (*GC)*. In Panel A of Table 13, I re-estimate models (1) and (2) on the two subsamples. The coefficient on *MW* is insignificant for geographically dispersed firms in columns (2) and (4). In contrast, the coefficient on *MW* is -0.0452 and -0.0645 for geographically concentrated firms in columns (1) and (3), significant at the 5 percent level.

Second, I split the sample into two periods: 2000-2009 (which captures "manufacturing's lost decade") and 2010-2018 (which captures the period immediately following manufacturing's decline). In Panel B of Table 13, I re-estimate models (1) and (2) on the two subsamples. The coefficient on *MW* is -0.0343 and -0.0521 for 2000-2009 in columns (1) and (3), significant at the 5 percent level. The coefficient on *MW* is -0.0914 and -0.2031 for 2010-2018 in columns (2) and (4), significant at the 1 percent level. Thus, while the negative effects of mini-WARN laws on corporate innovation are observed in both periods, the strength of the findings, both statistically and economically, is much higher in the latter half of the sample period.

Third, some states have laws that do not mandate, but encourage, voluntary provision of advance disclosure to employees (Malik 2022). Inclusion of these states as control states likely biases towards zero. In Panel C of Table 13, I re-estimate models (1) and (2) without including the fifteen states that encourage employers to provide voluntary advance disclosure. For some of these states, mini-WARN law adoption occurs during the 20th century, prior to my sample period: Hawaii (1987), Minnesota (1989), Tennessee (1989), Wisconsin (1991), and Connecticut (1995). The coefficient on *MW* is -0.0579 and -0.0843 in columns (1) and (2), significant at the 1 percent level. Finally, Ye (2018) considers three additional states classified as states encouraging voluntary advance disclosure provisions by Malik (2022) as states with mandatory disclosure: Wisconsin (2005), Maine (2007), and Hawaii (2011), with some of these states modifying pre-existing laws from the 20th century. In Panel D of Table 13, I re-estimate models (1) and (2) including these three states as treated states when defining *MW*. The coefficient on *MW* is -0.0380 and -0.0598 in columns (1) and (2), significant at the 5 percent level. Collectively, this evidence suggests misclassification of states as treated or control do not impact inferences.[42]

6. Conclusion

The theoretical effects of labor dismissal laws on innovation are ambiguous. Labor dismissal laws increase job security and can therefore incentivize employees to exert greater effort towards innovative activity. However, labor dismissal laws also impose constraints on employers in adjusting the workforce, potentially making it difficult for firms to innovate. I study the innovation consequences of labor dismissal laws that reduce information asymmetry between employers and employees by mandating advance disclosure of employment loss to employees.

[42] In untabulated analysis, I find similar results when removing states (i.e., New Jersey, New York, New Hampshire, and Iowa) where mini-WARN law adoption coincides with the financial crisis period (i.e., 2007-2010).

Using a difference-in-differences research design that exploits staggered adoption of mini-WARN laws across 7 U.S. states from 2003 to 2015, I find evidence that labor dismissal laws lead to lower levels of patent and citation counts. I use state-level survey and firm-level employment data to provide confirmatory evidence that the adoption of mini-WARN laws leads to an increase in labor dismissal costs as reflected in a decrease in layoffs. Cross-sectional tests indicate that the decrease is concentrated among states in which existing labor laws diminish employers' ability to hold up employees (i.e., WDL states) and allow for free movement of employees among employers (i.e., states without IDD laws or strict enforcement of non-compete agreements). The decrease is also concentrated among high-skill employees working for high-tech employers, where increased process innovation in labor-saving technologies is less feasible.

My evidence is consistent with U.S. labor regulation evolving over time to provide a minimum floor on employee welfare, resulting in diminishing marginal returns from incremental labor dismissal laws, thereby muting the positive employee incentive effects of these laws for innovation. My findings provide supporting evidence for critics of labor dismissal laws by showing that labor regulation can have negative real effects, via an accounting (i.e., mandatory disclosure) channel. The evidence, from a novel disclosure setting emphasizing labor market stakeholders, suggests that the innovation costs of mandated disclosures exceed their benefits.

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Appendix A

Variable descriptions.

