

License to Layoff?
Unemployment Insurance and the Moral Cost of Layoffs

Abstract

This study presents moral cost as a novel behavioral constraint on firm resource adjustment, specifically layoff decisions in response to negative economic shocks. Unemployment insurance (UI) reduces not only economic hardship for laid-off workers but also the moral cost of layoffs to managers. Using the expansions of UI benefits across US states, we show that expanding UI leads to larger layoffs in firms experiencing negative economic shocks. We find consistent evidence at the individual- and state-level using Census data. The effects are stronger for managers with greater discretion to act on moral concerns due to weaker corporate governance and for internally-promoted and non-Republican CEOs and firms that are family-owned or located in less populous zip codes that show stronger prosocial concerns.

Key words: layoffs, prosociality, moral licensing, resource adjustment, unemployment insurance

[Laying off workers] was probably the most difficult moment of my life. I think most CEOs say that was the hardest, most difficult, emotional thing to do.

- Brian Chesky, CEO of Airbnb¹

1. INTRODUCTION

Humans are social animals, and prosocial preferences and fairness norms affect all aspects of our lives (Fehr and Fischbacher, 2003). A growing body of research explores the role of prosocial preferences in the labor market and organizations (Brief and Motowidlo, 1986; Malmendier and Camerer, 2007; Fehr, Goette, and Zehnder, 2009). In particular, the idea that moral and fairness concerns constrain profit-seeking provides a critical extension to the standard competitive models of firm resource adjustment. An influential line of research examines wage rigidity (Akerloff, 1980; Kahneman et al., 1986), namely why firms do not reduce wages downward during periods of declining demand due to fairness considerations.² We examine how moral cost affects managers' layoff decisions and causes layoff rigidity and its interaction with unemployment insurance programs (UI). Managers point to laying off workers as one of the most stressful parts of their jobs (Horowitz, 2014; Knight, 2020), and we expect moral concerns and their mitigation through increases in UI benefits to play a significant role in layoff decisions which have critical consequences for employees' wellbeing.

We examine moral cost as a novel behavioral constraint on layoffs in the context of UI. Unemployment insurance programs are designed to help laid-off workers. The involuntary loss of a job not only affects workers profoundly in terms of lost income but also imposes physical and psychological costs, such as reduced food consumption (Gruber, 1997) and an increased risk of suicide (Wanberg, 2012). Most previous research investigates how UI affects the unemployed in terms of life satisfaction (Aghion *et al.*, 2016) or the efforts to look for a new job based on the premise that extended UI decreases the economic and psychological burden of an unemployment spell. This study shifts the focus from workers to managers and how expanding UI benefits affects their decisions to layoff workers in the first place.

Prior research suggests that the expansion of UI benefits, as an experience-rated tax on layoffs, could reduce layoffs (Baily, 1977; Brechling, 1981). However, expansions in social insurance programs provide a greater cushion for at-risk workers to fall back on and may

¹ <https://mastersofscale.com/wp-content/uploads/2021/03/rapid-response-transcript-brian-chesky.pdf>

² While prior research often uses these terms interchangeably, moral, fair, and prosocial decisions can diverge in some cases. For example, stealing to help the poor in your community (e.g., Robin Hood) can be prosocial but not moral. An eye for an eye punishment can be fair but not social or moral. In discussing prior studies, we adopt the term most often used in the referenced study.

morally license managers to layoff more workers. As a case in point, the *Wall Street Journal* reported that executives felt that the boost in unemployment benefits in response to the COVID-19 pandemic made layoffs “more palatable” and “without feeling they are abandoning their employees.”³

In our empirical analysis, we examine expansions in unemployment insurance benefit amounts by state governments between 1977 and 2007 (Agrawal and Matsa, 2013; Hsu, Matsa, and Melzer, 2018). Unlike UI *duration* that is extended automatically based on local economic conditions, changes in UI benefit *amounts* are determined by multiple economic and political factors, such as party preferences and logrolling within state legislatures, and states differ substantially in the timing, frequency, and magnitude of their expansions over time. For example, between our sample period of 1977 and 2007, Massachusetts made 26 increases that average \$24.80 to its maximum weekly amount, while Alabama made ten increases that average \$15.50. We combine UI expansions with negative economic shocks that require firms to layoff workers (Baily, 1977; Davis, Faberman, and Haltiwanger, 2012). In our baseline specification, we find that firms with below-industry performance layoff 3.9 percent of their workforce. A ten percent increase in UI benefits increases the layoff by 25.4 percent (or 0.99 percentage points). In contrast, we find a null effect of UI expansions on firm capital investment that does not pose any moral concerns. The falsification test helps to rule out the mismeasurement of investment opportunities as driving our results.

A key challenge to our empirical approach is that changes to the UI program may be driven by state-level economic conditions that decrease both firm performance and the demand for labor. Our firm-level analysis focuses on expansions in the maximum weekly benefit amount of UI and not its duration, but the endogeneity issue could still cast doubts on the causal interpretation of our results. However, firms operating in the same state differ in their industry, performance, and their need for layoffs over time. The incongruence in state and industry boundaries permits including firm, state-by-year, and industry-by-year fixed effects to flexibly and robustly control for state- and industry-level trends. We also obtain consistent results using industry-level shocks that are plausibly exogenous to the economic conditions of individual states, including Chinese import competition (Acemoglu *et al.*, 2016). The design closely resembles Autor, Dorn, and Hanson (2013) and Hagedorn *et al.* (2019) but takes the firm as its unit of analysis rather than regional blocks. We obtain consistent results at the state level using

³ <https://www.wsj.com/articles/companies-cite-new-government-benefits-in-cutting-workers-11586264075>.

employment data from County Business Pattern (CBP) data and at the individual level using Current Population Survey (CPS) data from the Census Bureau. We verify that the licensing effects of UI expansions are distinct from and robust to controlling for other worker-, firm-, and macro-level effects of UI considered in prior studies, especially decreased hiring and increased voluntary exits.

Next, we conduct a series of cross-sectional tests to get closer to the underlying mechanism. We expect moral costs to have a larger effect on managers who (a) have stronger prosocial preferences and (b) have the discretion to act on them. First, looking at managers and their characteristics, we find significant differences based on the personal political leanings of CEOs. Consistent with prior research that finds stronger prosocial preferences among Democratic managers (Chin et al., 2013; Di Giuli and Kostovetsky, 2014), UI expansions have a limited effect on Republican CEOs who make aggressive layoffs in response to low performance, but they license larger layoffs by Democratic CEOs who dismiss fewer workers despite low performance. We find a consistent pattern when comparing internally promoted CEOs who have spent greater time working with employees (Landier, Nair, and Wulf, 2009; Yonker, 2017) and are expected to incur a greater moral cost from layoffs relative to externally hired CEOs.

To identify managers with greater discretion to act on moral concerns, we first leverage the staggered adoptions of antitakeover laws that protect managers from disciplinary pressures of the market for corporate control (Bertrand and Mullainathan, 2003). In line with the “quiet life” hypothesis, we find that managers of protected firms refrain from costly efforts at restructuring and layoff fewer workers in response to low firm performance. However, UI expansions lead to a more aggressive restructuring in protected firms compared to non-protected firms, with a ten percent increase in UI benefits increasing the layoffs by an additional 1.42 percentage points, which fully makes up for the effect of weaker corporate governance. Second, we investigate low financial slack as an alternative check on managerial discretion to act on moral concerns (Jensen, 1986; John, Li, and Pang, 2017) and find that UI expansions have a limited effect on layoffs in financially constrained firms while increasing layoffs in unconstrained firms whose managers can better afford to act on their moral concerns. We obtain a consistent pattern using a plausibly exogenous financial constraint based on maturing long-term debt (Almeida *et al.*, 2012). Third, we find larger effects in firms that are family-owned or headquartered in a less populous zip code, which rely more heavily on implicit labor contracts and relational governance based on social ties and show lower layoff propensity (e.g.,

Landier, Nair, and Wulf, 2009; Mueller and Philippon, 2011; Feldman, Amit, and Villalonga, 2014; Kim et al., 2020).

Our study contributes primarily to two streams of research. First, we contribute to the literature on the role of managerial preferences on firm investment (e.g., Bertrand and Schoar, 2003; Malmendier and Tate, 2015). Our study presents a behavioral account of layoff decisions and how managers' prosocial preferences can lead to layoff rigidity. Our analyses shift the focus from fairness norms of employees to prosocial preferences of managers. Complementing prior research on how managers' self-serving biases can lead to both over- and under- reaction to economic shocks (e.g., overconfidence, narcissism) (e.g., Malmendier and Tate, 2008; Petrenoko et al., 2014), we show that managers' prosocial preferences reduce performance sensitivity of layoff decisions, especially in weakly governed and financially unconstrained firms. Our findings on governance strength and financial constraint provide field evidence to the experimental findings that performance pressures indeed suppress prosocial preferences (e.g., Bartling et al., 2015; Cohn et al., 2015), even with respect to high-stake decisions that involve significant physical and psychological costs to employees. Second, we contribute to the literature on UI. Our consideration of managers and their moral cost complements prior studies that predominantly focus on worker-, firm-, and macro-level effects of UI programs (e.g., Kroft and Notowidigdo, 2016). Our findings caution that expanding social insurance programs provides managers with an excuse to reduce private prosocial behaviors.⁴ UI expansion as an antecedent to layoff decisions also warns that evaluating how expanding UI helps the unemployed likely overestimates its benefits, as expanding UI benefits could have contributed to job loss in the first place.⁵

The paper proceeds as follows: Section 2 reviews related literature, Section 3 describes the data and our empirical approach, Section 4 presents the main results, and Section 5 discusses mechanisms. Section 6 discusses the implications of our findings and concludes. The Appendix provides additional empirical results, robustness checks, and dataset details.

2. MORAL COSTS AND LAYOFFS

⁴ Research in public finance shows that government grants crowd out fundraising efforts by charitable organizations (Kingma, 1989; Andreoni and Payne, 2003, 2014; Manzoor and Straub, 2005). While related, our study differs in its focus on for-profit firms; UI expansions also do not provide any monetary provisions and instead impose (indirect) costs through UI taxes.

⁵ Our focus on layoffs can be viewed as a counterpart to Hagedorn, Karahan, Manovskii, and Mitman (2019) and Johnston (2020) that examine how UI affects a firm's *hiring* decisions.

Humans are social animals that strive to act in ways that are moral, fair, and prosocial (e.g., Ariely, Bracha, and Meier, 2009; Abeler et al., 2019). In fact, many individuals are willing to incur personal costs to not hurt or even help others. These prosocial preferences have far-reaching organizational consequences, affecting employee misconduct and performance (Greenberg, 2002; Dana, Weber, and Kuang, 2007), wage setting and incentive structure (Bertrand, 2004; Cohn et al., 2015), firm scope (Nickerson and Zenger, 2008), and firm participation in corporate social responsibility (CSR) activities (Di Giuli and Kostovetsky, 2014). We shift the focus from individuals and employees to managers. A vast literature investigates how managers can be selfish and ego-centric, looking at the effects of managerial overconfidence, narcissism, and self-interest on firm investment decisions (e.g., Malmendier and Tate, 2005; Petrenoko et al., 2014; Zhu and Chen, 2015). We examine how managers' other-regarding preferences affect layoff decisions. Managers are responsible for employees and their livelihood, the financial interests of shareholders, and their own career and self-interest, facing a complex and intense moral tradeoff relative to individuals engaged in short-term exchanges.

Prior research on the role of prosociality in labor market dynamics highlights how it can cause downward rigidity in setting wages and increase unemployment. Managers do not cut wages even during periods of high unemployment or decreasing demand because employees may perceive the cut as being unfair and reduce efforts, offsetting the potential cost savings (e.g., Akerlof and Yellen, 1990; Cohn et al., 2015). We expect prosocial preferences to play a critical role in layoff decisions as well. Workers enter employment relations based on friendship networks, social norms, and unwritten understandings (e.g., Bertrand, 2004; Bidwell, Briscoe, Fernandez-Mateo, and Sterling, 2013; Sterling, 2014; Frydlinger, Hart, and Vitasek, 2019), and one-third of employees work without a contract referencing discharge laws (Bird, 2005). This relational nature of employment contracts creates significant room for informal moral obligations to affect layoff decisions.

We expect the moral cost of layoffs to reduce the performance sensitivity of layoff decisions, specifically reducing the layoffs in response to negative economic shocks. Losing a job often leads to dire economic consequences for the average American worker who cannot cover an unexpected \$400 expense, and managers seem to internalize the costs for workers when making their layoff decisions.⁶ Frontline managers who implement layoffs experience

⁶ <https://www.federalreserve.gov/publications/files/2018-report-economic-well-being-us-households-201905.pdf>

higher incidences of sleep and health-related problems (e.g., ulcers) with emotional exhaustion as the underlying mechanism (Grunberg et al., 2006) and try to increase interpersonal distance prior to making layoffs (Folger and Skarlicki, 1998). Not surprisingly, executives often cite layoff decisions as the most difficult decision in their job (Horowitz, 2014; Knight, 2020), indicating that they care about their workers and want to avoid harming them.

In contrast to the extensive experimental evidence at the individual level, examining the role of moral cost and prosocial concerns in managerial decision-making has remained elusive. CEOs rarely answer questions about their social preferences (Cycyota and Harrison, 2006), leaving researchers to infer them indirectly based on publicly observable events and actions, such as the birth of a male or female child (Dahl, Dezso, and Ross, 2012) or political donations (Chin et al., 2013). Recent advances using field experiments (e.g., Fehr and List, 2004; Cohn et al., 2015), while offering rigorous causal analysis, focus on individuals rather than managers and abstract away from the economic, social, and shareholder pressures that serve as a critical context under which managers develop and act on informal moral obligations to their workers. As a natural experiment that reduces managers' moral concerns for workers, we examine expansions in social insurance programs, specifically unemployment insurance.

UI expansion reduces economic hardship for laid-off workers and allows us to examine how managers react when the moral cost is externally reduced. Our argument also finds a robust theoretical foundation in psychology research on moral licensing (Bandura et al., 1996; Effron and Conway, 2015).⁷ Building on the earlier work on intra-person substitution, a growing body of research indicates vicarious moral licensing whereby people use not only their own good deeds but those of proximate others to justify helping less or cheating more (e.g., Kouchaki, 2011; Gino and Galinsky, 2012; Newman and Brucks, 2018). Expansion in UI, which is partly funded by the firm, provides a highly salient justification (or an excuse) to relegate their moral concerns and lay off more workers.⁸

We expect the significance of moral costs to differ based on (a) the strength of managers' prosocial preferences and (b) their discretion to act on them. For example, Republican-leaning CEOs spend substantially less on CSR activities relative to Democratic

⁷ Akerlof and Yellen (1990) draw from research on social psychology and sociology to motivate their model where workers have a fair reference wage in mind.

⁸ Hamermesh (1996) estimates that firms paid approximately 50% of unemployment benefits during the 70s and 80s. Johnston (2012) estimates that the Tax Equity and Fiscal Responsibility Act of 1982 increased this share to 87% for Florida firms.

