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Finance Working Paper N° 728/2021

May 2022

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Abstract

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Keywords: Employees, Unemployment Insurance, Mergers and Acquisitions, Board of Directors

JEL Classifications: G32, G34, G38, J65

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Abstract

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1. Introduction

Mergers and acquisitions (M&As) intensify conflicts of interests between shareholders and employees in target firms (Dessaint, Golubov and Volpin, 2017). While shareholders usually receive large premiums for selling their shares, employees bear the brunt of post-merger layoffs (Maksimovic, Phillips and Prabhala, 2011). Thus, when deciding whether to accept an acquisition offer, target boards of directors face a tradeoff between securing an immediate gain for its shareholders and exposing its employees to a serious risk of post-merger layoffs. Existing studies generally assume that employee interests have minimal impact on target board decisions. This view stems from the shareholder primacy doctrine that is widely taught in finance textbooks. However, as many legal scholars point out, neither Delaware law nor the law of any state "enshrine a principle of shareholder primacy or precludes a board of directors from considering the interests of other stakeholders."[1] In fact, since the 1980s, court decisions in many states and the passage of state constituency statutes have explicitly allowed directors to consider the effects of their decisions on non-shareholder constituencies. Anecdotal and survey evidence from as early as the mid-1960s suggests that in practice a large majority of managers and directors would consider any sizable impacts of a takeover on its various stakeholders (Donaldson and Preston, 1995).[2] In this paper, we test if employee interests affect the decisions of target boards of directors to accept acquisition offers.

According to economic theory, employee interests can affect the decision of target boards of directors to accept acquisition offers through two potential channels. First, target boards of directors do so to fulfill their obligations under implicit contracts with employees (Shleifer and Summers, 1988). The success of a firm depends on the ex ante investments made by various stakeholders who enter into implicit long-term contracts with the firm. To encourage such investments, it is in the ex-ante interests of shareholders to ensure that the implicit contracts with other stakeholders are fulfilled ex post. Second, target boards of

[1] Martin Lipton, et al., 2019, "Stakeholder Governance and the Fiduciary Duties of Directors", *Harvard Law School Forum on Corporate Governance.*

[2] Explaining the recent change in the Business Roundtable's new statement of corporate purpose, Alex Gorsky, Chair of the Roundtable's Corporate Governance Committee, stated that "BRT has always maintained that investing in employees and communities is an essential part of generating value for shareholders. But, the fact is, words matter. And our own language was not consistent with the ways our member CEOs strive to run their companies every day."

directors do so to maximize shareholder welfare. Hart and Zingales (2017) argue that directors should maximize shareholder welfare, not shareholder value. These two objectives can deviate when a significant proportion of shareholders have pro-social preferences, in which case, the board should consider the social impact of an acquisition on its stakeholders besides its financial impact on shareholders. In addition to these two economic theories, it seems natural for target directors to look beyond the immediate shareholder gains when assessing acquisition offers, given that a firm has to effectively manage relations with its various stakeholders for its long term success. Target directors may also consider employee interests due to their own pro-social preferences. In this study, we test the hypothesis that in our sample period a sizable fraction of target boards give serious weight to the expected cost of unemployment of their employees when assessing acquisition offers.

Testing this hypothesis is challenging because we do not observe target board deliberations about acquisition offers. However, if we can observe an exogenous shock that changes the cost of unemployment borne by individual employees which does not alter the economic fundamentals that affect takeover activity, then we can test the above hypothesis by observing the change in acquisition likelihood of the shock affected firms following the exogenous shock. From a board of directors' perspective, the adverse impact of an acquisition on employees can be measured by the product of the pre-acquisition total number of employees, the percentage of employees expected to be laid off in a post-merger restructuring, and the average unemployment cost borne per employee.[3] Now, imagine that an exogenous shock reduces each individual worker's unemployment cost. Target boards can now be willing to accept some acquisition offers with a larger scale of expected post-merger layoffs than they would have accepted before the shock, since the total unemployment cost can be kept the same or actually reduced. Thus, this shock should lead to an increased willingness on the part of target boards to accept acquisition offers and, consequently, raise the overall acquisition likelihood.