Variable	Description (Compustat data items in parentheses)
<u>Dependent Variables</u>	
LN_NPAT	Natural logarithm of one plus the number of patents filed.
LN_NCITE	Natural logarithm of one plus the number of forward citations received on patents that are filed.
LN_RD	Natural logarithm of one plus R&D expenditures (xrd) scaled by book value of assets (at). *LN_RD* equals 0 if its value is missing.
LN_CAPEX	Natural logarithm of one plus capital expenditures (capx) divided by the book value of assets (at).
LN_PROCI	Natural logarithm of one plus the number of process claims contained in a firm's patents.
LN_NPROCI	Natural logarithm of one plus the number of non-process claims contained in a firm's patents.
LN_CITEPP	Natural logarithm of one plus the average number of forward citations per patent received on patents.
LN_VOLCPP	Natural logarithm of the cross-sectional standard deviation of the number of forward citations per patent received on patents.
LN_USP	Natural logarithm of the proportion of the firms' patents that are in both the top 20% of forward citations (most useful patents) and in the bottom 20% of backwards citations (most novel patents) in the same patent year.
LN_MVPP	Natural logarithm of the market value per patent, which is generated based on three-day CARs around grant date and averaged across all patents in a given year.
LN_CTL	Natural logarithm of property, plant, and equipment (ppent) divided by the total number of employees (emp).
JOLTS_LAYOFF	Natural logarithm of the number of involuntary separations for state *s* in year *t*, where an involuntary separation includes layoffs with no intent to rehire, discharges because positions were eliminated, discharges resulting from mergers, downsizing, or plant closings, firings or other discharges for cause, terminations of seasonal employees, and layoffs (suspensions from pay status) lasting or expected to last more than 7 days.
COMPUSTAT_LAYOFF	Equals 1 if the year-over-year percentage decrease in the number of employees is 5% or higher for firm *i* in year *t*, and 0 otherwise.
25_LAYOFF	Equals 1 if the year-over-year decrease in the number of employees is 25 or higher for firm *i* in year *t*, and 0 otherwise.
50_LAYOFF	Equals 1 if the year-over-year decrease in the number of employees is 50 or higher for firm *i* in year *t*, and 0 otherwise.
<u>Independent Variables</u>	
MW	*MW* equals 1 if a firm's headquarter state adopts a mini-WARN law in the adoption year and all the subsequent years. *MW* equals 0 in years prior to a firm's headquarter state adopting a mini-WARN law. *MW* also equals 0 (in all years) if a firm's headquarter state does not adopt a mini-WARN law by the end of our sample period.
CASH	Cash and short-term investments (che) divided by the book value of assets (at).
FSIZE	Natural logarithm of the book value of assets (at).
LEV	Book value of long-term debt (dltt) plus debt in current liabilities (dlc) divided by the book value of assets (at).

CAPEX	Capital expenditures (capx) divided by the book value of assets (at)
ROA	Operating income before depreciation (oibdp) divided by the book value of assets (at).
MTB	Market value of assets (market value of equity (prcc_f × csho) plus book value of assets (at) minus book value of equity (ceq) minus deferred taxes (txdb)) divided by the book value of assets (at).
FIXED	Property, plant, and equipment (ppent) divided by the book value of assets (at).
HI	Sum of squared sales (sale) based on market shares of all firms in a three-digit SIC industry in a given year.
HISQ	The squared value of *HI*.
RD	Natural logarithm of one plus R&D expenditures (xrd) divided by book value of assets (at). *RD* equals 0 if its value is missing.
Additional Variables	
HT	Equals 1 if 3-digit SIC code is 283, 357, 366, 367, 382, 384, 481, 482, 489, 737, 873 as per Kile and Phillips (2009), and 0 otherwise.
NHT	Equals 1 if 3-digit SIC code is 283, 357, 366, 367, 382, 384, 481, 482, 489, 737, 873 as per Kile and Phillips (2009), and 0 otherwise.
LS	Equals 1 if 3-digit SIC or 4-digit NAICS code is below median value of the Belo et al. (2017) labor skill measure, and 0 otherwise.
HS	Equals 1 if 3-digit SIC or 4-digit NAICS code is above median value of the Belo et al. (2017) labor skill measure, and 0 otherwise.
WDL	Equals 1 if the firm's headquarter state has adopted a good-faith and/or public policy exception to the employment-at-will doctrine as per Serfling (2016), and 0 otherwise.
NWDL	Equals 1 if firm's headquarter state has not adopted a good-faith and/or public policy exception to employment-at-will doctrine as per Serfling (2016), and 0 otherwise.
LNC	Equals 1 if firm's headquarter state has below median enforcement of non-compete agreements as per Marx et al. (2022), and 0 otherwise.
HNC	Equals 1 if firm's headquarter state has above median enforcement of non-compete agreements as per Marx et al. (2022), and 0 otherwise.
ID	Equals 1 if firm's headquarter state has adopted the inevitable disclosure doctrine as per Chen et al. (2021), and 0 otherwise.
NID	Equals 1 if firm's headquarter state has not adopted the inevitable disclosure doctrine as per Chen et al. (2021), and 0 otherwise.
GD	Equals 1 if 1-digit SIC code is 4 or 5 as per Dou et al. (2016), and 0 otherwise.
NGD	Equals 1 if 1-digit SIC code is not 4 or 5 as per Dou et al. (2016), and 0 otherwise.
POP	Natural logarithm of total population divided by land area in square miles for the state. The population density data comes from the United States Census Bureau for the 1990, 2000, and 2010 census.
VOTE	Natural logarithm of total votes cast for the Democratic Party presidential candidate divided by total votes cast for any presidential candidate for the state. The vote share data comes from the MIT Election Data Lab for presidential elections in 1996, 2000, 2004, 2008, 2012, and 2016.
UR	Natural logarithm of annual unemployment rate for the state. The unemployment rate data comes from the Bureau of Labor Statistics, annually for the years 1999-2018.