CEOs. Prior research on self-serving managerial characteristics and agency conflicts finds that strong governance and monitoring by external stakeholders reduce managerial discretion to act on their preferences and, in turn, the effects on firm decisions (e.g., Malmendier and Tate, 2005; Giroud and Mueller, 2010; Zhu and Chen, 2014; Gormley and Matsa, 2016; Yonker, 2017). In Section 5, we explore the effects of CEO prosociality and the strengths of corporate governance.

3. DATA AND METHODS

3.1. Unemployment Insurance Program

The unemployment insurance program provides short-term cash provisions to workers who have become involuntarily unemployed, for example, due to plant closures or declining demand from increased import competition. As a joint program between the federal and state governments, the federal government mandates the broad program features and ensures consistency across states, while individual states are responsible for setting three key policy parameters: eligibility, benefit duration, and weekly benefit amount.

Weekly benefit amounts are set to make up for approximately half of the full-time weekly wage, subject to minimum and maximum bounds. We obtain information on the maximum weekly benefit and duration for each state from the United States Department of Labor's publication "Significant Provisions of State UI Laws." We focus on changes in the maximum potential benefits, calculated as the product of the maximum benefit amount and the maximum duration, to be consistent with Agrawal and Matsa (2013) and Hsu, Matsa, and Melzer (2018). Using only the changes in maximum weekly benefit amount, as in Kroft and Notowidigdo (2016), yields consistent and sharper results (Appendix Section E.1). Most states set the regular maximum duration to 26 weeks prior to 2008 (93.35% of our state-year observations), and changes in UI benefits are predominantly driven by increases to the maximum weekly benefit amount. States, on average, expand unemployment insurance by 4.3 percent per year but differ significantly in the frequency and magnitude of their expansions (see Panel A in Table 1 for summary statistics). The minimum and maximum values for ΔUI are large (-0.52, -0.65) due to a few state-years with very large increases in UI amounts, for example, Arkansas increasing from \$120 to \$222 in 1981 or New York increasing from \$180 to \$245 in 1990. We verify that all of our results are robust to their exclusion.

UI programs are funded by federal and state UI payroll taxes, which show significant differences across firms, typically ranging between 0.6 to 6.0 percent. A higher UI payroll tax

rate is applied to firms that laid off more workers. The ‘reserve ratio’ used to determine the firm-specific UI tax rate is defined as the difference between tax credits collected and benefits paid, divided by the employer’s average covered payroll in the past three to five years, subject to minimum and maximum rates.⁹ Such experience-based rating should work against our argument; by increasing the benefits paid to laid-off workers and decreasing the reserve ratio, UI expansions act as a de-facto increase in the marginal UI tax rate and, in turn, the cost of layoffs (Brechling, 1981; Anderson and Meyer, 1993).

3.2. Sample

For our baseline sample, we start with the universe of public firms in the Compustat database between 1977 and 2007. We restrict the sample window to 2007 because the Federal Emergency Unemployment Compensation (EUC) program in response to the 2008 financial crisis extended the maximum duration of benefits to 99 weeks for some states, preventing meaningful comparison with prior periods. To reduce extreme values of employment changes and frequent delisting, we require that firms employ at least 500 workers for two consecutive years, a cutoff line to qualify for small business firms. We show below that our results are robust to including recent years (2007-2017) or small firms. In supplementary analyses, we use the County Business Pattern (CBP) and Current Population Survey (CPS) databases from the Census Bureau and examine whether we observe a consistent pattern in state- and individual-level employment. Refer to Panel A of Table 1 for a summary of state-level UI sample statistics.

3.3. Empirical Specification

To test whether UI expansions lead to larger layoffs, we estimate the following two OLS regressions:

$$\Delta Emp_{iskt} = \alpha_i + \alpha_{st} + \alpha_{kt} + \beta_1 \Delta UI_{st} + \beta_2 Low\ firm\ performance_{it} + \beta_3 \Delta UI_{st} \times Low\ firm\ performance_{it} + X_{iskt} + \varepsilon_{iskt}, \quad (1)$$

$$\Delta Emp_{iskt} = \alpha_i + \alpha_{st} + \beta_1 \Delta UI_{st} + \beta_2 Negative\ industry\ shock_{kt} + \beta_3 \Delta UI_{st} \times Negative\ industry\ shock_{kt} + X_{iskt} + \varepsilon_{iskt}, \quad (2)$$

where i indexes firm, s indexes the state of a firm’s primary operation, k indexes a firm’s primary industry of operation, and t indexes year. α_i , α_{st} , and α_{kt} are firm, state-by-year, and industry-by-year fixed effects; X is a vector of firm, state, and industry controls; and ε is an idiosyncratic error. ΔUI_{st} captures UI expansion in a firm’s primary state of operation s at

$$\frac{emp_t - emp_{t-1}}{0.5 \times emp_t + 0.5 \times emp_{t-1}}$$

⁹ See <https://www.bls.gov/opub/mlr/2020/article/the-cost-of-layoffs-in-ui-taxes.htm>. Twenty states use benefit-ratio or benefit wage-ratio instead of the reserve-ratio approach, but the overall design scheme of experience-rating, subject to maximum bounds, is the same (Pavosevich, 2020).

year t .^{10,11} ΔEmp is the employment growth rate measured as $\frac{Emp_t - Emp_{t-1}}{Emp_{t-1}}$, which captures a net change in firm employment that includes both layoffs and hiring, an issue we address below.¹² Our sample firms, on average, increase their workforce by 4.2 percent per year. Refer to Panel B of Table 1 for a summary of firm-level sample statistics.

----- Insert Table 1 about here -----

β_2 estimates layoffs in response to firm-level low performance in equation (1) and industry-level negative shocks in equation (2) in the absence of any changes to UI benefits. We expect UI expansions to affect managers when they must actively layoff workers while having a limited effect when firms are actively expanding (Feldstein, 1976; Baily, 1977). Our specification examining the conditional effect of ΔUI in response to negative shocks draws a close parallel to fairness research, which examines (un)responsiveness in wages to adverse labor market conditions, such as high unemployment rate or falling demand (Kahneman et al. 1986; Akerlof and Yellen, 1990; Bertrand, 2004), and firm growth dynamics literature, which examines firm downsizing and exits during recessions (Hopenhayn, 1992).

As a primary measure of a firm-level negative shock, we use low firm performance based on return on assets (ROA) in equation (1). *Low firm performance* is a binary variable set to one if a firm's realized performance falls below the industry benchmark, calculated as the median ROA of a firm's primary four-digit SIC industry for each fiscal year.¹³ The blunt binary measure is intuitive and insensitive to potential noise or functional forms of a firm's need for layoffs. We obtain consistent results using alternative operationalizations of low-firm performance, for example, replacing the binary *Low firm performance* with a continuous variable using a linear spline or basing performance on negative income (refer to Appendix Section E.1 and E.4). In estimating equation (2), we examine three industry-level negative shocks that are unlikely to be influenced by the economic conditions of individual states while requiring layoffs from firms operating in affected industries: Chinese import competition, decline in industry revenue growth, and decline in industry value-add, further detailed in Section 4.3.

¹⁰ ΔUI_{st} is subsumed and dropped from the estimation when we include state-by-year fixed effects (α_{st}).

¹¹ To identify the effective UI expansions, we assign firms to their state of firm headquarters, as reported in Compustat (Agrawal and Matsa, 2013). To the extent that firms maintain employment across multiple states, ΔUI is measured with noise, likely resulting in a downward bias. We find a stronger positive relation between UI expansions and layoffs in firms that have a higher share of operations in the state of their headquarters (see Appendix Section E.5).

¹² Our results are robust to calculating employment changes as a year-to-year log difference.

¹³ We exclude firms with ROA less than 100 percent and greater than -100 percent. All of the results are unaffected by their inclusion.

Our main variable of interest is the coefficient for the interaction term β_3 , which estimates whether the intensity of layoffs varies with UI expansions. We predict β_3 to be negative because increases in the UI benefit amount license laying off additional workers who would have been retained in the absence of UI expansion. UI reduces the managerial cost of layoffs, specifically its moral component, and increases the sensitivity of layoffs to negative shocks.

Whether moral licensing should depend on the nominal UI amount or its expansion (Δ UI) relates to a long-standing debate. Managers may focus on whether the overall UI amount is sufficient to cushion economic hardship (e.g., \$862 in Massachusetts in 2007 *versus* \$133 in Mississippi per week in 2007) or there is an increase providing an improvement relative to the condition at the time of hiring or recent past years. Prior UI research has focused on the nominal UI amount in assessing the consequences of UI's financial costs to the firm and benefits to the workers (e.g., Dou, Khan, and Zhou, 2016; Flammer and Luo, 2017; Johnston, 2021). In contrast, behavioral research on fairness emphasizes reference dependence and the resulting focus on change as one of its defining features (Fehr, Goette, and Zehnder, 2009). People have difficulty judging what is “fair” or “moral” in a vacuum and rely on deviations from what Kahneman et al. (1986) term “reference transaction.” In their telephone surveys, Kahneman et al. (1986) find that 83% of the respondents judge it unfair to decrease the wage of an existing employee from \$9 to \$7 an hour during times of high unemployment, but 73% judge it fair to hire a new employee at \$7 an hour. In other words, a fair wage depends less on its nominal amount and more on whether it is higher or lower relative to a reference amount (i.e., change). Similarly, because the perception of fair wages strongly depends on minimum wage, increasing and decreasing the minimum wage, for example, from \$7 to \$9 then back to \$7, can decrease the labor supply compared to the no change condition in the short term. This reference dependence provides key extensions to standard competitive models of the labor market, for example, in explaining wage rigidity, cohort effects, and asymmetric effects of minimum wage changes. In the same vein, moral licensing research points to recent positive and salient acts that increase credit in one's “moral bank” as an excuse to help less or cheat more.

Our baseline specification uses Δ UI, but we explore both UI change and amount below. In particular, we examine UI amount as a critical contextual variable that determines how much UI expansions lead to moral licensing. Anderson et al. (2011) show that the size of the prize affects the resource allocation in ultimatum games. Participants offer and accept a lower share of the overall stake as its nominal amount increases, indicating that the demand curve for

fairness slopes downward. We expect UI expansions to provide managers with stronger justification to disengage from informal moral obligations under high levels of UI while providing only a weak excuse when the UI amount is patently low (e.g., below living wage).

Identifying layoffs is complicated by the fact that changes in firm-level employment consist of three components: involuntary layoffs, voluntary exits (‘quits’), and new hires. Davis, Faberman, and Haltiwanger (2012) document that both new hires and voluntary exits quickly decline during industry downturns, especially at contracting establishments. This leaves involuntary layoffs to dominate the net decrease in employment in response to industry-level negative shocks. The redress is less effective for equation (1). Low firm performance decreases new hires but may also increase voluntary exits (“sinking ship” effect) (Brown and Matsa, 2015). However, the coefficient for the interaction term $\Delta UI \times \text{Negative shock}$ (β_3) captures additional layoffs that stem from expansions in UI benefits, which are limited to the involuntary loss of a job in the US (Solon, 1984). This requirement minimizes voluntary exits and helps β_3 to capture involuntary layoffs. We use Current Population Survey data to address the concern that the negative values of β_3 may be driven by reduced hiring and voluntary exits rather than increased layoffs in Section 4.5. Throughout the paper, we refer to ‘layoffs’ when presenting results based on decreases in firm-level employment.

The vector X includes an extensive set of firm- and industry-level control variables that influence the demand for labor.¹⁴ We control for industry revenue growth, calculated as the mean growth rate of all public firms in the same 4-digit SIC code, Tobin’s Q as a proxy for firm-specific growth opportunities reflected in a firm’s stock price, firm size (log of total assets), and industry concentration (the Herfindahl index and its square) based on revenue of Compustat firms. Agrawal and Matsa (2013) show that unemployment benefits increase firm leverage by reducing workers’ exposure to unemployment risk. Because low financial slack might reduce firms’ ability to retain workers, we include four proxies of a firm’s financial resources: leverage based on its debt ratio, current ratio, working capital-to-sales ratio, and Altman’s Z-score. All standard errors are double-clustered at the firm and state level to correct for potential cross-sectional and serial correlation in the error term (Petersen, 2009).¹⁵

3.4. Endogeneity Concerns

¹⁴ All control variables are from $t+0$ but using values from $t-1$ or omitting all control variables other than firm and industry-by-year fixed effects yields consistent results.

¹⁵ We find very little difference when we instead bootstrap standard errors with two-way clustering at the firm- and year-level using the algorithm from Gow, Ormazabal, and Taylor (2010).

UI expansions are thought to be influenced by local economic and political conditions, including the unemployment rate, incumbent officials' reelection prospects, and Republican control of state legislatures. The potential role of a state's economic conditions raises concerns of omitted variable bias, as they likely reduce the demand for labor, especially among firms experiencing low performance. Our empirical specification in equation (1), however, provides several assurances against this concern.

First, our firm-level analysis controls for industry-by-year and state-by-year fixed effects in addition to the usual firm-fixed effects. A firm's primary industry of operation (indexed with k) differs from a firm's primary state of operation (indexed with s), and firms operating in the same state and industry show significant differences in their performance (or *Negative shock*) and the need for layoffs over time. For example, California as a whole may be experiencing a recession while most (but not all) of its high-tech firms experience fast-growing demand; the oil and gas sector may be contracting even as Texas as a whole shows strong economic growth. The incongruence in state and industry boundaries, combined with variations in firm performance over time, allows for estimating β_3 (but not β_1) while controlling for firm, industry-by-year, and state-by-year fixed effects. This means that any variation in layoffs that stems from unobserved common shocks affecting all firms in a given state or industry in a given year is not used to identify β_3 .

Second, prior studies that use UI expansions for causal identification conduct a battery of checks for their exogeneity. After controlling for state and year fixed effects, they find a small and statistically insignificant relation between UI benefits and various state-level economic indicators, including state unemployment rates, GDP per capita, UI payroll taxes, and other government transfer programs (Kroft and Notowidigdo, 2016; Hsu, Matsa, and Melzer, 2018). At the firm level, Agrawal and Matsa (2013) do not detect any significant association between UI benefits and firms' profits or operating performance. In Appendix A.1, we conduct additional firm-level tests and show that UI expansions are not a function of in-state firms' financial performance or investment activities, including revenue and profit growth, R&D spending, and inventory level. Moreover, the share of firms with below-industry performance is distributed evenly across states, as shown in Appendix Figure A, which further mitigates the concern that state governments systematically undertake UI expansions in efforts to bail out employees of particular firms or industries. In other words, the coefficient for the interaction term $\Delta UI \times \text{Negative shock}$ (β_3) estimates whether UI expansions that happen to coincide with low firm performance license larger layoffs.