[3] Or a measure that is highly positively correlated with total unemployment cost of employees calculated this way.

Although shocks to unemployment costs are in general difficult to find, changes in state-level public unemployment insurance (UI) benefits meet the key requirements of such exogenous shocks. First, prior studies show that UI benefits significantly reduce the unemployment costs per laid-off employee (Gruber, 1997; Agrawal and Matsa, 2013; Hsu, Matsa and Melzer, 2018). Second, UI benefits cover almost all employees in a firm. In the U.S., 97% of wage and salary workers are eligible to apply for UI benefits after involuntary separations from a firm. Third, there is large heterogeneity across states in the timing and size of UI adjustments. Lastly, changes in UI benefits must be approved by the state legislature. Due to the uncertain level of legislative support for these changes, the timing of state-level UI changes is largely exogenous to the state's economic fundamentals. These staggered state UI changes thus provide an ideal setting for testing whether employee unemployment costs affect target boards' decisions to accept acquisition offers.

Our target firm sample consists of all U.S. public firms in the CRSP-Compustat merged database over the 1986-2018 period, excluding heavily regulated utilities and financial service firms. We begin our sample period in the second half of the 1980s since target firms are better able to control takeover bid outcomes after 1986 when many states passed antitakeover statutes and when shareholder rights plans were recognized by state courts. Following Agrawal and Matsa (2013) and Hsu, Matsa and Melzer (2018), we calculate a state's UI benefit level in a year as the maximum total UI benefits an unemployed worker is eligible for under the UI schedule for that state-year. We treat the UI level in a target firm's headquarters state as the unemployment benefits available to its employees.

Our main specification is a firm-year panel regression of an acquisition indicator on the UI level in the target firm's headquarters state. We include state-industry, industry-year, and region-year fixed effects. We also explicitly control for time-varying state economic conditions and political uncertainty because they may be simultaneously related to state UI levels and takeover activity in the state. We find that the coefficient on the target state UI level is positive and statistically significant, suggesting that increases in the target state UI level raise the takeover likelihood for firms in these states. In terms of economic magnitude, a $1,000 increase in UI level raises the takeover likelihood of a firm in the state by about 40

basis points or 10% over the sample's unconditional takeover likelihood of 4% per year. This result supports the hypothesis that employee unemployment costs affect target board decisions about takeover offers.

Although we carefully show that state UI changes are exogenous to state economic conditions that may otherwise affect takeover activity, we conduct a variety of additional tests to address any remaining concerns that our result is endogenous. The fixed effects we include in our main specification suggest that our results are not driven by time-invariant heterogeneity at the state-industry level or unobserved industry trends or regional trends. Our results are also not driven by reverse causality since we find no relation between state UI levels and contemporaneous and lagged state takeover frequencies in state-panel regressions. However, there are two remaining concerns about our baseline result. First, a time-varying omitted variable at the state level could be correlated with both the change in UI level (hereafter UI change for brevity) and takeover activity. Second, the exogeneity of UI changes is mainly in its timing because eventually, UI levels must adjust to inflation, rising wage levels, and rising living costs. Thus, only if the change in acquisition likelihood occurs shortly after UI changes can we reliably attribute it to UI changes.

To address these two issues, we conduct a difference-in-differences analysis of the effect of a large UI increase on acquisition likelihoods in event time. A large UI increase is defined to be an increase in UI level of more than 10% over the prior year's level. We then compare the change in acquisition likelihood over the [-4,+4] year event window for firms in a state with a large UI increase event with that for firms in bordering counties of other states without a large UI increase event. Using firms in bordering counties as control firms mitigates concerns that our results are driven by unobserved local economic conditions. In addition, the use of large UI increase events allows us to analyze the timing of the changes in acquisition likelihood relative to the year of the large UI level shock. We find that the acquisition likelihood rises for firms experiencing a large UI level increase event relative to that of control firms beginning one year after the event and remains significantly higher for two of the next three years.