HI	Natural logarithm of annual median household income for the state. The household income data comes from the United States Census Bureau, annually for the years 1999-2018.
GDP	Annual change in gross domestic product (GDP) for the state. The GDP data comes from the Bureau of Economic Analysis, annually for the years 1999-2018.
MW_ADOPT	Equals 1 if a state with a mini-WARN law has adopted a mini-WARN law in year *t*, and 0 otherwise.
MW_PSEUDO	Equals 1 if a firm's headquarter state adopts a mini-WARN law 10 years prior to the adoption year and all the subsequent years, and 0 otherwise.
MW_NEIGHBOUR	Equals 1 if a firm's neighboring state adopts a mini-WARN law in the adoption year and all the subsequent years, and 0 otherwise.

Table 1
State-level Worker Adjustment and Retraining Notification laws ("mini-WARN laws")

State	Year
California	2003
Illinois	2005
New Jersey	2007
New York	2009
New Hampshire	2010
Iowa	2010
Vermont	2015

This table reports the year when each state adopted state-level mini-WARN laws requiring employers pursuing employment displacements (i.e., plant closures, mass layoffs, relocations) to provide advance disclosure to employees, from 2000 to 2018. The year represents the first calendar year that the mini-WARN laws were effective.

Table 2
Descriptive statistics.

Variable	*N*	Mean	Std. dev.	Median	10th	90th
NPAT	54,607	8.2933	31.7949	0.0000	0.0000	13.0000
NCITE	54,607	8.7655	33.9853	0.0000	0.0000	12.6579
MW	54,607	0.2430	0.4289	0.0000	0.0000	1.0000
CASH	54,607	0.2290	0.2474	0.1312	0.0117	0.6342
FSIZE	54,607	5.9879	2.0618	5.9323	3.3162	8.6976
LEV	54,607	0.2136	0.2200	0.1646	0.0000	0.5155
CAPEX	54,607	0.0501	0.0601	0.0304	0.0064	0.1145
ROA	54,607	0.0327	0.2549	0.0999	-0.2447	0.2218
MTB	54,607	2.1520	1.7706	1.5611	0.8967	4.0457
FIXED	54,607	0.2410	0.2311	0.1583	0.0273	0.6243
HI	54,607	0.2011	0.1684	0.1502	0.0569	0.4097
HISQ	54,607	0.0688	0.1298	0.0226	0.0032	0.1679
RD	54,607	0.0647	0.1234	0.0052	0.0000	0.1971

This table presents the descriptive statistics of variables in our primary tests. The sample period is 2000-2018. I show the mean, standard deviation, and the 10th, 50th, and 90th percentiles of the variables used in the empirical analyses. All variables are winsorized at the top and bottom 1%. Please refer to Appendix A for variable definitions.

Table 3
Baseline tests: Effects of mini-WARN laws on number of patents and number of patent citations.

		LN_NPAT		*LN_NPAT*		*LN_NCITE*		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	0.6969	<0.001	-0.2758	0.060	0.6291	<0.001	-0.2168	0.092
MW	-	-0.0619	0.004	-0.0437	0.016	-0.0833	<0.001	-0.0665	0.001
CASH	+			0.1103	0.116			0.1428	0.077
FSIZE	+			0.1415	<0.001			0.1174	<0.001
LEV	-			-0.1809	<0.001			-0.1753	<0.001
CAPEX	-			0.0201	0.850			-0.0263	0.805
ROA	+			-0.0173	0.536			-0.0364	0.371
MTB	+			0.0228	<0.001			0.0210	<0.001
FIXED	+			0.1672	0.012			0.2131	0.009
HI	+			0.1269	0.461			0.1315	0.495
HISQ	+/-			0.0412	0.847			0.1207	0.601
RD	+			0.2127	0.018			0.2017	0.057
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			85.46%		85.77%		78.98%		79.21%
N			53,832		53,832		53,832		53,832