When we use industry-level negative shocks, our identification strategy closely resembles Autor, Dorn, and Hanson (2013), who look at the interaction between Chinese import penetration and cross-regional differences in exposure to these shocks based on initial industry composition. Instead of regional differences, we interact firm-level differences in exposure to these shocks based on a firm's primary industry of operation and, in turn, the need for layoffs.¹⁶ The key identifying assumption is that the two interacted variables – industry-level shocks and state-level UI expansions – are not systemically related (Goldsmith-Pinkham, Sorkin, and Swift, 2000). In other words, the coefficient for the interaction term $\Delta UI \times \text{Negative shock}$ should estimate how state-level UI expansions that happen to coincide with negative industry-level shocks affect layoffs. We present evidence that there is no significant association between the negative economic shocks used and the state-level UI expansions in Appendix A.2. We also test sensitivity to excluding locally important industries that represent a significant share (e.g., >3%) of the state's economy and hence may influence a state's spending in social insurance programs.¹⁷

Third, our firm-level analysis extends recent studies that take advantage of spatial variations across contiguous counties or commuting zones (e.g., Hagedorn *et al.*, 2019). In addition to state-level policy differences, we take advantage of variations in managerial and firm-level characteristics, for example, CEO personal history and exogenous shocks to a firm's corporate governance or financial constraints. These characteristics measure differences in managers' prosocial preferences and their discretion to act on them without being correlated with potential alternative mechanisms and provide important mechanism and robustness tests.

4. RESULTS

We present the results of the field study in four steps: first, we show that UI expansions lead to larger layoffs in firms with low performance. Second, we use Chinese import competition and other industry-level shocks as alternative shocks that require layoffs. Third, we provide a state- and individual-level analysis. Fourth, we conduct additional robustness checks.

4.1. Layoffs in Response to Low Firm Performance

We begin by estimating a version of equation (1) that only includes ΔUI in column 1 of Table 2. Firms layoff 0.27 percent of their workforce in response to a ten percent increase in UI

¹⁶ In the context of UI, Kroft and Notowidigdo (2016) adopt a related approach that uses variations in the interaction of state-level unemployment rates with UI benefits over time.

¹⁷ These industries for example are SIC 3570 (Computer & Office Equipment) for California, SIC 1311 (Crude Petroleum and Natural Gas) for Texas, and SIC 6020 (Commercial Banks) for Massachusetts.

benefits ($p=0.062$). This weak average effect, however, obscures the contingent effect of UI expansions on layoffs, which should primarily affect firms when they need to layoff workers. In column 2, which only includes *Low firm performance*, firms reduce their workforce by 4.4 percent when their performance falls below the industry benchmark. Including ΔUI and *Low Performance* simultaneously makes little difference to the respective coefficients in column 3. However, in column 4, which includes their interaction, ΔUI no longer increases layoffs independently but through its interaction with *Low firm performance*. The coefficient for $\Delta UI \times Low\ firm\ performance$ is -0.099 ($p<0.01$), suggesting that a ten percent increase in UI increases layoffs by 0.99 percentage points. In marginal terms, firms increase layoffs by 25.4 percent compared to years when UI benefits remain flat. Column 5 replaces industry-by-year fixed effects with state-by-year fixed effects, and ΔUI is subsumed and dropped from the estimation. There is little change in the statistical and economic significance of *Low firm Performance* and $\Delta UI \times Low\ firm\ performance$. Column 6 includes both industry-by-year and state-by-year fixed effects, and we again find a consistent pattern where UI expansion increases layoffs in response to low performance. The insensitivity of the coefficients for $\Delta UI \times Low\ firm\ performance$ to these additional fixed effects is consistent with the low correlation between UI expansions and individual firm performance. We also show in Appendix B a lack of significant pre-trends; the increased layoffs are driven by contemporaneous UI expansions and not by past expansions.

In columns 7 and 8, as a falsification test, we examine whether UI expansions affect firm capital investment intensity (capital investment normalized by total assets with a one-year lag) that do not pose moral concerns. As expected, firms reduce capital investment in response to low performance, but the coefficients for both ΔUI and $\Delta UI \times Low\ firm\ performance$ are small and insignificant. The null result helps to rule out mismeasurement of investment opportunities that may be correlated with low firm performance or UI expansions as driving our results on layoffs. If that were the case, then the coefficient for $\Delta UI \times Low\ firm\ performance$ should be negative and significant also with respect to capital investment.

----- Insert Table 2 about here -----

4.2. High- versus Low-Benefit States

If the increased layoffs indeed stem from providing dismissed workers with an economic safety net, we expect the licensing effect of UI expansions to be stronger in high-benefit states and attenuated in low-benefit states where the expansions do not meaningfully reduce the economic hardship experienced by the dismissed worker. In Table 3, we repeat the earlier analysis from

Table 2 but divide our sample into high- and low-benefit states based on the median value of the maximum weekly benefit amount for each year.¹⁸ Because of sporadic and discrete expansions to maximum weekly benefit amounts, thirty-one states switch positions as a low- or high-benefit state during our sample window, reducing the concern that our results may be driven by a few states. For example, California was a high-benefit state from 1972 to 1977, became a low-benefit state from 1978 to 2001, and went back to being a high-benefit state from 2002 to 2018.

In columns 1 and 5, we observe little difference in the coefficient of *Low Performance* across high- and low-benefit states (-0.039 versus -0.044). Looking across columns 2 and 6, however, the increased layoffs from UI expansions are driven entirely by high-benefit states with a null effect in low-benefit states. A ten percent increase in UI increases layoffs by 0.42 percentage points in high-benefit states ($p < 0.01$) but only by 0.04 percentage points in low-benefit states ($p = 0.86$). As before, ΔUI is no longer independently significant with the inclusion of *Low Performance* and its interaction with ΔUI in columns 3-4 and 7-8. $\Delta UI \times Low Performance$ is negative and statistically significant only for high-benefit states. In other words, the licensing effect is absent in low-benefit states where expansions are unlikely to provide substantive economic relief to laid-off workers.

The analysis also addresses whether the licensing effect should depend on the nominal *amount* (i.e., UI) or *change* in UI benefits (i.e., ΔUI). We expect moral licensing to depend on the increase in benefit amount relative to the past, in line with prior research on fairness assessment in wages and psychology research on moral licensing. However, we also find that the absolute level of cash provisions for laid-off workers serves as a critical contextual variable. We do not find any effects at low levels of UI where UI does not provide meaningful relief and, in turn, an excuse to disengage from moral concerns. In Appendix D, we repeat all of our analyses using UI amount instead of ΔUI and find weak results.

----- Insert Table 3 about here -----

4.3. Layoffs in Response to Industry-Level Negative Shocks

In the previous analysis, we used firm-level low performance to identify the need for layoffs. In this section, we use three industry-level negative shocks that decrease demand for labor for

¹⁸ A related stream of empirical research on UI generosity investigates the wage-replacement ratio as a variable of interest, defined as the ratio of the weekly UI benefit to weekly wages. In our empirical context, the replacement ratio shows little variation across states and over time (Agrawal and Matsa, 2013). Moreover, the wage data is available for less than 20 percent of the sample firms, and the large within-firm variance in wage limits a meaningful estimation of the replacement ratio for our firm-level analysis.

firms operating in affected industries. A growing body of empirical evidence documents that import competition from China is responsible for a significant share of the decline in US manufacturing employment. We expect firms to make larger layoffs from the increased import competition in state-years in which UI benefits were expanded. In Table 4, we follow Acemoglu *et al.* (2016) and calculate Chinese import competition (ΔIP) for each four-digit SIC code as below but do it for each year rather than for 5-year intervals. $\Delta \text{Chinese import}_{kt}$ is the change in imports from China from the previous year, and the denominator is initial absorption measured as the sum of US industry shipments and Chinese imports minus US exports.

(2)

Using this industry-level shock helps to reduce endogeneity concerns related to state-level trends and generalize our results beyond firms with low performance, but one important limitation is that we are unable to include industry-by-year fixed effects. The import competition data is available from 1991, and our sample period covers 1991 to 2007. In column 1, we find that a ten percent increase in import penetration reduces firm employment by 0.24 points ($p < 0.01$), replicating Acemoglu *et al.* (2016)'s industry-level result at the firm-level. In column 2, the coefficient for the interaction between ΔIP and ΔUI is negative and significant, indicating that firms undertake larger layoffs in response to increasing Chinese import competition when states expand UI benefits. The inclusion of state-by-year fixed effects in column 3 makes little difference to the results.

We next use slowing industry growth as an alternative negative economic shock that requires firms to reduce their workforce. In columns 4 through 6, we use changes in total value-add from the NBER-CES Manufacturing Industry Database. *Negative shock* is set to one if total value-add declines relative to the previous year for each four-digit SIC code. In columns 7 through 9, we use industry revenue growth, calculated as the mean revenue growth rate of public firms in Compustat for each four-digit SIC code.¹⁹ Across both proxies, we obtain results that are in line with the earlier results from Table 2. Negative economic shocks decrease firm employment by 1.9-3.0 percent, and a ten percent increase in UI increases layoffs by 0.61-1.08 percentage points. The effect sizes are in line with but moderately smaller than those observed in Table 2 based on firm-level negative performance.

¹⁹ We require that the industry has at least ten active firms.

As a robustness check, we verify that there is little difference from excluding industries that represent more than two, three, or five percent of the state’s economy and may influence UI and other economic policies in Appendix E.2.

----- Insert Table 4 about here -----

4.4. State-level Analysis

We examine in Table 5 whether our firm-level results hold at the more aggregate state-level of analysis. We use the County Business Pattern (CBP) database from the Census Bureau that provides employment and establishment data for each state by four-digit SIC industry codes (Eckert *et al.*, 2020). CBP’s employment data includes private establishments, and the total reported number of employees is two to three times larger than the Compustat database that only includes employment by public firms. While providing more comprehensive coverage, a key limitation of CBP data is that it does not allow observing establishment- (or firm-) specific negative shocks, and we rely on more blunt, industry-level negative economic shocks to identify the demand for layoffs.

Our CBP sample starts in 1977 and ends in 1997, the last year in which data is available in SIC classification. The sample window limits the analysis using Chinese import competition, which is available only from 1991. As in Table 4 columns 4-6, we use value-add data from NBER-CES Manufacturing Industry Database as a proxy for changes in industry-level demand and set *Negative industry shock* to one if the value-add declines relative to the previous year for each four-digit SIC code. We observe an empirical pattern consistent with that in Table 2 and Table 4. In column 5, declining industry demand is associated with a 2.4 percent decrease in state-industry employment. The magnitude is closely in line with that observed for public firms using the same proxy (2.6 percent, column 6 of Table 4) but smaller than that using firm-level low performance (3.7 percent, column 5 of Table 2). A ten percent increase in UI increases layoffs by 1.45 percentage points in column 4, and including state-by-year fixed effects in column 5 yields negligible change to the result. These results help to generalize our results to private firms beyond the public firms in the Compustat database.

Inferring the effects of UI expansions on aggregate state-level employment requires much caution and is well beyond the scope of this study.²⁰ However, tentatively, we note that both firm- and state-level estimates consistently point to a mid-sized effect. Expansion in UI

²⁰ See Kroft and Notowidigdo (2016), Johnston and Mas (2018), Chodorow-Reich, Coglianesi, and Karabarbounis (2019), and Hagedorn *et al.* (2019).

benefits by one standard deviation (6.2 percent) increases layoffs by 0.61 to 0.90 percentage points in firms experiencing negative economic shocks. When extrapolated to the full sample that includes firms and industries that are growing, the average effect falls in the range of 0.31 to 0.45 percentage points.

----- Insert Table 5 about here -----

4.5. Alternative Mechanism: Reduced Hiring

As a potential alternative mechanism, we examine the possibility that decreased hiring drives our findings rather than increased layoffs. Standard models of UI benefits predict that UI expansions lower a worker’s search intensity by decreasing the relative value of finding a new job (“micro”-effect). UI expansions may also reduce vacancy creations by increasing workers’ reservation wage and in turn, the equilibrium wage (“macro”-effect) (e.g., Acemoglu and Shimer, 1999; Chetty, 2008; for a review, see Schmieder and von Wachter, 2016). Both effects slow hiring and offer an alternative mechanism to our findings, especially as we are constrained to measuring layoffs as a net decrease in firm-level employment that combines decreased hiring and increased layoffs. Providing empirical support to the hiring channel, Hagedorn *et al.* (2019) exploit a sudden decrease in UI benefit *duration* in Missouri and find an increase in vacancy postings relative to contiguous counties in neighboring states.²¹ Johnston (2021) finds a consistent result using full population records from Florida in a research design that leverages a kink in the tax schedule.²² However, the interaction term $\Delta UI \times \text{Negative industry shock}$ captures additional layoffs (or reduction in the number of employees) from UI expansions in response to specific and time-varying negative economic shocks rather than a general decrease in hiring, as examined in previous studies. A decrease of 3.7 to 4.4 percent observed in Table 2 is difficult to achieve from natural turnover and slowed hiring and requires active layoffs. We present individual- and firm-level evidence against slowed hiring and voluntary exits as driving our results below.

In Table 6, we use Current Population Survey (CPS) data from 1979 to 2007. CPS is a monthly survey of approximately 60,000 US households by the Census Bureau collecting detailed information on the employment status of each member of the household, including industry, the location of employment, and whether a job loss is voluntarily or temporary

²¹ In contrast, Boone et al. (2019) do not find any significant effect on aggregate employment from expanding maximum UI benefit duration during the Great Recession using a similar empirical approach.

²² Using a modified version of equation (1), we also find some suggestive firm-level evidence in our sample that higher UI amounts slow employment growth in a subset of firms that are actively expanding their workforce (Appendix D).

(refer to Data Appendix for more detailed information on sample construction). As industry-level negative shocks that prompt layoffs, we again use Chinese import competition and decline in industry value add (Table 4).²³ We use the concordance between the 1987 SIC code and the 1990 Census Industry Classification to link these industry-level shocks to individual-level employment data. The overall empirical design closely follows Bertrand (2004), who examines how import competition erodes informal wage agreements and relaxes downward wage rigidity during periods of high unemployment.

We estimate a linear probability model where the dependent variable *Layoff* is set to one if an individual’s employment status switches from employed to “on indefinite layoff.” We find an empirical pattern consistent with the earlier firm- and state-level results. The coefficient for *Negative shock* is positive and significant, indicating a higher probability of losing a job. The coefficient for $\Delta UI \times \textit{Negative shock}$ is also positive and significant, indicating that UI expansions increase layoffs in state-years that experience negative economic shocks. The coefficient for ΔUI is negative but significant only in column 7. These results help to rule out decreased hiring, temporary layoffs (Feldstein, 1976, 1978; Topel, 1983), and voluntary exits (or “quits”) (Carvalho, Corbi, and Narita, 2018; Albanese, Piccio, and Ghirelli, 2020; Jäger et al., 2020) as driving our results. These results are robust to the inclusion of additional controls, including household and occupation fixed effects.