This table presents the effects of mini-WARN laws on the number of patents filed and citations on patents filed. The dependent variable in columns (1) and (2) is *LN_NPAT*, the log of one plus the number of patents filed in year *t+1*. The dependent variable in columns (3) and (4) is *LN_NCITE*, the log of one plus the number of citations received on patents filed in year *t+1*. The key independent variable is *MW*, an indicator for firm-year observations affected by state-level mini-WARN laws. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 4
Dynamic analysis: Effects of mini-WARN laws on number of patents and number of patent citations.

		LN_NPAT		*LN_NPAT*		*LN_NCITE*		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	0.6984	<0.001	-0.2738	0.061	0.6299	<0.001	-0.2146	0.093
MW^{-2}	+/-	0.0031	0.786	0.0066	0.511	0.0214	0.318	0.0240	0.230
MW^{-1}	+/-	0.0020	0.854	0.0172	0.220	0.0080	0.582	0.0211	0.108
MW^{0}	-	-0.0243	0.023	-0.0086	0.484	-0.0226	0.114	-0.0082	0.583
MW^{+1}	-	-0.0340	0.036	-0.0150	0.430	-0.0348	0.015	-0.0182	0.207
MW^{+2}	-	-0.0449	0.113	-0.0206	0.456	-0.0706	0.005	-0.0464	0.067
MW^{3+}	-	-0.0861	0.006	-0.0586	0.026	-0.1111	0.002	-0.0855	0.004
Controls			NO		YES		NO		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			85.40%		85.77%		78.94%		79.22%
N			54,398		53,832		54,398		53,832

This table presents the dynamic effects of mini-WARN laws on the number of patents filed and citations on patents filed. The dependent variable in columns (1) and (2) is *LN_NPAT*, the log of one plus the number of patents filed in year *t+1*. The dependent variable in columns (3) and (4) is *LN_NCITE*, the log of one plus the number of citations received on patents filed in year *t+1*. The key independent variable are MW^{-2}, MW^{-1}, MW^{+0}, MW^{+1}, MW^{+2} , *and* MW^{3+} which are equal to one if the firm is headquartered in a state that will adopt mini-WARN law in two years, in one year, the current year, adopted one year ago, two years ago, and three or more years ago, and zero otherwise. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 5
Effects of mini-WARN laws on number of patents and patent citations using alternative estimator.

		LN_NPAT		*LN_NPAT*		*LN_NCITE*		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
MW^{-2}	+/-	0.0300	0.484	0.0401	0.350	0.0326	0.633	0.0239	0.721
MW^{-1}	+/-	0.0387	0.377	0.0220	0.580	0.0267	0.610	0.0266	0.619
MW^{0}	-	-0.0347	<0.001	-0.0221	0.006	-0.0468	<0.001	-0.0361	<0.001
MW^{+1}	-	-0.0505	<0.001	-0.0335	<0.001	-0.0632	<0.001	-0.0464	<0.001
MW^{+2}	-	-0.0388	<0.001	-0.0204	0.045	-0.0783	<0.001	-0.0578	<0.001
MW^{+3}	-	-0.0477	<0.001	-0.0346	0.003	-0.0646	<0.001	-0.0514	<0.001
Controls			NO		YES		NO		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
N			49,942		49,344		49,942		49,344

This table presents the effects of mini-WARN laws on the number of patents filed and citations on patents filed using an alternative estimator based on Borusyak et al. (2022). The alternative estimator estimates fixed effects among the untreated observations only, imputes untreated outcomes for treated observations, and then forms treatment-effect estimates as weighted averages over the differences between actual and imputed outcomes. The dependent variable in columns (1) and (2) is *LN_NPAT*, the log of one plus the number of patents filed in year *t+1*. The dependent variable in columns (3) and (4) is *LN_NCITE*, the log of one plus the number of citations received on patents filed in year *t+1*. The key independent variable are MW^{-2}, MW^{-1}, MW^{+0}, MW^{+1}, MW^{+2} , *and* MW^{+3} which are equal to one if the firm is headquartered in a state that will adopt mini-WARN law in two years, in one year, the current year, adopted one year ago, two years ago, and adopted three years ago, and zero otherwise. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 6

Cross-sectional tests: The role of other labor laws shaping employee and employer hold up problems.

Panel A: Effects of mini-WARN laws on number of patents and patent citations using WDL and Low NCA enforcement partition.

		WDL & LNC States		All Other States		WDL & LNC States		All Other States	
		LN_NPAT		*LN_NPAT*		*LN_NCITE*		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2894	0.263	-0.2697	0.008	-0.1556	0.498	-0.2405	0.013
MW	-	-0.1161	<0.001	-0.0225	0.414	-0.1530	<0.001	-0.0480	0.112
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			85.30%		86.26%		79.38%		79.23%
N			20,560		33,168		20,560		33,168

Panel B: Effects of mini-WARN laws on number of patents and patent citations using WDL & Non-IDD partition.

		WDL & NID States		All Other States		WDL & NID States		All Other States	
		LN_NPAT		*LN_NPAT*		*LN_NCITE*		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2940	0.223	-0.2362	0.019	-0.2291	0.223	-0.1553	0.182
MW	-	-0.0919	<0.001	-0.0194	0.486	-0.1173	<0.001	-0.0494	0.074
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			85.93%		85.91%		79.98%		78.64%
N			26,121		27,618		26,121		27,618

This table presents the effects of mini-WARN laws on number of patents filed and citations on patents filed, partitioning the sample based on other labor laws that shape employee and employer hold up problems. Panel A partitions the sample based on whether the firms are headquartered in states that have adopted the public-policy and/or good-faith wrongful discharge law (WDL) exceptions and in states with below median level of enforcement of non-compete agreements (LNC). Panel B partitions the sample based on whether the firms are headquartered in states that have adopted the public-policy and/or good-faith WDL exceptions and in states that have not adopted the inevitable disclosure doctrine (NID) law. All other states serve as a benchmark. The key independent variable in both panels is *MW*, an indicator for firm-year observations affected by state-level mini-WARN laws. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 7
Directly validating the employer constraint hypothesis.

Panel A: Effects of mini-WARN laws on number of involuntary separations (i.e., layoffs and discharges).									
		WDL & LNC States		All Other States		WDL& NID States		All Other States	
	Predicted Sign	*JOLTS_LAYOFF*		*JOLTS_LAYOFF*		*JOLTS_LAYOFF*		*JOLTS_LAYOFF*	
		Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	4.6967	0.001	3.7831	0.034	3.6851	0.036	2.9310	0.089
MW	-	-0.0446	0.055	0.0248	0.519	-0.0576	0.074	0.0390	0.350
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
State Fixed Effects			YES		YES		YES		YES
Adjusted R^2			99.24%		98.64%		99.04%		98.70%
N			403		504		522		385
Panel B: Effects of mini-WARN laws on likelihood of layoffs, measured using Compustat threshold of 5% year-over year decrease.									
		WDL & LNC States		All Other States		WDL & NID States		All Other States	
		COMPUSTAT_LAYOFF		*COMPUSTAT_LAYOFF*		*COMPUSTAT_LAYOFF*		*COMPUSTAT_LAYOFF*	
		Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.0401	0.541	-0.0167	0.747	-0.0219	0.616	-0.0531	0.456
MW	-	-0.0290	0.038	0.0251	0.098	-0.0277	<0.001	0.0372	0.017
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			17.52%		17.48%		17.42%		17.69%
N			20,561		33,164		26,119		27,617

Panel C: Effects of mini-WARN laws on likelihood of corporate layoffs, measured using state-specific employment loss thresholds.

		WDL & LNC States		WDL & NID States		WDL & LNC States		WDL & NID States	
		25_LAYOFF		*25_LAYOFF*		*50_LAYOFF*		*50_LAYOFF*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2587	0.124	-0.1719	0.006	-0.2841	<0.001	-0.2551	<0.001
MW	-	-0.0527	0.019	-0.0665	<0.001	-0.0158	0.075	-0.0139	0.076
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			21.31%		20.12%		21.12%		20.40%
N			6,943		10,558		26,044		32,678