While providing consistent support, it is important to qualify the individual-level results. Our analysis requires state-by-industry-by-year data, rather than industry-by-year or state-by-year as in prior studies using CPS data. Combined with comparatively rare layoff events, this leads to estimates based on very few observations. We interpret the CPS results as providing complementary evidence that reinforces our firm- and state-level results.

In Appendix C.1, we present additional firm-level evidence against decreased hiring as driving our results. To summarize, we find that the licensing effect of UI expansions is driven by firms that are actively shrinking their workforce, not those that are merely reducing the pace of hiring. We obtain consistent results when we limit our sample to firms that are reducing their workforce by more than one, two, and three percent, firms that are slow to hire with a below-industry pace of employment growth, and across both low and high turnover industries and firms, making decreased hiring unlikely to be the underlying mechanism. We

²³ This limits our sample to manufacturing sectors.

provide a more detailed discussion on temporary layoffs and voluntary exits in Appendix C.2 and C.3.

----- Insert Table 6 about here -----

4.6. Robustness Tests

In Appendix E, we conduct a series of robustness checks. Notably, we obtain consistent and slightly sharper results when we disregard changes in the maximum duration and calculate UI expansions based solely on changes in the maximum weekly benefit amount. The results are also robust to subsampling across state-years with low unemployment rates as well as high unemployment rates that may raise the expectation of triggering the Extended Benefits (EB) program (Hagedorn *et al.*, 2019). The effect on layoffs becomes stronger when we exclude firms that operate across multiple states, for which the effective UI expansions are measured with errors. Our results are also robust to (1) replacing the binary *Low firm performance* with a continuous variable using a linear spline, (2) using inflation-adjusted nominal dollar amount increases in the maximum weekly benefit amount (*versus* percentage increases), (3) using sample periods that include more recent years (2008-2015) and/or earlier years (1970-1975), (4) excluding state-years with a large share of firms with low performance (i.e., left-tail observations in Appendix Figure A) at various thresholds, (5) excluding the three largest states (California, New York, and Texas), and (6) alternative specifications and control variables.

5. MECHANISM

In this section, we investigate the potential mechanism by which UI expansion leads to larger layoffs. In particular, we look at managerial and firm characteristics for indications that the additional layoffs are due to the moral cost of layoffs incurred by managers.

5.1. The Strength of Managerial Prosocial Preferences

While managerial preferences and motivations for layoffs are difficult to observe, we rely on CEOs' political leanings as a well-established proxy. Prior research documents that Democratic managers tend to be more prosocial, spending an additional ten percent of the firm's net income on CSR activities on average despite limited evidence of their financial benefits (Di Giuli and Kostovetsky, 2014). They are also less likely to be the subject of civil rights and labor litigations relative to Republican managers (Hutton, Jiang, and Kumar, 2015). In columns 1-2 and 3-4 of Table 7, we divide our sample based on whether the CEO made personal political donations to Senate, House, and presidential candidates who are Republican (Hutton, Jiang, and Kumar, 2014). Political donation data is collected from the

Federal Election Commission (FEC) and spans 1993 to 2007. Republican CEOs represent 40 percent of our sample. In response to low performance, Democratic CEOs layoff 2.0 percent of workers relative to 4.4 percent by Republican CEOs, consistent with having stronger prosocial preferences and incurring greater moral costs of layoffs. The retention of additional workers by Democratic CEOs, however, leads to a significantly larger licensing effect in years with UI expansions. A ten percent increase in UI increases layoffs by 2.86 percentage points ($p < 0.01$) in contrast to 0.98 percentage points for Republican CEOs ($p = 0.37$). This difference is significant at 10% based on z-statistics.

Columns 5-6 and 7-8 divide the sample based on whether the CEO was promoted internally or hired externally. Yonker (2017) draws from the psychological theory of place attachments and shows managerial bias against divestitures and pay reductions for plants near the hometowns of CEOs. We expect that internal CEOs incur a greater moral cost of layoffs because of a similar set of social factors. An external CEO is identified as a CEO whose first year as a CEO is the year they joined the firm. Internal CEOs are non-external CEOs. We obtain CEO-related information from the Execucomp database, and the sample covers 1993-2007. External CEOs represent 36 percent of our sample firms. In line with our earlier analysis based on political leanings, UI expansions have a larger effect on internal CEOs who layoff fewer workers despite low performance. This difference is significant at 5% based on z-statistics. We interpret these results with caution because appointing a Republican or external CEO may be endogenous to the need for layoffs (see Data Appendix for data sources and variable definitions for subsample analyses). However, the exogenous expansions in UI benefits (ΔUI) help to mitigate this concern.

----- Insert Table 7 about here -----

5.2. Corporate Governance and the Discretion to Act on Moral Concerns

5.2.1 Takeover Protection

If the increased layoffs from UI expansion indeed stem from reducing the moral cost to managers, we expect the effects to be greater in weakly-governed firms that allow managers to act on their moral concerns. To test the prediction, we leverage the staggered adoption of antitakeover laws that insulate managers from activist investors and performance pressures of the market for corporate control. Since the pioneering study by Bertrand and Mullainathan (2003), a vast body of empirical research documents that antitakeover laws allow managers to shirk from costly efforts at restructuring and enjoy a quiet life. Among the multiple types of antitakeover laws, we follow Bertrand and Mullainathan (2003) and focus on the adoption of

Business Combination laws. *Weak governance_{gt}* is a binary variable set to one if a Business Combination law was adopted in a firm's state of incorporation g by year t and sets up a standard difference-in-difference estimation. A firm's state of incorporation, which dictates corporate governance laws (indexed with g) (e.g., Delaware), differs from a state of a firm's primary operation (indexed with s). This incongruence, in addition to the incongruence in state and industry boundaries, allows us to estimate the effects of weak governance while controlling for state of operation-by-year fixed effects.²⁴ Approximately sixty percent of public firms are incorporated in Delaware to take advantage of its advanced corporate laws, but less than three percent of firms maintain their primary operations there.

In column 1 of Table 8, *Weak governance* does not independently affect layoffs. However, in column 2, the negative and significant coefficient for *Low performance* and the positive and significant coefficient for the interaction term *Weak governance* $\times$ *Low performance* indicate that poorly performing firms engage in layoffs, but to a lesser extent, when they are protected against takeover pressures; *Weak governance* decreases layoffs in response to low performance by 14.6 percent. In other words, layoffs in weakly governed firms are less sensitive to poor performance. Column 3 examines the interaction between *Weak governance* and UI expansions. The negative and significant coefficient for *Weak gov.* $\times$ ΔUI indicates that UI expansions indeed lead to larger layoffs in weakly governed firms that maintain a larger number of marginal workers. To further examine the dynamics, column 4 includes the three-way interaction between *Weak governance*, ΔUI , and *Low performance* in what amounts to a quadruple-differences estimation. The negative and significant coefficient for the interaction term *Weak gov.* $\times$ ΔUI $\times$ *Low performance* indicates that protected managers make larger layoffs in response to UI expansions, despite being insulated against takeover pressures; a ten percent increase in UI benefits is sufficient to fully moderate the reduced layoffs from the adoption of the Business Combination law.

With respect to capital investment in columns 6 and 7, we again do not observe any significant effect from ΔUI and its interactions with *Low performance* and *Weak governance*. The null effect on capital investment sets a challenging bar for alternative explanations. They would have to coincide with (i) the adoptions of antitakeover laws in the state of incorporation, (ii) UI expansions in the state of operation, and (iii) firm-specific low performance, and (iv) only affect layoffs and not capital investment. In Appendix B, we also show in a dynamic

²⁴ We use the period of 1976 to 2008, but limiting the sample to 1983-1999, as recommended by Karpoff and Wittry (2018), yields consistent results.

specification that the larger layoffs in response to UI expansions in weakly governed firms occur two years after the passage of Business Combination laws and not before.

The results provide support for our argument as well as deeper insight into the behavioral underpinnings of the quiet life hypothesis. Bertrand and Mullainathan (2003) suggest that weak governance allows managers to become lazy and shirk from “cognitively difficult activities” (p.1067), such as the closing of a low-productivity plant. Our results suggest that weaker governance also allows managers to avoid morally costly activities. As indicated by the opening quote from Brian Chesky and Grunberg et al. (2006), managers report layoffs as negatively affecting their physical and mental health and sometimes opt to making abrupt layoff notices (e.g., slipping a pink slip) that minimize stressful personal interactions (Folger and Skarlicki, 1998). The reduced restructuring stems at least in part from an increased exercise of their prosocial preferences, not solely because of their preference to idle.

Our interpretation based on moral cost also aligns with more recent empirical evidence that managers, when left unchecked, do not sit still and instead make themselves busy. For example, they make unrelated acquisitions (Gormley and Matsa, 2016), increase CSR expenditure (Di Giuli and Kostovetsky, 2014), and provide pay increases to plants located in their hometowns or closer to corporate headquarters (Landier, Nair, and Wulf, 2009; Yonker, 2017).

----- Insert Table 8 about here -----

5.2.2. Financial Constraint

In Table 9, we examine how financial slack affects layoffs in response to UI expansions. Agency research documents that internal cash flows are not subject to the same degree of external monitoring and allow managers to deprioritize efficiency and pursue other personal goals (Jensen, 1986; Harvey et al., 2004; John, Li, and Pang, 2017). Similar to our earlier discussion based on antitakeover laws, we expect the reduction in moral cost from UI expansions to have a larger effect on financially unconstrained firms whose managers have greater discretion to act on their moral concerns and keep additional workers on the payroll despite falling demand.

We divide our sample based on a firm’s overall debt ratio (total debt divided by total asset) in columns 1 and 2. The negative coefficient for the interaction term $\Delta UI \times Low\ performance$ is larger for low debt firms relative to high debt firms (-0.172 *versus* -0.101), but their difference is statistically insignificant. In columns 3 and 4, we divide our sample based on the ratio of short-term debt relative to the industry median with the expectation that short-

term debt that must be repaid within a year more effectively constrains managers' discretion to keep workers on the payroll. The coefficient for $\Delta UI \times Low\ performance$ is three times larger and statistically significant only for low short-term debt firms (-0.134 *versus* -0.045).

Because a firm's capital structure is likely endogenous to firm performance, demand for labor, and also UI (Agrawal and Matsa, 2013), we next isolate the fraction of short-term debt that comes from currently maturing long-term debt. Almeida *et al.* (2012) suggest that long-term borrowing decisions made several years earlier are plausibly exogenous to a firm's performance and industry conditions in the year in which such debt matures. Columns 5 and 6 divide firms into high and low constraint firms based on the amount of long-term maturing debt but restrict the sample to firms with a total debt ratio above the median (i.e., firms in column 1) to capture firms with a meaningful amount of such debt. The differences between high- and low-constraint firms become more drastic. A ten percent increase in UI increases firms' layoffs by 1.23 percentage points in the low short-debt sample ($p=0.01$) but only by 0.07 percentage points in the high short-debt sample ($p=0.98$), and their differences are significant at 10% based on z-statistics.²⁵ In Appendix E.7, we obtain consistent results using alternative proxies of financial constraints based on the WW Index (Whited and Wu, 2006) and the SA Index (Hadlock and Pierce, 2010). In Appendix E.8, we show that financial slack affects only layoffs in response to UI expansion and not capital investment.

----- Insert Table 9 about here -----

We cannot infer conclusively whether the layoff rigidity from moral cost and its relaxation from UI expansions are optimal or detrimental to firm performance. Displaying moral concerns and reducing layoffs may increase employee morale and have positive effects on employee morale and retention. However, managers act more strongly on their moral concerns in the absence of performance pressure from the market for corporate control (Table 8). It could be that insulated managers can better act on private internal information, but moral concerns have a stronger effect during periods of financial slack (Table 9), which shows significant quasi-random fluctuations based on internal funding cycles unrelated to firm investment opportunities (Table 9). In contrast, we do not see significant differences across years when the labor market shows high degrees of slack and when it is easier to find

²⁵ Agrawal and Matsa (2013) find that UI permits firms to *increase* leverage by reducing the unemployment risk of workers and the wage premium that must be provided for the risk of job loss. Johnston (2021) shows that increased UI taxes decrease labor demand in financially constrained firms. The stronger effect in low-constraint firms reduces the concern that increased sensitivity of layoff decisions to low performance may stem from UI's effect on firm capital structure.

replacement hires (Appendix C.2). In a close parallel, prior research on managerial characteristics, such as overconfidence, narcissism, and hometown bias, interprets their weaker effect on firm investment under strong governance as an indication of their negative effect (e.g., Malmendier and Tate, 2005; Zhu and Chen, 2014; Yonker, 2017). These patterns provide tentative yet consistent evidence that layoff rigidity from managerial moral costs is unlikely to be driven by strategic considerations.

5.2.3 Strength of Relational Governance

In Table 10, we examine differences based on two proxies of the importance of relational governance supported by close social ties (Poppo and Zenger, 2002). We expect UI expansions to have a greater effect in family firms, which show higher quality of labor relations, longer average employee tenure, and lower layoff propensity (e.g., Mueller and Philippon, 2011; Ellul, Pagano, and Schivardi, 2018; Kim et al., 2020). We obtain a firm's ownership status from Anderson et al. (2012), who collect data on the 2000 largest firms between 2001 and 2010. Family firms with more than five percent ownership by family members comprise approximately one-third of the sample. Consistent with the larger importance of moral costs, the coefficient for $\Delta UI \times Low\ performance$ is approximately two times larger and achieves statistical significance only in the family-firm subsample despite the smaller sample size in columns 1-2 and 3-4.

Next, we look at the population size of a firm's headquarter location. Landier, Nair, and Wulf (2009) find that layoffs and divestitures are less likely in divisions located closer to the corporate headquarters due to stronger social ties, especially when the headquarters are in a less populous county. We obtain population counts for each zipcode in 2003 and split our sample based on the population size of firm headquarter location from 1993 to 2007. In columns 5-6 and 7-8, firms headquartered in less populous zipcodes engage in fewer layoffs in response to low firm performance but show a greater increase in layoffs from UI expansions.²⁶ While reinforcing our earlier findings, we interpret the results based on family ownership and populations size more tentatively because we do not have an exogenous variation in family ownership and headquarter location.

----- Insert Table 10 about here -----

5.3. Reaction to Stakeholder Concerns

²⁶ The results become incrementally weaker as we expand the time window further back in time.

We lastly examine in Table 11 whether the increased layoffs from UI expansions reflect the preferences of external stakeholders or managers. Firms may be more conservative about layoffs to project a positive image to activists and customers, which could affect a firm’s long-term performance. With more generous social safety nets in place, these stakeholders with a preference for prosocial firms may be less inclined to punish firms for dismissing workers. As a result, UI expansions could reduce the value of “doing good” for the sake of “doing well,” and managers may exploit UI expansions as a strategic opportunity to layoff more workers. This potentially strategic aspect of moral cost closely corresponds to the motivational and efficiency-based explanations for downward wage rigidity (Goette, Sunde, and Bauer, 2007).

To investigate this potential mechanism, we divide our sample into firms that depend more or less on positive external evaluation. Columns 1 and 2 divide our sample into business-to-business and business-to-consumer (B2C) industries, where purchasing decisions depend more heavily on customers’ positive perception of a firm’s image (Lev, Petrovits, and Radhakrishnan, 2008). The coefficients for both *Low Performance* and $\Delta UI \times Low Performance$ show little difference. Columns 3 and 4 divide the sample based on the Kinder, Lydenberg, and Domini (KLD) index of labor relations based on the intuition that higher-rated firms would be more constrained to take opportunistic advantage of UI expansions (Flammer and Luo, 2017).²⁷ Higher-rated firms indeed show lower performance sensitivity and layoff fewer workers despite low performance (1.2 percent *versus* 2.9 percent). However, UI expansions increase layoffs similarly across both samples; a ten percent increase in UI benefits increases layoffs by 1.6 percent points and 2.1 percent points for high and low KLD scores, respectively. Columns 5 and 6 divide the sample based on a firm’s marketing spend with the expectation that positive external evaluation is more important for firms that heavily invest in marketing (Fisman, Heal, and Nair, 2006), and the coefficient for $\Delta UI \times Low Performance$ shows little difference. The differences in the coefficients of $\Delta UI \times Low Performance$ uniformly fail to reject the null across all three subsamples at 10% significance based on *z*-statistics, indicating that external image concerns at the least do not dominate the moral concerns. All of the results remain the same after controlling for state-by-year fixed effects.

----- Insert Table 11 about here -----

6. CONCLUSION AND DISCUSSION

²⁷ The index consists of 9 categories: union relations, cash profit sharing, employee involvement, employee health and safety, supply chain labor standards, human capital development, labor management, controversial sourcing, and human capital-other strength.

This study presents evidence consistent with the presence of moral cost to managers' layoff decisions. We take advantage of expansions in state UI benefits over time and show that UI expansions lead to larger layoffs in firms experiencing negative economic shocks, such as low firm performance or increased Chinese import competition. The moral cost has a highly heterogeneous effect, decreasing in importance for well-governed or financially-constrained firms that provide little room for managers to act on their moral concerns while increasing in importance for more socially-conscious managers and family-owned firms.

Our findings extend the literature on individual prosocial behavior in experimental and behavioral economics to managers operating under market and stakeholder pressures. In particular, it provides a more balanced view of managers and their preferences. Although they can be selfish and self-centered, managers also care about their employees and act on moral concerns. Our findings also present a more controversial view of managers' prosocial behaviors (Di Giuli and Kostovetsky, 2014). For rank and file employees, prosocial preferences are thought to benefit the firm by constraining self-serving behaviors, especially in instances where external monitoring is not feasible. In the case of managers, our findings on governance strength provide a tentative but consistent set of evidence that managerial prosocial preferences result in avoiding necessary but morally costly restructuring activities, in this case, layoffs. By extension, we speculate that UI expansions facilitate firm adaptation to negative economic shocks by allowing managers to undertake morally costly yet necessary layoffs.

The negative performance implication of moral concerns adds to the growing body of evidence that views prosocial investments as stemming from private managerial benefits and agency conflicts (Di Giuli and Kostovetsky, 2014). For example, Petrenoko et al. (2014) find that narcissistic CEOs engage in greater CSR activities to satisfy their higher need for attention and praise and reinforce their positive self-view without clear financial returns. The absence of strategic motivation is a key difference to prior examinations of wage rigidity that consider the effects on worker productivity (Cohn et al., 2015). An important question for future research concerns whether managers acting on their moral and prosocial concerns are beneficial to long-term firm performance.

Furthermore, this study has important implications for designing UI and social insurance programs more generally. The far-reaching consequences of UI programs have been studied at multiple levels of analysis, including an individual's job search behavior and aggregate macroeconomic effects. This study shifts the focus to firms and their managers, allowing us to link the vast research on UI with firm-level research on corporate governance

and organizational research. For example, Walmart has received intense criticism for paying low wages that force its employees to rely on the Supplemental Nutrition Assistance Program (SNAP, formerly Food Stamps).²⁸ We suggest potential reverse causation; expansions in SNAP reduce the risk that workers and their families will starve and license Walmart to keep wages below the poverty line. As seen in the anecdote from the *Wall Street Journal* in the introduction, we expect moral licensing to reduce the effectiveness of expanding UI during crises by providing managers with an excuse to make layoffs.

This is the first step of a broader research project that incorporates the role of managers' moral concerns in the evaluation of social insurance programs. We expect the moral cost to extend beyond UI to other government programs that reduce the costs of unemployment, such as universal basic income and Medicare-for-all, and to broader labor market dynamics, including hiring and wage-setting. For example, a straightforward extension of our argument leads us to speculate that Medicaid expansion could have led to laying off additional workers because managers did not want to be responsible for the loss of their health insurance. This and other extensions of moral cost to evaluating public policy and firm adjustment are left for future research.

²⁸ <https://www.forbes.com/sites/clareoconnor/2014/04/15/report-walmart-workers-cost-taxpayers-6-2-billion-in-public-assistance/#31bdb96c720b>

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TABLE 1
Sample Statistics

Variables	N	Mean	SD	Min	Max
<i>Panel A. State-level characteristics (1977-2007)</i>					
1. ΔUI_t	1,612	0.043	0.062	-0.52	0.65
2. Maximum Weekly Benefit	1,612	237.9	106.5	60	862
3. Maximum Regular Duration (Week)	1,612	26.3	1.5	20	39
4. Max UI Benefit amount	1,612	6260.0	2897.6	1,200	25,860
<i>Panel B. Firm-level characteristics (1977-2007)</i>					
1. Employee growth $_{t+1}$	66,718	0.042	0.214	-1.83	1.91
2. Capital investment intensity $_{t+1}$	66,208	0.072	0.063	-0.01	2.99
3. ΔUI_t	66,718	0.045	0.063	-0.52	0.65
4. Low performance $_t$ (=1)	66,718	0.383	0.486	0.00	1.00
5. Tobin's Q	66,718	1.547	1.495	0.20	203.47
6. Industry revenue growth $_t$	66,718	0.117	0.146	-1.96	2.78
7. Debt ratio $_t$	66,718	0.269	0.215	0.00	8.15
8. Current ratio $_t$	66,718	2.169	1.474	0.00	57.83
9. Working capital to sales ratio $_t$	66,718	0.263	13.584	-69.25	3453.4
10. Distance to bankruptcy $_t$	66,718	3.952	4.038	-80.40	190.96
11. Total asset $_t$ (log)	66,718	5.898	1.698	0.62	12.53
12. Industry concentration $_t$	66,718	0.227	0.171	0.01	1.00
13. Industry concentration $_t^2$	66,718	0.080	0.132	0.00	1.00

Notes: This table presents sample statistics for the variables. The sample includes all Compustat firm-year observations from 1977–2007 with non-missing variables and at least 500 workers for years $t+0$ and $t-1$. ΔUI is the year-to-year change in maximum UI benefits. Information on the maximum weekly benefit amount and duration for each state is obtained from the Department of Labor’s publication “Significant Provisions of State UI Laws.” *Low Performance* is a binary variable set to one if firm performance falls below the industry benchmark, calculated as the median ROA of a firm’s primary four-digit SIC industry for each fiscal year.

TABLE 2
UI Expansion and Layoffs in Response to Low Firm Performance

	$\Delta \text{Employment}_t$						$\text{Capital investment}_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔUI_t	-0.027*		-0.030**	0.009			-0.001	
	[0.014]		[0.014]	[0.014]			[0.003]	
Low performance _t (=1)		-0.044***	-0.044***	-0.039***	-0.037***	-0.039***	-0.008***	-0.008***
		[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.001]	[0.001]
$\Delta \text{UI}_t \times \text{Low performance}_t$				-0.099***	-0.093***	-0.110***	0.004	0.004
				[0.026]	[0.022]	[0.027]	[0.005]	[0.005]
Adj. R2	0.176	0.183	0.183	0.183	0.160	0.182	0.579	0.582
Obs.	66,718	66,718	66,718	66,718	67,569	66,591	66,166	66,039
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	no	yes	yes	yes
State×Year FE	no	no	no	no	yes	yes	no	yes

Notes: OLS estimates of Equation (1). The sample includes all public firms recorded in Compustat with non-missing variables between 1977 and 2007. Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 3
UI Expansion and Industry-Level Negative Economic Shocks

DV:	$\Delta \text{Employment}_t$								
IV:	Chinese Import Competition			Industry Value added			Industry Revenue Growth		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Negative shock _t	-0.024*** [0.005]	-0.023*** [0.005]	-0.020*** [0.006]	-0.033*** [0.004]	-0.030*** [0.004]	-0.026*** [0.004]	-0.022*** [0.002]	-0.019*** [0.002]	-0.019*** [0.002]
ΔUI_t		-0.080*** [0.025]			-0.021 [0.019]			0.004 [0.023]	
$\Delta \text{UI}_t \times \text{Negative shock}_t$		-0.012*** [0.003]	-0.014** [0.006]		-0.080* [0.046]	-0.108** [0.048]		-0.061** [0.024]	-0.061** [0.025]
Adj. R2	0.157	0.157	0.157	0.14	0.140	0.146	0.144	0.144	0.147
Obs.	10,746	10,746	10,621	21,128	21,128	20,920	57,197	57,197	57,065
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year	yes	yes	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	no	yes	no	no	yes	no	no	yes

Notes: OLS estimates of Equation (1). Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 4
High- versus Low- Benefit Amount States

DV:	$\Delta \text{Employment}_t$							
	High Benefit States				Low Benefit States			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low performance _t (=1)	-0.039*** [0.003]		-0.033*** [0.004]	-0.032*** [0.004]	-0.044*** [0.010]		-0.042*** [0.011]	-0.043*** [0.012]
ΔUI_t		-0.042** [0.018]	0.006 [0.023]			-0.004 [0.024]	0.013 [0.033]	
$\Delta \text{UI}_t \times \text{Low performance}_t$			-0.136** [0.052]	-0.148** [0.055]			-0.051 [0.061]	-0.040 [0.061]
Adj. R2	0.185	0.180	0.185	0.184	0.186	0.180	0.186	0.182
Obs.	37,842	37,842	37,842	37,800	25,764	25,764	25,764	25,677
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	no	no	yes	no	no	no	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 5
State-Level Analysis Using County Business Pattern (CBP) Data

DV:	$\Delta \text{Employment}_t$				
	(1)	(2)	(3)	(4)	(5)
Negative shock _t (=1)	-0.032*** [0.006]		-0.032*** [0.006]	-0.025*** [0.006]	-0.024*** [0.006]
ΔUI_t		-0.045 [0.039]	-0.045 [0.039]	-0.006 [0.036]	
$\Delta \text{UI}_t \times \text{Negative shock}_t$				-0.145** [0.062]	-0.151** [0.067]
Adj. R2	0.149	0.148	0.149	0.149	0.155
Obs.	104,171	104,171	104,171	104,171	104,168
Year FE	yes	yes	yes	yes	yes
State×SIC FE	yes	yes	yes	yes	yes
State×Year FE	no	no	no	no	yes

Notes: The unit of observation is state-industry. *Negative shock* is set to one for each four-digit SIC code if value-add from the NBER-CES Manufacturing Industry Database declines relative to the previous year. Standard errors are clustered at the state and industry (SIC4) level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 6
Decreased Hiring versus Increased Layoffs: CPS Results

DV:	Layoff _t (=1) × 100							
IV:	Chinese Import Competition				Industry Value Added			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔUI_t	-0.131 [0.121]		-0.160 [0.118]		-0.605 [0.579]		-1.506** [0.682]	
Negative shock _t		0.270** [0.113]	0.149* [0.077]	0.213 [0.139]		0.589*** [0.174]	0.450*** [0.141]	0.398*** [0.119]
$\Delta UI_t \times \text{Negative shock}_t$			3.099*** [1.029]	2.273** [1.028]			3.111** [1.238]	2.355* [1.182]
Adj. R2	0.015	0.015	0.015	0.016	0.019	0.020	0.020	0.025
Obs.	1,624,031	1,624,031	1,624,031	1,624,031	2,640,836	2,640,836	2,640,836	2,640,836
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
State×SIC FE	yes	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	no	no	yes	no	no	no	yes

Notes: Standard errors are clustered at the industry and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 7
Strengths of Managers' Moral Concerns

	$\Delta \text{Employment}_t$							
	Republican CEO		Non-Rep. CEO		External CEO		Internal CEO	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔUI_t	-0.101 [0.068]		0.058 [0.041]		-0.127 [0.081]		0.010 [0.045]	
Low performance _t (=1)	-0.039*** [0.012]	-0.044*** [0.015]	-0.019** [0.009]	-0.020** [0.009]	-0.033** [0.013]	-0.012 [0.020]	-0.007 [0.011]	-0.008 [0.013]
$\Delta UI_t \times \text{Low performance}_t$	0.015 [0.079]	-0.098 [0.108]	-0.216*** [0.065]	-0.286*** [0.078]	-0.103 [0.067]	-0.132* [0.073]	-0.350*** [0.087]	-0.397*** [0.106]
Adj. R2	0.204	0.189	0.196	0.184	0.234	0.135	0.224	0.193
Obs.	5,162	5,055	8,195	8,110	2,149	1,920	4,670	4,538
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	yes	no	yes	no	yes	no	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 8
Corporate Governance and Layoffs in Response to UI Expansion

DV:	$\Delta \text{Employment}_t$					$\text{Capital investment}_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Weak gov _t (=1)	0.004 [0.006]	0.000 [0.006]	0.006 [0.006]	0.000 [0.006]	0.002 [0.005]	0.003 [0.002]	0.006*** [0.002]
Low performance _t		-0.048*** [0.004]		-0.046*** [0.005]	-0.047*** [0.005]	-0.010*** [0.001]	-0.009*** [0.001]
Weak gov _t × Low performance _t		0.007* [0.004]		0.012*** [0.004]	0.012*** [0.004]	0.001 [0.001]	0.001 [0.001]
ΔUI_t			0.002 [0.014]	0.010 [0.020]		-0.001 [0.006]	
Weak gov _t × ΔUI_t			-0.060*** [0.020]	-0.006 [0.027]	-0.076 [0.068]	0.001 [0.011]	-0.024 [0.016]
$\Delta \text{UI}_t \times \text{Low performance}_t$				-0.027 [0.037]	-0.035 [0.038]	0.000 [0.008]	-0.001 [0.008]
Weak gov _t × $\Delta \text{UI}_t \times \text{Low performance}_t$				-0.142* [0.077]	-0.145* [0.074]	0.005 [0.014]	0.004 [0.014]
Adj. R2	0.176	0.183	0.176	0.183	0.182	0.569	0.572
Obs.	66,718	66,718	66,718	66,718	66,591	70,913	70,801
Controls	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	no	no	no	yes	no	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 9
Financial Constraints and Layoffs in Response to UI Expansion