This table presents the effects of mini-WARN laws on likelihood and number of layoffs, partitioning the sample based on other labor laws that shape employee and employer hold up problems. In Panels A and B, Column (1) partitions the sample based on whether the firms are headquartered in states that have adopted the public-policy and/or good-faith wrongful discharge law (WDL) exceptions and in states with below median level of enforcement of non-compete agreements (LNC). In Panels A and B, Column (2) partitions the sample based on whether the firms are headquartered in states that have adopted the public-policy and good-faith WDL exceptions and in states that have not adopted the inevitable disclosure doctrine (NID) law. In Panels A and B, Columns (3) and (4) includes all other states as a benchmark. In Panel C, Columns (1) and (3), all sample firms are headquartered in states that have adopted the public-policy and/or good-faith wrongful discharge law (WDL) exceptions and in states with below median level of enforcement of non-compete agreements (LNC). In Panel C, Columns (2) and (4), all sample firms are headquartered in states that have adopted the public-policy and good-faith WDL exceptions and in states that have not adopted the inevitable disclosure doctrine (NID) law. The model in Panel C is estimated six years around mini-WARN law adoption by focal states (i..e, Iowa in columns (1) and (2) and California and Vermont in columns (3) and (4)). The key independent variable in all panels is *MW*, an indicator for state-year (Panels A) or firm-year (Panels B to C) observations affected by state-level mini-WARN laws. The dependent variables all capture layoff activity and are measured based on BLS and Compustat data. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 8
Cross-sectional tests: The role of industry and labor characteristics.

Panel A: Effects of mini-WARN laws on number of patents and patent citations using industry-level technology partitions.

		High-Tech		Low-Tech		High-Tech		Low-Tech	
		LN_NPAT		*LN_NPAT*		*LN_NCITE*		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	0.0193	0.922	-0.2801	0.020	0.2131	0.406	-0.2403	0.021
MW	-	-0.0788	0.041	-0.0196	0.348	-0.1112	0.002	-0.0290	0.235
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			85.96%		85.38%		79.51%		78.64%
N			14,479		39,280		14,479		39,280

Panel B: Effects of mini-WARN laws on number of patents and patent citations using industry-level employee skill partitions.

		High-Skill		Low-Skill		High-Skill		Low-Skill	
		LN_NPAT		*LN_NPAT*		*LN_NCITE*		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2771	0.270	-0.0828	0.470	-0.1872	0.435	-0.1409	0.215
MW	-	-0.0630	0.005	0.0031	0.909	-0.0772	0.002	-0.0192	0.609
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			87.54%		89.12%		83.46%		86.77%
N			22,134		18,895		22,134		18,895

This table presents the effects of mini-WARN laws on number of patents filed and citations on patents filed, partitioning the sample based on industry and labor characteristics. Panel A partitions the sample based on whether firms belong to the high-tech (low-tech) sector. Panel B partitions the sample based on whether firms are above (or below) median industry-level labor skill. The key independent variable in all three panels is *MW*, an indicator for firm-year observations affected by state-level mini-WARN laws. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 9

Examining physical capital deepening as a response to increased employer constraints.

Panel A: Effects of mini-WARN laws on physical capital deepening.

		LN_RD		*LN_RD*		*LN_CAPEX*		*LN_CTE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	0.0851	<0.001	0.1359	<0.001	0.0377	<0.001	1.4152	<0.001
MW	+/-	-0.0010	0.308	-0.0017	0.107	0.0003	0.698	-0.0360	0.236
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			85.21%		81.00%		66.71%		92.57%
N			53,817		34,775		53,852		52,885

Panel B: Effects of mini-WARN laws on number of process and non-process claims in patents.

		LN_PROCI		*LN_NPROCI*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.4296	0.040	-0.3070	0.185
MW	-	-0.0999	0.008	-0.0950	0.019
Controls			YES		YES
Year Fixed Effects			YES		YES
Firm Fixed Effects			YES		YES
Adjusted R^2			78.50%		79.42%
N			53,851		53,851

This table examines physical capital deepening. Panel A presents the effects of mini-WARN laws on four alternative dependent variables. The dependent variable in columns (1) and (2) is *LN_RD*, the log of one plus R&D expenditures, divided by total assets (at). Column (1) assigns observations with missing values as zero while column (2) excludes observations with missing values. The dependent variable in columns (3) is *LN_CAPEX*, the log of one plus capital expenditures (capx), divided by total assets (at). The dependent variable in columns (4) is *LN_CTE*, the log of one plus property, plant, and equipment (ppent), divided by total number of employees (emp). All dependent variables are measured one year after the year in which the key independent variable *MW* is measured, Panel B presents the effects of mini-WARN laws on two alternative dependent variables. The dependent variables are *LN_PROCI (LN_NPROCI)*, the log of one plus the number of process (non-process) claims contained in a firm's patents. The key independent variable in both panels is *MW*, an indicator for firm-year observations affected by state-level mini-WARN laws. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 10
Innovation hinderance: Riskiness and return characteristics of patents.