DV:	$\Delta \text{Employment}_t$					
	Debt		Short-term debt		Maturing long-term debt	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
ΔUI_t	0.010 [0.029]	0.015 [0.018]	-0.041 [0.033]	0.019 [0.021]	-0.084 [0.061]	0.039 [0.037]
Low performance _t (=1)	-0.033*** [0.004]	-0.043*** [0.004]	-0.036*** [0.006]	-0.037*** [0.004]	-0.038*** [0.006]	-0.019*** [0.006]
$\Delta \text{UI}_t \times \text{Low performance}_t$	-0.101** [0.041]	-0.172*** [0.062]	-0.045 [0.047]	-0.134*** [0.050]	-0.007 [0.075]	-0.123** [0.046]
Adj. R2	0.171	0.226	0.175	0.211	0.173	0.160
Obs.	35,506	27,798	28,146	34,536	13,803	16,850
Controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes
State×Year FE	no	no	no	no	no	no

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 10
Relational Governance and Layoffs in Response to UI Expansion

	$\Delta \text{Employment}_t$							
	Family firm		Non-family firm		Small population		Large population	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔUI_t	0.057		0.035		-0.014		0.019	
	[0.063]		[0.046]		[0.045]		[0.034]	
Low performance _t (=1)	-0.014	-0.015	-0.013	-0.013	-0.023***	-0.023***	-0.032***	-0.033***
	[0.011]	[0.012]	[0.008]	[0.009]	[0.006]	[0.007]	[0.006]	[0.007]
$\Delta \text{UI}_t \times \text{Low performance}_t$	-0.334***	-0.378***	-0.188	-0.200	-0.251***	-0.277***	-0.140**	-0.146**
	[0.120]	[0.115]	[0.126]	[0.132]	[0.085]	[0.077]	[0.062]	[0.070]
Adj. R2	0.099	0.055	0.158	0.138	0.183	0.178	0.176	0.166
Obs.	2,903	2,865	5,586	5,542	15,140	15,081	15,016	14,933
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	yes	no	yes	no	yes	no	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 11
Stakeholder Pressures versus Moral Cost

	$\Delta \text{Employment}_t$					
	B2C	B2B	High KLD	Low KLD	High Marketing	Low Marketing
	(1)	(2)	(3)	(4)	(5)	(6)
ΔUI_t	0.034*	-0.010	0.007	0.036	0.021	-0.009
	[0.020]	[0.019]	[0.024]	[0.049]	[0.022]	[0.020]
Low performance _t (=1)	-0.045***	-0.034***	-0.012**	-0.029***	-0.045***	-0.038***
	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]	[0.004]
$\Delta UI_t \times \text{Low performance}_t$	-0.104***	-0.095***	-0.160**	-0.210**	-0.091*	-0.101***
	[0.034]	[0.034]	[0.068]	[0.085]	[0.046]	[0.035]
Adj. R2	0.169	0.198	0.186	0.179	0.177	0.201
Obs.	32,199	34,519	15,462	14,358	30,353	34,534
Controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes
State×Year FE	no	no	no	no	no	no

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

DATA APPENDIX

Data Source, Definition, and Details For Subsample Analysis

Table 1: Distribution of ΔUI

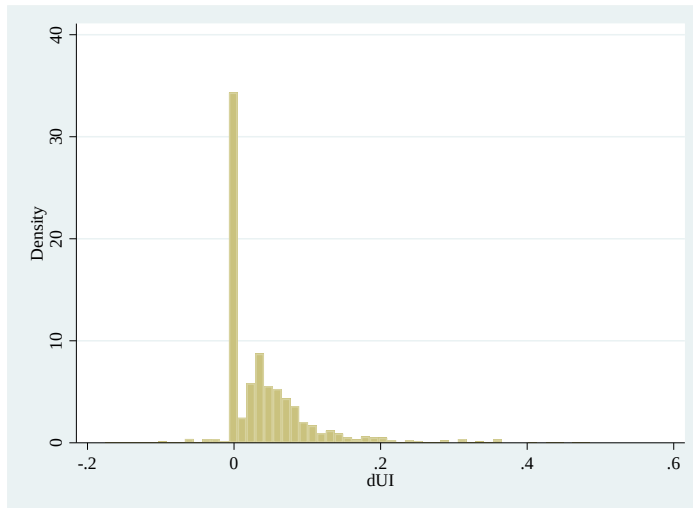


Table 2: Alternative Industry-Level Negative Economic Shocks

Chinese import competition data: accessible at
<https://economics.mit.edu/faculty/acemoglu/data/empag>

Table 4: County Business Pattern (CBP) database

Accessible at <http://fpeckert.me/cbp/>

Table 5: Current Population Survey (Table 5)

Our overall empirical design closely follows Bertrand (2004) who examines the interaction between import competition and (state-level) unemployment rate, which negatively affects wage rigidity by relaxing implicit wage agreements.

Sample: we obtain data from IPUMS over the period of 1978 to 2007.²⁹ The sample window starts from 1978, rather than 1976, because of the incompatibility of the 1977 data. Our sample is limited to manufacturing industries because data for the two industry-level shocks are available for only the manufacturing sector. For our analysis, we start with individuals who are in the workforce (*labforce* variable = “yes, in the labor force”).

Identifying layoffs: we identify an individual to be laid off if their employment status (*whyunemp* variable) changes to “job loser - on layoff” from employment (“niu”) within the four consecutive months of the survey. The variable *whyabsnt* provides additional information on the types of layoffs, including “on temporary layoff (under 30 days)” and

²⁹ <https://usa.ipums.org/usa/>

“on indefinite layoff (30+ days).” We set our dependent variable *Layoff* to one for indefinite layoffs but the results remain unchanged when we also include temporary layoffs.

Concordance between SIC and Census industry classification: IPUMS provides 1990 Census Industry Classification (CIC) for its data. Linking Chinese import competition and NBER Manufacturing Industry data in 1987 SIC classification to 1990 CIC presents a complex task. We take a two-step procedure using the concordance tables provided by David Dorn:³⁰ (1) We first map SIC87 to ind1990ddx industries, computed by Autor, David Dorn and Gordon Hanson (2019) to provide a more detailed balanced industry panel for the 1980, 1990, and 2000 CIC (File C8). (2) We then link ind1990ddx to 1990 CIC (File C6). There are few cases where more than one SIC87 correspond to a single ind1990ddx industry, in which case we take an average.

Summary Statistics					
Variables	N	Mean	SD	Min	Max
<i>Panel A. Chinese import competition (1991-2007)</i>					
1. Layoff(=1)	1,624,031	0.011	0.103	0.00	1.00
2. Exposure	1,624,031	0.006	0.033	-0.25	2.33
3. ΔUI_t	1,624,031	0.035	0.049	-0.52	0.52
<i>Panel B. Industry value add (1979-2007)</i>					
1. Layoff(=1)	2,640,836	0.014	0.117	0.00	1.00
2. Decline (=1)	2,640,836	0.295	0.361	0.00	1.00
3. ΔUI_t	2,640,836	0.044	0.059	-0.52	0.54

Table 7: Corporate Governance and Layoffs in Response to UI Expansion

Business Combination Laws: we use the adoption years from Karpoff and Wittry (2018: Table II). We use the state of incorporation data from Compustat. The sample covers 1977-2007.

Table 6 and Table 11. Stakeholder Pressures and Layoffs in Response to UI Expansion

B2B vs. B2C firms: We adopt the classification of B2C industries from Lev *et al.* (2010: 188). The sample covers 1977-2007.

High KLD vs. Low KLD firms: we use the employee-related strength score (*emp_str_num*) that ranges from 0 to 9 from the Kinder, Lydenberg, and Domini (KLD) index of labor relations. The index consists of 9 categories: union relations, cash profit sharing, employee involvement, employee health and safety, supply chain labor standards, human capital development, labor management, controversial sourcing, and human capital-other strength. Its coverage starts from 1991 with unbalanced starting dates for firms, and we use the maximum value. The sample covers 1991-2007.

³⁰ <https://www.ddorn.net/data.htm>

High marketing vs. Low marketing firms: we estimate the average industry marketing intensity for each four-digit SIC code as the mean value of firm marketing intensity (Compustat variable *xad* divided by *revt*). The sample covers 1977-2007.

Republican CEO vs. Non-Republican CEOs: we use data from Hutton, Jiang, and Kumar (2014) who collect political donation data from the Federal Election Commission (FEC) Web site from 1992 to 2008. Accessible at: <https://sites.google.com/site/danlingjiang/data-library?authuser=0>.

External CEO vs. Internal CEO firms: we obtain CEO-related information from the Execucomp database. An external CEO is identified as a CEO whose first year as a CEO is the year the executive joined the firm. Internal CEOs are non-external CEOs. The sample covers 1993-2007.

Family firms: from Anderson, Reeb, and Zhao (2012). Accessible at: <http://www.ronandersonprofessionalpage.net/data-sets.html>. The sample covers 2001-2007.

APPENDIX A. Relation Between UI Expansions and Negative Economic Shocks

Prior studies that leverage the states' staggered UI expansions conduct a battery of tests and find a weak and null association between UI expansions and various *state*-level economic indicators, UI payroll taxes, and other government transfer programs (Agrawal and Matsa, 2013; Kroft and Notowidigdo, 2016; Hsu, Matsa, and Melzer, 2018). Most closely related to our tests, Agrawal and Matsa (2013: Table 6) do not detect any significant association between UI benefits and firms' profits or operating performance. Table B1.1. presents evidence that the UI expansions are not correlated with firm performance (ROA), revenue and profit growth, R&D investment, and inventory at year t or $t+1$. B1.2 repeats the analysis with respect to the three negative industry-level economic shocks we use: (1) Chinese import penetration, (2) slowing industry-level growth of Compustat firms, and (3) decreases in the year-to-year industry value-add from the NBER-CES Manufacturing Industry Database.

Table A.1. Firm Performance and UI Expansions

DV:	$\Delta UI_t (\log) \times 100$						$\Delta UI_{t+1} (\log) \times 100$					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ROA	-0.017 [0.161]					0.152 [0.214]	-0.370 [0.278]					-0.063 [0.344]
Revenue growth		-0.013 [0.115]				0.017 [0.134]		0.081 [0.193]				0.269** [0.131]
Profit growth			-0.013 [0.048]			-0.019 [0.047]			-0.028 [0.052]			-0.06 [0.051]
R&D (log)				0.133 [0.088]		0.113 [0.101]				0.145 [0.089]		0.104 [0.089]
Inventory (log)					-0.002 [0.060]	-0.026 [0.078]					-0.002 [0.042]	0.008 [0.044]
Adj. R2	0.121	0.121	0.117	0.117	0.121	0.121	0.141	0.141	0.14	0.137	0.141	0.144
Obs.	72,477	71,994	61,568	72,477	72,087	61,210	68,645	67,859	58,448	68,645	68,283	58,116
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: The unit of observation is firm-year. Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

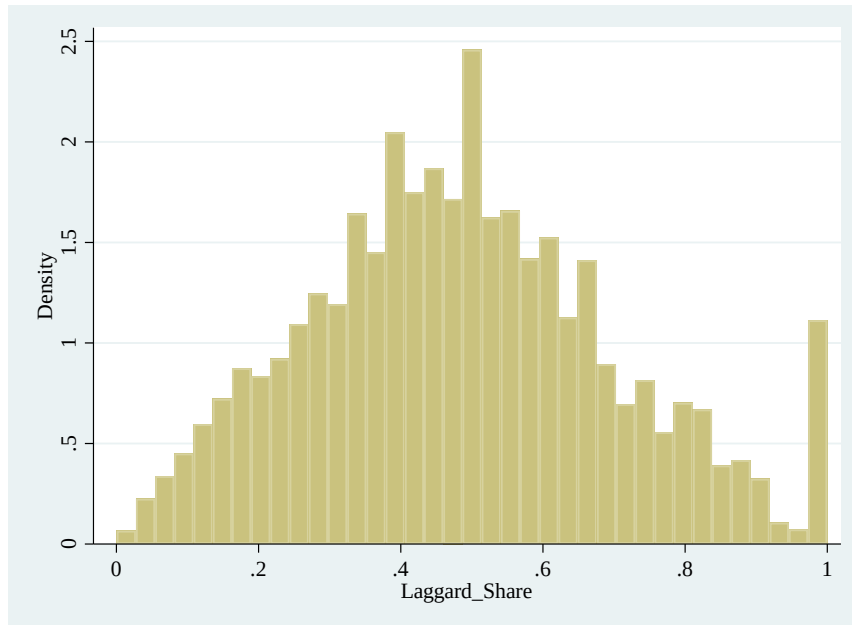
Table A.2. Industry-level Shocks and UI Expansions

DV:	ΔUI_t (log)			ΔUI_{t+1} (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
Chinese import competition	0.673 [0.522]			0.215 [0.444]		
Industry value added		-0.171 [0.295]			-0.081 [0.588]	
Industry revenue growth			-0.041 [0.556]			-0.606 [0.934]
Adj. R2	0.131	0.117	0.129	0.137	0.137	0.144
Obs.	11,398	72,467	22,178	10,768	68,637	20,985
Controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
SIC3 FE	yes	yes	yes	yes	yes	yes

Notes: The unit of observation is firm-year. Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Figure A. State-Year Distribution of Firms with Below-Industry Performance

Figure A shows that firms with below-industry performance are normally distributed across state-years (rather than being concentrated), making it more difficult for state governments to systematically undertake UI expansions in efforts to bail out employees of particular firms or industries. In Appendix Section C.1 and C.2, we show robustness to excluding state-years where the share of firms with low performance is above fifty percent (left tail observations in Figure B1) or excluding state-industries that employ more than three percent of the state's workforce.



x-axis (*Laggard_share*) indicates the percentage of firm-year observations with ROA below the industry benchmark (*Low performance*=1) for each state-year between 1977 and 2007.

Appendix B. Forward and Backward Lags

We show that managers only respond to a contemporaneous expansion in UI. The reduced moral obligation from UI expansion should depend on what Kahneman et al. (1986) terms “reference transaction” – in our case, improvement relative to managers’ expectations of workers’ welfare after being laid off. In almost the exact empirical setting as ours, Agrawal and Matsa (2013) and Dou, Khan, and Zhou (2016) find that managers update their reference point within a year. Fehr, Goette, and Zehnder (2009:372) observe that “For incumbent workers, however, the reference outcome is the contract that was in place the *last* period... (emphasis added).” In her examination of informal relational agreements, Bertrand (2004:727) specifies workers’ reservation wage to follow an AR(1) process. More generally, behavioral research on reference points documents that firms update their historical benchmarks rather quickly. For example, Greve (2003) and Eggers and Kaul (2018) report that less than 30 percent of historical reference points persist after one year.

TABLE B.1. UI Expansion and Layoffs

DV:	$\Delta \text{Employment}_t$					
	$n=-2$	$n=-1$	$n=0$	$n=1$	$n=2$	$n=3$
	<i>(Before⁻²)</i>	<i>(Before⁻¹)</i>	<i>(After^{t+0})</i>	<i>(After^{t+1})</i>	<i>(After^{t+2})</i>	<i>(After^{t+3})</i>
	(1)	(1)	(1)	(2)	(3)	(4)
ΔUI_{t-n}	0.010	0.014	0.009	-0.021	-0.014	0.004
	[0.022]	[0.017]	[0.014]	[0.020]	[0.015]	[0.015]
Low performance _{t-n} (=1)	-0.002	-0.017***	-0.039***	-0.054***	-0.022***	-0.004
	[0.002]	[0.003]	[0.004]	[0.002]	[0.003]	[0.003]
$\Delta \text{UI}_{t-n} \times \text{Low performance}_{t-n}$	0.040	-0.017	-0.099***	0.024	0.006	-0.041
	[0.046]	[0.037]	[0.026]	[0.022]	[0.026]	[0.026]
Adj. R2	0.181	0.181	0.181	0.180	0.173	0.167
Obs.	68,912	68,912	68,912	68,176	66,628	64,508
Controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes
State×Year FE	yes	yes	yes	yes	yes	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE B.2. UI Expansion and Layoffs

DV:	$\Delta \text{Employment}_t$					
	Run simultaneously					
	$n=-3$ ($Past^{-3}$)	$n=-2$ ($Past^{-2}$)	$n=-1$ ($Past^{-1}$)	$n=0$ ($Past^0$)	$n=1$ ($Future^{+1}$)	$n=2$ ($Future^{+2}$)
ΔUI_{t-n}	0.00 [0.016]	-0.002 [0.017]	-0.018 [0.018]	0.024 [0.016]	-0.006 [0.022]	0.000 [0.022]
Low performance $_{t-n}$ (=1)	-0.001 [0.003]	-0.010*** [0.003]	-0.040*** [0.003]	-0.026*** [0.004]	-0.009*** [0.003]	0.001 [0.003]
$\Delta UI_{t-n} \times \text{Low performance}_{t-n}$	-0.021 [0.027]	-0.009 [0.028]	-0.003 [0.029]	-0.122*** [0.033]	-0.015 [0.037]	0.01 [0.035]
Adj. R2	0.175					
Obs.	55,102					
Controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes
State×Year FE	yes	yes	yes	yes	yes	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

APPENDIX C.1. Decreased Hiring

Complementing the individual-level CPS results in Table 5, we present here *firm-level* evidence against decreased hiring as driving our results. In column 1 of Table C.1.1, we repeat our analysis from Table 2 but divide our sample into firm-years with a net decrease in employment (i.e., negative values of ΔEmp) and a net increase in employment (i.e., positive values of ΔEmp). Looking across columns 1-2 and 3-4, we find that the licensing effect of UI expansions is driven by firms that are actively shrinking their workforce, not those that are merely reducing the pace of hiring. The coefficient for $\Delta UI \times Negative\ shock$ is more than two times larger and achieves statistical significance only for firm-years that show a net decrease in a firm's workforce in columns 1 and 2. The results indicate that decreased hiring is unlikely to drive our findings.

We next divide our sample into fast and slow turnover industries with the expectation that hiring has a smaller effect on firm employment changes in slow-turnover industries. We approximate the average rate of turnover using the separation rate, measured as the ratio of workers affected by extended mass layoffs to total industry employment. The average industry separation rate is measured at the two-digit North American Industry Classification System level and obtained from Agrawal and Matsa (2013: Table A1) who use "Mass Layoff Statistics" from the US Bureau of Labor. We find that firms in high turnover industries indeed make slightly larger layoffs in response to low performance (-0.047 *versus* -0.034). The coefficient for $\Delta UI \times Negative\ shock$ is negative and significant across both samples with a slightly higher value for low-turnover industries (-0.090 *versus* -0.125), further indicating that decreased hiring is unlikely to be a primary driver of our findings.

Table C.1.1 Decreased Hiring *versus* Increased Layoffs: Firm-level Results

DV:	$\Delta Employment_t$							
	$\Delta Emp < 0$		$\Delta Emp \geq 0$		High turnover		Low turnover	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔUI_t	0.002 [0.018]		-0.007 [0.014]		-0.001 [0.022]		0.003 [0.021]	
Low performance _t (=1)	-0.016*** [0.004]	-0.018*** [0.004]	0.003 [0.004]	0.004 [0.004]	-0.047*** [0.006]	-0.046*** [0.006]	-0.034*** [0.004]	-0.034*** [0.004]
$\Delta UI_t \times Low\ performance_t$	-0.060** [0.029]	-0.061** [0.029]	-0.025 [0.026]	-0.041 [0.027]	-0.090** [0.035]	-0.100*** [0.034]	-0.125*** [0.041]	-0.136*** [0.043]
Adj. R2	0.173	0.168	0.232	0.231	0.184	0.186	0.184	0.181
Obs.	22,229	21,998	40,149	40,000	30,155	30,003	28,632	28,463
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	yes	no	yes	no	yes	no	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

In columns 1-3 of Table C.1.2., we look at firms that show a lower than (SIC3-digit level) industry average employment growth. In columns 4-6, we look at firms that show employment growth lower than the overall sample average. While the effect sizes are slightly smaller ($\sim 0.099^{***}$ - 0.110^{***} vs. $\sim 0.077^{**}$ - 0.076^{*}), we find robust, consistent patterns.

Table C.1.2 Decreased Hiring *versus* Increased Layoffs: Firm-level Results

	$\Delta \text{Employment}_t$			$\Delta \text{Employment}_t$		
	Lower than industry average			Lower than sample average		
	(1)	(2)	(3)	(4)	(5)	(6)
ΔUI_t	-0.003			0.000		
	[0.020]	[0.000]	[0.000]	[0.020]	[0.000]	[0.000]
Low performance _t (=1)	-0.035***	-0.036***	-0.037***	-0.031***	-0.032***	-0.031***
	[0.005]	[0.004]	[0.005]	[0.004]	[0.004]	[0.004]
$\Delta UI_t \times \text{Low performance}_t$	-0.077**	-0.062*	-0.076*	-0.063*	-0.057*	-0.072*
	[0.036]	[0.032]	[0.040]	[0.035]	[0.033]	[0.037]
Adj. R2	0.124	0.096	0.119	0.112	0.082	0.108
Obs.	32,486	33,847	32,285	31,895	33,361	31,681
Controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	no	yes	yes	no	yes
State×Year FE	no	yes	yes	no	yes	yes

Appendix C.2. Temporary Layoffs

Feldstein (1976) proposes a model of risk-neutral workers and firms where UI expansions increase temporary layoffs during times of demand decline. Temporary layoffs generally refer to “short-term duration” unemployment spells that do not exceed the maximum UI benefit period. Feldstein (1978) and Topel (1983) document a cross-sectional pattern consistent with increased temporary layoffs in the March 1971 and March 1975 Current Population Survey, and van Doornik, Schoenherr, and Skrastins (2018) find evidence of increased layoffs and rehiring that coincide with UI eligibility and benefit expiration in Brazil.

We find that temporary layoffs are unlikely to drive our findings based on long-term (or permanent) layoffs. We measure employment change as a year-to-year change in the total number of employees reported in 10K filings at the end of the fiscal year, and any temporary layoffs that begin and end during the year should not affect the employment number. As the maximum UI duration is typically 26 weeks in our sample period, most temporary layoffs should fall in this category. To rule out increased temporary layoffs more formally as driving our results, we discuss three sets of results.

First, CPS data allows us to distinguish “indefinite layoff” from “temporary layoff,” and we find that our results (Table 5) are driven by indefinite layoffs. Second, we show that there is no upward reversion in firm employment that would appear if the temporary layoffs were driving our results. Specifically, we estimate equation (1) with UI expansions during previous years and find a uniformly insignificant coefficient for future lags of $\Delta UI \times \text{Low Performance}$ in Appendix B. Third, as an alternative check, Feldstein (1978) notes that temporary layoffs become more costly during times of strong labor demand when firms risk

losing workers to other firms. As shown below, we find a consistent pattern when we restrict the sample to state-years with unemployment rates less than four percent. The robust pattern across these subsamples indicates that temporary layoffs are unlikely to be a source of substantial bias.

In columns 1-2, we restrict the sample to *post-1982* when most states began to impose involuntary joblessness as a requirement to qualify for UI benefits (Solon, 1984). In columns 3-4 and 5-6, we restrict our sample to state-years when the state-level unemployment rate is higher than five and six percent, respectively. High unemployment rates increase the risk that a worker cannot regain employment even after the expiration of UI coverage (26 weeks) and discourage voluntary exits aimed at extracting rent from the UI system (Davis, Faberman, and Haltiwanger, 2012; Schmieder, von Wachter, and Bender, 2012; Kroft and Notowidigdo, 2016).

Table C.2. Differences across Labor Market Conditions

DV:	$\Delta \text{Employment}_t$					
	Sample year:		State unemp.		State unemp.	
	1983-2007		rate > 5%		rate > 6%	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔUI_t	0.018		0.004		0.007	
	[0.021]		[0.021]		[0.019]	
Low performance _t (=1)	-0.036***	-0.036***	-0.039***	-0.039***	-0.038***	-0.038***
	[0.004]	[0.004]	[0.005]	[0.005]	[0.006]	[0.006]
$\Delta UI_t \times \text{Low performance}_t$	-0.142***	-0.153***	-0.068**	-0.076**	-0.035	-0.057**
	[0.036]	[0.035]	[0.029]	[0.029]	[0.024]	[0.025]
Adj. R2	0.182	0.179	0.183	0.182	0.22	0.219
Obs.	53,338	53,237	66,718	66,591	27,707	27,633
Controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes
State×Year FE	no	yes	no	yes	no	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix C.3. Voluntary Exits (“Quits”)

CPS data allows us to distinguish involuntary layoffs from voluntary exits (Table 5). We discuss additional firm-level evidence against increased voluntary exits from UI expansions as driving our results. Carvalho, Corbi, and Narita (2018), Albanese, Piccio, and Ghirelli (2020), and Jäger *et al.* (2020) find that qualifying for UI eligibility increases voluntary exits that resemble increased layoffs in Italy, Brazil, and Austria, respectively.³¹ We do not expect moral hazard to be a primary channel in our empirical context: eligibility for UI benefits in the US requires joblessness to be “without good cause,” and increases in UI benefits in our sample center on the maximum weekly benefit *amount*, rather than UI *duration* or *eligibility*. States have historically differed in the stringency of UI eligibility, but we obtain consistent results when we restrict our sample period to *post*-1982 when most states began to require involuntary separation for UI eligibility (Solon, 1984). We also examine high state-level unemployment rates and industry-specific negative growth as constraints on voluntary exits. High unemployment and industry downturn make it more difficult to regain employment even when UI benefits expire and has the effect of discouraging voluntary exits (Schmieder, von Wachter, and Bender, 2012; Kroft and Notowidigdo, 2016). The argument is consistent with Davis, Faberman, and Haltiwanger (2012) who find that negative demand reduces voluntary exits (or “quits”). As shown above in Table C.2, we find consistent robust results across labor market conditions.

³¹ These studies also find increased voluntary exits to be driven by extensions in UI duration and by workers who recently crossed the eligibility threshold for collecting UI benefits. In the US during our sample period, UI expansions are driven by increases in the weekly maximum amount, rather than duration extensions, and we obtain consistent and sharper results using an alternative operationalization of UI expansions based only on increases in weekly maximum amounts.

APPENDIX D. UI Benefit Amount (vs. Change)

Expanding the analysis in Table 4, we here examine the role of UI benefit amount. Research on fairness assessment emphasizes a managerial focus on ΔUI , rather than UI amount, as driving moral licensing. We also expect UI amount to affect firm employment growth through a distinct channel, especially by slowing hiring (Hagedorn *et al.*, 2019; Johnston, 2021).

In Table D.1, we interact *Low Performance* with the level of UI benefits, not changes (i.e., ΔUI). In columns 1-4, the coefficient for UI_t is consistently negative, consistent with decreased hiring. Unlike the previous results on ΔUI (column 1 *versus* 4 in Table 2), the coefficient for UI remains negative and significant even after including its interaction with *Low performance* UI. The coefficient for $UI \times Low Performance$ is positive but the net effect on firm employment growth ($UI + UI \times Low Performance$) is negative and significant, as the negative direct effect of UI amount dominates the small positive effect from the interaction term $UI \times Low Performance$. The overall results, driven by the main effect of UI amount, are consistent with the general decrease in hiring. We divide our sample into positive and negative values of employment change in columns 5 and 6. The positive coefficient for $UI \times Low Performance$ is driven by firms that are expanding in column 5 with the null effect of UI in column 6.

Table D.1. UI Amount and Layoffs

DV:	$\Delta Employment_t$					
	All				$\Delta Emp_t \geq 0$	$\Delta Emp_t < 0$
	(1)	(2)	(3)	(4)	(5)	(6)
UI_t	-0.032*** [0.009]	-0.028*** [0.009]	-0.032*** [0.008]		-0.008 [0.009]	0.003 [0.016]
Low performance _t (=1)		-0.044*** [0.004]	-0.130*** [0.040]	-0.128*** [0.041]	-0.232*** [0.038]	0.027 [0.046]
$UI_t \times Low performance_t$			0.010** [0.004]	0.010** [0.004]	0.027*** [0.004]	-0.005 [0.005]
Adj. R2	0.176	0.183	0.183	0.181	0.233	0.173
Obs.	66,718	66,718	66,718	66,591	40,149	22,229
Controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes
State×Year FE	no	no	no	yes	no	no

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

In Table D.2., we show that prior split-sample results based on ΔUI either fail to reject the null or show an opposite, small positive coefficient with respect to $UI \times Low\ Performance$.

Table D.2. UI Amount and Layoffs: Split Sample Analyses

DV:	$\Delta Employment_t$											
	Republican CEO		External CEO		Takeover law		Debt		Short-term debt		Maturing long-term debt	
	Yes	No	Yes	No	Yes	No	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
UI_t	-0.057	0.052	-0.023	0.011	-0.040***	-0.039***	-0.024*	-0.042***	-0.026**	-0.035***	-0.029	-0.039*
	[0.035]	[0.034]	[0.038]	[0.054]	[0.013]	[0.013]	[0.013]	[0.013]	[0.012]	[0.013]	[0.023]	[0.023]
Low performance _t (=1)	-0.451	-0.007	-0.035	-0.765***	-0.084	-0.196**	-0.075	-0.141**	-0.175***	-0.189***	-0.169	-0.089
	[0.357]	[0.271]	[0.407]	[0.279]	[0.095]	[0.082]	[0.058]	[0.061]	[0.049]	[0.070]	[0.124]	[0.108]
$UI_t \times Low\ performance_t$	0.046	-0.002	0.002	0.080**	0.005	0.018*	0.003	0.012*	0.015***	0.017**	0.017	0.006
	[0.039]	[0.029]	[0.045]	[0.030]	[0.010]	[0.010]	[0.006]	[0.007]	[0.005]	[0.008]	[0.014]	[0.012]
Adj. R2	0.205	0.195	0.221	0.233	0.185	0.233	0.225	0.171	0.211	0.175	0.161	0.173
Obs.	5,162	8,195	4,670	2,138	39,505	25,382	27,798	35,506	34,536	28,146	16,767	13,891
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	no	no	no	no	no	no	no	no	no	no	no

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

APPENDIX E. Robustness Checks

We verify the robustness of our results to different empirical choices, including (1) alternative operationalizations of UI expansions based solely on changes in the maximum weekly benefit amount, (2) an alternative sample that excludes major industries in each state, (3) an alternative proxy for low firm performance, (4) alternative sample periods and eligible states, and (5) alternative specifications. We find uniformly consistent results.