		LN_VOLPP		*LN_USP*		*LN_CITEPP*		*LN_MVPP*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.0452	0.199	0.0099	0.763	0.0186	0.277	0.0243	0.730
MW	-	-0.0226	<0.001	-0.0251	0.005	-0.0128	0.033	-0.0650	0.007
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			64.07%		54.90%		54.90%		73.67%
N			53,833		53,833		53,817		53,833

This table examines the effects of mini-WARN laws on four alternative dependent variables that capture the riskiness, scientific value of patents, and market value of patents. The dependent variable in column (1) is *LN_VOLCPP*, the log of one plus the cross-sectional standard deviation of the number of forward citations per patent. The dependent variable in column (2) is *LN_USP*, the log of one plus the proportion of patents that are in both the top 20% of forward citations (most useful patents) and in the bottom 20% of backward citations (most novel patents) in the same patent year. The dependent variable in column (3) is *LN_CITEPP*, the log of one plus the average number of forward citations per patent. The dependent variable in column (4) is *LN_MVPP*, the log of one plus the market value per patent, which is generated based on three-day cumulative abnormal returns (CARs) around grant date. All dependent variables are measured one year after the year in which the key independent variable *MW* is measured. The key independent variable is *MW*, an indicator for firm-year observations affected by state-level mini-WARN laws. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 11
Confounding events: Contemporaneous state-level law changes impacting employer-employee relations.

Panel A: Contemporaneous (i.e., within *t-1* to *t+1)* state-level law changes impacting employer-employee relations.

Other law changes	# of concurrent mini-WARN law changes
Wrongful discharge laws	0
Right-to-work laws	0
Stakeholder (e.g., employees) orientation statutes	0
Employment nondiscrimination acts	1
Healthy work environment laws	1
Inevitable disclosure doctrine laws	0
Anti-takeover laws	0
Addback statutes	1

Panel B: Effects of mini-WARN laws on number of patents and patent citations after removing confounded states.

		LN_NPAT		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2848	0.069	-0.2041	0.141
MW	-	-0.0664	<0.001	-0.0835	<0.001
Controls			YES		YES
Year Fixed Effects			YES		YES
Firm Fixed Effects			YES		YES
Adjusted R^2			85.63%		79.20%
N			49,341		49,341

This table examines contemporaneous (i.e., within *t-1* to *t+1*) state-level law changes impacting employer-employee relations. Panel A reports the number of state-level mini-WARN laws that coincide with other changes in state laws impacting employer-employee relations. Panel B presents the effects of mini-WARN laws on the number of patents filed and citations on patents filed after removing confounded states. The dependent variable in column (1) is *LN_NPAT*, the log of one plus the number of patents filed in year *t+1*. The dependent variable in column (2) is *LN_NCITE*, the log of one plus the number of citations received on patents filed in year *t+1*. The key independent variable is *MW*, an indicator for firm-year observations affected by state-level mini-WARN laws. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 12
Endogeneity in mini-WARN law adoption and alternative specifications addressing state, region, and time confounds.

Panel A: Do state-level economic conditions and population characteristics predict the adoption of state-level mini-WARN laws?

		MW_ADOPT		*MW_ADOPT*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.0839	0.495	-1.1619	0.216
POP	+/-	-0.0001	0.970	0.0064	0.881
VOTE	+/-	0.0185	0.355	0.0074	0.855
UR	+/-	-0.0024	0.660	0.0023	0.809
HI	+/-	0.0096	0.393	0.1043	0.188
GDP	+/-	-0.0012	0.557	-0.0037	0.165
Year Fixed Effects			NO		YES
State Fixed Effects			NO		YES
R^2			0.51%		8.08%
N			792		792

Panel B: Effects of mini-WARN laws on number of patents and patent citations after including state-level control variables.

		LN_NPAT		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.0762	0.681	-0.0064	0.982
MW	-	-0.0345	0.047	-0.0584	0.006
State-Level Controls			YES		YES
Firm-Level Controls			YES		YES
Year Fixed Effects			YES		YES
Firm Fixed Effects			YES		YES
Adjusted R^2			85.79%		79.20%
N			52,651		52,651

Panel C: Effects of mini-WARN laws on number of patents and patent citations after controlling for regional economic shocks.

		LN_NPAT		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2787	0.061	-0.2151	0.103
MW	-	-0.0401	0.041	-0.0686	0.001
MW_NEIGHBOUR	+/-	0.0178	0.519	-0.0103	0.763
Controls			YES		YES
Year Fixed Effects			YES		YES
Firm Fixed Effects			YES		YES
Adjusted R^2			85.77%		79.21%
N			53,832		53,832

Panel D: Effects of mini-WARN laws on number of patents and patent citations using a pseudo-event treatment.

		LN_NPAT		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.3669	0.069	-0.2941	0.117
MW_PSEUDO	+/-	0.0349	0.358	0.0224	0.575
Controls			YES		YES
Year Fixed Effects			YES		YES
Firm Fixed Effects			YES		YES
Adjusted R^2			83.15%		80.43%
N			67,360		67,360

This table examines endogeneity and alternative specifications. Panel A presents the test on whether state-level economic and population characteristics predict a state's adoption of mini-WARN laws. The dependent variable is, *MW_ADOPT*, an indicator variable that equals 1 if a state with a mini-WARN law has adopted a mini-WARN law in year *t*, and 0 otherwise. I include the log of several state-level measures from year *t-1* in the model: *POP*, *VOTE*, *UR*, *HI*,*GDP***.** Panel B presents the effects of mini-WARN laws on the number of patents filed and citations on patents filed after controlling for *POP*, *VOTE*, *UR*, *HI*, *GDP*. Panel C presents the effects of mini-WARN laws on the number of patents filed and citations on patents filed after including an additional variable, *MW_NEIGHBOUR*, an indicator variable equal to one if a neighboring state has adopted a mini-WARN law by year *t*, and zero otherwise. Panel D present the effects of pseudo event on the number of patents filed and citations on patents filed. The pseudo-event is captured by the variable *MW_PSEUDO*, an indicator variable equal to one if a firm's headquarter state adopts a mini-WARN law 10 years prior to the actual adoption year and all subsequent years, and zero otherwise. The dependent variable in column (1) is *LN_NPAT*, the log of one plus the number of patents filed in year *t+1*. The dependent variable in column (2) is *LN_NCITE*, the log of one plus the number of citations received on patents filed in year *t+1*. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed test.

Table 13
Alternative specifications addressing sample composition and definitions of treatment and control.

Panel A: Effects of mini-WARN laws on number of patents and patent citations using geographical concentration partitions.

		Geographically Concentrated		Geographically Dispersed		Geographically Concentrated		Geographically Dispersed	
		LN_NPAT		*LN_NPAT*		*LN_NCITE*		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2493	0.103	-0.0988	0.334	-0.1856	0.182	-0.0252	0.807
MW	-	-0.0452	0.032	-0.0096	0.635	-0.0645	0.004	-0.0319	0.108
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			85.53%		85.19%		78.91%		79.61%
N			43,838		9,949		43,838		9,949

Panel B: Effects of mini-WARN laws on number of patents and patent citations using decade partitions.

		2000-2009		2010-2018		2000-2009		2010-2018	
		LN_NPAT		*LN_NPAT*		*LN_NCITE*		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2344	0.199	0.3737	<0.001	-0.1694	0.341	0.4571	<0.001
MW	-	-0.0343	0.034	-0.0914	0.001	-0.0521	0.004	-0.2031	<0.001
Controls			YES		YES		YES		YES
Year Fixed Effects			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Adjusted R^2			88.46%		89.32%		85.25%		78.79%
N			31,333		22,276		31,333		22,276

Panel C: Effects of mini-WARN laws on number of patents and patent citations after removing states with voluntary mini-WARNs.

		LN_NPAT		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2841	0.141	-0.2617	0.098
MW	-	-0.0579	0.001	-0.0843	<0.001
Controls			YES		YES
Year Fixed Effects			YES		YES
Firm Fixed Effects			YES		YES
Adjusted R^2			86.12%		79.55%
N			38,725		38,725

Panel D: Effects of mini-WARN laws on number of patents and patent citations after varying the definition of treatment.

		LN_NPAT		*LN_NCITE*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.2776	0.059	-0.2187	0.089
MW	-	-0.0380	0.035	-0.0598	0.003
Controls			YES		YES
Year Fixed Effects			YES		YES
Firm Fixed Effects			YES		YES
Adjusted R^2			85.77%		79.21%
N			53,832		53,832

This table examines alternative sample compositions and definitions of treatment and control states. Panel A partitions the sample based on whether the firms belong to industries that are geographically concentrated (dispersed). Panel B partitions the sample based on decades (2000-2009 & 2010-2018). Panel C presents the effects of mini-WARN laws on the number of patents filed and citations on patents filed after removing states that have voluntary mini-WARN laws from the sample. Panel D presents the effects of mini-WARN laws on the number of patents filed and citations on patents filed after adding Wisconsin, Maine, and Hawaii as additional treatment states when defining *MW*. The dependent variable in column (1) is *LN_NPAT*, the log of one plus the number of patents filed in year *t+1*. The dependent variable in column (2) is *LN_NCITE*, the log of one plus the number of citations received on patents filed in year *t+1*. The key independent variable is *MW*, an indicator for firm-year observations affected by state-level mini-WARN laws. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.