TABLE E.1. Alternative Operationalization of UI Expansions and Sample Periods

Columns 1 and 2 operationalize ΔUI using only the changes in maximum weekly benefit amounts and disregard changes in maximum duration. Columns 3 and 4 exclude all control variables other than fixed effects. Columns 5 and 6 use inflation-adjusted (1990) nominal dollar amount increases in the maximum weekly benefit amount (*versus* percentage increases). Columns 7 and 8 set *Low Performance* to one if a firm's net income is negative. Columns 9 and 10 exclude state-years where the share of firms with low performance is above fifty percent. Columns 11 and 12 expand the sample period to 1971-2018. Columns 13-14 include firms employing less than 500.

DV:	$\Delta \text{Employment}_t$													
	$\Delta \text{Max.}$		No control variables		Inflation adjusted		Low performance =1		Excluding lagging		Sample year:		No size	
	payment only		other than FEs		nominal UI amount		if income<0		states		1971-2018		restriction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
ΔUI_t	0.009		0.022*		0.168		0.000		-0.009		-0.002		0.014	
	[0.014]		[0.011]		[0.740]		[0.007]		[0.021]		[0.010]		[0.015]	
Low performance _t (=1)	-0.039***	-0.039***	-0.036***	-0.036***	-0.039***	-0.039***	-0.060***	-0.060***	-0.044***	-0.044***	-0.037***	-0.036***	-0.075***	-0.075***
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.003]	[0.003]	[0.004]	[0.004]	[0.003]	[0.003]	[0.005]	[0.005]
$\Delta UI_t \times \text{Low performance}_t$	-0.103***	-0.112***	-0.082***	-0.093***	-0.043***	-0.048***	-0.085**	-0.079**	-0.063**	-0.070**	-0.051***	-0.054***	-0.064**	-0.062**
	[0.027]	[0.027]	[0.021]	[0.022]	[0.012]	[0.012]	[0.033]	[0.032]	[0.028]	[0.029]	[0.014]	[0.016]	[0.027]	[0.028]
Adj. R2	0.183	0.182	0.160	0.157	0.183	0.182	0.165	0.162	0.185	0.182	0.177	0.175	0.133	0.131
Obs.	66,718	66,591	92,545	92,479	66,718	66,591	66,718	66,591	46,054	45,945	98,989	98,791	131,380	131,334
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE E.2. Excluding Major Industries for Each State

Table F.2. modifies Table 2 and Table 3 and excludes state-industries that employ more than three percent of the state's workforce. We find consistent results. Using alternative cutoff values, such as five or two percent, makes little difference.

DV:	$\Delta \text{Employment}_t$							
IV:	Low		Chinese		Industry		Industry	
	firm performance		Import Competition		Value added		Revenue Growth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Negative shock _t	-0.039*** [0.004]	-0.039*** [0.004]	-0.023*** [0.005]	-0.022*** [0.006]	-0.029*** [0.005]	-0.027*** [0.005]	-0.017*** [0.002]	-0.017*** [0.003]
ΔUI_t	0.025 [0.016]		-0.070** [0.029]		-0.002 [0.020]		0.010 [0.020]	
$\Delta \text{UI}_t \times \text{Negative shock}_t$	-0.118*** [0.029]	-0.126*** [0.030]	-0.010** [0.005]	-0.012 [0.008]	-0.094* [0.053]	-0.115** [0.056]	-0.062** [0.028]	-0.057** [0.026]
Adj. R2	0.183	0.182	0.157	0.155	0.138	0.140	0.147	0.150
Obs.	53,657	53,541	8,790	8,719	17,535	17,399	45,283	45,179
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
Year	yes	yes	yes	yes	yes	yes	yes	yes
State×Year FE	no	yes	no	yes	no	yes	no	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE E.3. Controlling for the Level of UI for Table 2

Table F.3. modifies Table 2 and additionally controls for the level of UI benefits. We obtain highly consistent results. Using quartile, quintile, or decile values of UI yields robust results.

	$\Delta \text{Employment}_t$						$\text{Capital investment}_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UI_t	-0.029*** [0.010]	-0.028*** [0.009]	-0.024** [0.010]	-0.024** [0.010]			-0.003 [0.004]	
ΔUI_t	-0.014 [0.015]		-0.019 [0.015]	0.019 [0.016]			0.000 [0.004]	
$\text{Low performance}_t (=1)$		-0.044*** [0.004]	-0.044*** [0.004]	-0.039*** [0.004]	-0.037*** [0.004]	-0.039*** [0.004]	-0.008*** [0.001]	-0.008*** [0.001]
$\Delta \text{UI}_t \times \text{Low performance}_t$				-0.098*** [0.026]	-0.093*** [0.022]	-0.110*** [0.027]	0.004 [0.005]	0.004 [0.005]
Adj. R2	0.176	0.183	0.183	0.183	0.160	0.182	0.579	0.582
Obs.	66,718	66,718	66,718	66,718	67,569	66,591	66,166	66,039
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	no	yes	yes	yes
State×Year FE	no	no	no	no	yes	yes	no	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE E.4. UI Expansion and Layoffs in Response to Positive and Negative Performance

Table F.4. revises the binary *Low Performance* and form linear splines of firm performance relative to the industry performance benchmark, yielding two continuous variables: *Positive Performance* and *Negative Performance*. *Negative Performance* takes negative values by construction, and we take their absolute values for the ease of interpretation. Across columns 4, 5, and 6, the coefficient for $\Delta UI \times \text{Negative shock}$ is consistently negative and significant. In columns 4 and 6, the coefficient for $\Delta UI \times \text{Positive shock}$ is positive with economic significance similar to the coefficient for $\Delta UI \times \text{Negative shock}$. The positive interaction between ΔUI and *Positive shock* is consistent with making up for laying off fewer workers. The overall effect is that UI expansions increase the performance sensitivity of both hiring and layoff. In columns 7 and 8, we again do not observe any significant effects on firm capital investment.

DV:	$\Delta \text{Employment}_t$						$\text{Capital investment}_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔUI_t	-0.027*		-0.029**	-0.025			-0.001	
	[0.014]		[0.012]	[0.016]			[0.003]	
$\text{Positive Performance}_t$		-0.005	-0.005	-0.016	-0.074***	-0.017	0.020**	0.021***
		[0.031]	[0.031]	[0.030]	[0.024]	[0.030]	[0.008]	[0.008]
$\text{Negative Performance}_t$		-0.550***	-0.551***	-0.527***	-0.551***	-0.530***	-0.034***	-0.035***
		[0.022]	[0.022]	[0.023]	[0.024]	[0.024]	[0.005]	[0.005]
$\Delta UI_t \times \text{Positive Performance}_t$				0.280	0.043	0.424**	0.036	0.035
				[0.183]	[0.198]	[0.195]	[0.028]	[0.031]
$\Delta UI_t \times \text{Negative Performance}_t$				-0.533**	-0.540**	-0.487**	0.018	0.021
				[0.230]	[0.257]	[0.236]	[0.038]	[0.042]
Adj. R2	0.176	0.202	0.202	0.202	0.182	0.200	0.580	0.582
Obs.	66,718	66,474	66,474	66,474	67,329	66,346	65,926	65,798
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	no	yes	no	yes
State×Year FE	no	no	no	no	yes	yes	yes	yes

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

APPENDIX E.5. Geographical Dispersion in Firm Operation

We use the firm's state of headquarters, as reported in Compustat, to identify the effective UI expansions. However, some firms are geographically dispersed and maintain operations and employment across multiple states. UI expansions are measured with significant errors for these "multi-state" firms (Agrawal and Matsa, 2013; Flammer and Luo, 2017). As a partial redress, we obtain data on the state-level operations from Garcia and Norli (2012), who conduct a textual analysis of the 10-k filings to estimate the percentage of a firm's operations for each state. The sample window is limited to 1993-2007.

Column 1 is the full sample with information on the percentage of a firm's operations. Columns 2 and 3 divide the sample into high- and low-dispersion firms based on the median share of operations in the state of headquarters. We indeed find a much stronger positive relationship between UI expansions and layoffs in the low-dispersion sample.

	$\Delta \text{Employment}_t$		
	All	Dispersion	
		Low	High
ΔUI_t	0.006 [0.026]	0.036 [0.036]	-0.070 [0.048]
Low performance _t (=1)	-0.026*** [0.004]	-0.029*** [0.006]	-0.022*** [0.006]
$\Delta \text{UI}_t \times \text{Low performance}_t$	-0.196** [0.075]	-0.264*** [0.076]	-0.074 [0.102]
Adj. R2	0.183	0.200	0.166
Obs.	27,141	12,786	12,494
Controls	yes	yes	yes
Firm FE	yes	yes	yes
SIC3×Year FE	yes	yes	yes
State×Year FE	no	no	no

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

APPENDIX E.6. Dynamic Specification for Table 8

The table below adopts a dynamic specification from Bertrand and Mullainathan (2003) and decomposes the passage of Business Combination laws (BCL) into five separate periods with five indicator variables: BCL^{year-2} , BCL^{year-1} , BCL^{year0} , BCL^{year+1} , and $BCL^{year\geq 2}$. The variable of interest ($BCL \times \Delta UI \times Low\ performance$) achieves statistical significance only after the adoption of BCL with respect to employment growth, mitigating concerns of reverse causality. $BCL \times \Delta UI \times Low\ performance$ does not achieve statistical significance with respect to capital investment at forward and backward lags, mitigating concerns of omitted variable bias.

	$\Delta Employment_t$					Capital Investment _t				
	Run simultaneously					Run simultaneously				
	$n=-2$	$n=-1$	$n=0$	$n=1$	$n=2$	$n=-2$	$n=-1$	$n=0$	$n=1$	$n=2$
	(Before ⁻²)	(Before ⁻¹)	(After ^{t+0})	(After ^{t+1})	(After ^{t+2})	(Before ⁻²)	(Before ⁻¹)	(After ^{t+0})	(After ^{t+1})	(After ^{t+2})
Low performance _t (=1)			-0.046***					-0.009***		
			[0.004]					[0.001]		
$\Delta UI_t \times Low\ performance_t$			-0.020					0.001		
			[0.030]					[0.007]		
BCL_{t+n}	0.016	0.022*	0.041***	0.022*	0.015**	0.002	0.004	0.008**	0.008**	0.010***
	[0.011]	[0.012]	[0.009]	[0.012]	[0.007]	[0.003]	[0.003]	[0.004]	[0.003]	[0.003]
Low performance _t (=1) $\times BCL_{t+n}$	-0.019	0.004	-0.040*	0.001	0.014***	-0.004	-0.001	-0.003	-0.007**	0.001
	[0.015]	[0.019]	[0.022]	[0.015]	[0.004]	[0.004]	[0.003]	[0.003]	[0.003]	[0.001]
$\Delta UI_t \times BCL_{t+n}$	0.036	-0.063	-0.563***	-0.135	-0.013	0.001	-0.022	-0.050	-0.028	-0.017
	[0.185]	[0.101]	[0.179]	[0.188]	[0.051]	[0.048]	[0.018]	[0.040]	[0.032]	[0.018]
$\Delta UI_t \times Low\ performance_t \times BCL_{t+n}$	0.119	-0.051	0.388	-0.039	-0.174**	0.024	-0.006	-0.029	0.057	0.002
	[0.300]	[0.225]	[0.312]	[0.267]	[0.072]	[0.045]	[0.033]	[0.044]	[0.054]	[0.012]
Controls			yes					yes		
Firm FE			yes					yes		
SIC3 $\times$ Year FE			yes					yes		
State $\times$ Year FE			yes					yes		
Adj. R2			0.181					0.57		
Obs.			68,912					73,239		

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE E.7. Alternative Proxies of Financial Constraint

As a robustness check to Table 7 that uses debt ratio as a proxy for financial constraint, we use two alternative proxies: WW Index (Whited and Wu, 2006) and SA Index (Hadlock and Pierce, 2010). The statistical and economic significance of the licensing effect from UI expansions is consistently stronger in low-constraint firms that can afford to keep additional employees on the payroll.

DV: Financial Constraint:	$\Delta \text{Employment}_t$			
	SA Index		WW Index	
	High	Low	High	Low
	(1)	(2)	(3)	(4)
ΔUI_t	-0.006 [0.026]	0.017 [0.017]	0.007 [0.023]	0.012 [0.019]
Low performance _t (=1)	-0.066*** [0.006]	-0.009** [0.004]	-0.061*** [0.006]	-0.019*** [0.003]
$\Delta \text{UI}_t \times \text{Low performance}_t$	-0.058 [0.041]	-0.070** [0.030]	-0.074 [0.051]	-0.103* [0.052]
Adj. R2	0.199	0.202	0.232	0.160
Obs.	29,323	29,395	31,869	32,066
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes
State×Year FE	no	no	no	no

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE E.8. Financial Constraint, UI Expansions, and Capital Investment

We repeat the analysis in Table 7 using capital investment as the dependent variable. Consistent with the results in Table 2, the coefficient for the interaction term $\Delta UI \times Low\ performance$ consistently fails to reject the null in columns 3-4 that use short-term debt and columns 5-6 that use currently maturing long-term debt. As an exception, the coefficient for the interaction term is significant in column 2 that uses the overall debt ratio, but it is likely highly endogenous.

DV:	Capital investment _t					
	Debt		Short-term debt		Maturing long-term debt	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
ΔUI_t	-0.001 [0.004]	0.009** [0.004]	-0.006 [0.005]	0.006 [0.009]	-0.003 [0.007]	0.012 [0.011]
Low performance _t (=1)	-0.007*** [0.001]	-0.009*** [0.001]	-0.009*** [0.002]	-0.010*** [0.001]	-0.010*** [0.002]	-0.008*** [0.001]
$\Delta UI_t \times Low\ performance_t$	0.011 [0.012]	-0.012** [0.006]	-0.003 [0.006]	0.003 [0.009]	0.007 [0.009]	-0.017 [0.014]
Adj. R2	0.618	0.563	0.595	0.539	0.572	0.536
Obs.	29,839	37,508	36,462	30,217	17,468	14,946
Controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
SIC3×Year FE	yes	yes	yes	yes	yes	yes
State×Year FE	no	no	no	no	no	no

Notes: Standard errors are clustered at the firm and state level and reported in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.