To relate our main result to target board concerns about employee interests, we exploit the staggered adoption of constituency statutes by 35 U.S. states during our sample period as a source of exogenous variation in the likelihood of target boards considering employee interests. These statutes authorize target

firm directors to consider the impact of an acquisition on employees and other non-shareholder stakeholders when evaluating acquisition offers. Prior studies find that the adoptions of constituency statutes generally increase boards' stakeholder orientation (Flammer and Kacperczyk, 2016; Cremers, Guernsey and Sepe, 2019; Gao, Li and Ma, 2021). To the extent that constituency statutes encourage more boards to consider employee interests when assessing an acquisition offer, we should observe a rise in the target state UI level-acquisition likelihood relation for target firms incorporated in constituency statute states post-adoption. Our finding confirms this prediction. We find that the adoption of a constituency statute increases the UI effect on acquisition likelihoods by 42%. Since state constituency statute adoption is exogenous for almost all firms incorporated in a state, our result is unlikely to be driven by unobserved changes in firms incorporated in constituency statute states that somehow strengthen the UI level-acquisition likelihood relation other than by a change in the target board's stakeholder orientation. Hence, the result suggests a causal link from target board consideration of employee interests to a UI effect on acquisition likelihoods.

Our hypothesis does not assert that all corporate boards seriously consider employee interests when evaluating acquisition offers. We next shed light on the question of what types of boards of directors are more likely to consider employee unemployment costs when assessing acquisition offers. Guided by existing economic and social theories and evidence, we identify five firm characteristics that are likely to be positively correlated with the board's propensity to consider the impact of UI on unemployed workers.

First, we conjecture that boards of firms in labor intensive industries are more likely to consider employee interests than are boards of firms in capital intensive industries. This is because employees in labor intensive industries are in general exposed to greater risk of post-merger layoffs and they tend to have lower skills and earn lower wages than those in capital intensive industries. Thus, UI changes are likely to have a larger impact on the lives of unemployed workers in labor intensive industries.

Second, we conjecture that boards of firms with larger fraction of short-term institutional ownership are less likely to consider employee interests when assessing an acquisition offer. This follows since short-term institutional investors are more willing to tender their shares to hostile bidders, which weakens a board's ability to resist a takeover bid (Gaspar, Massa and Matos, 2005). In addition, short-term institutions

are more likely to put pressure on the board to break any implicit contracts with current employees so that they can earn an immediate takeover premium.

Third, we conjecture that target boards of firms headquartered in less populated counties are more likely to consider employee interests for several reasons. First, managers and local board members are more likely to have stronger social connections with employees and their families in smaller communities (Landier, Nair and Wulf, 2009). Second, smaller communities can foster a cooperative culture where managers and employees mutually support and take care of each other (Steblay, 1987). Third, the layoffs by a firm headquartered in a less populated county are likely to have larger negative impacts on the local economy than similar size layoffs by a firm headquartered in a large metropolitan area. Thus, a board is more likely to consider an acquisition's negative impact on the local community when a firm is headquartered in a less populated county.

Fourth, we conjecture that the boards of firms headquartered in counties with a higher level of social capital are more likely to consider employee interests when evaluating acquisition offers. Guiso, Sapienza and Zingales (2011) define social capital to be the shared values and norms that encourage cooperation and limit opportunistic behaviors. Jha and Cox (2015) empirically show that this definition of social capital is a useful measure of a board's altruistic inclinations measured by CSR ratings. The level of social capital in a county is likely to be positively related to the likelihood that boards in local firms consider employee interest in acquisition decisions due to both the congruence of social preferences of local managers and directors with local norms, and community pressure exerted on a board to behave in a prosocial way.

Lastly, we conjecture that boards with female independent directors (IDs) are more likely to consider employee interests. The social psychology literature documents that women are more community-minded and more caring about others than men (Eagly and Crowley, 1986). Experiments also find that women are more altruistic and long-term oriented than men (Andreoni and Vesterlund, 2001; Silverman, 2003). However, given concerns about the selectivity of board members, it is important to examine evidence of corporate director preferences. Adams, Licht and Sagiv (2011) find that female directors are more likely to side with non-shareholder stakeholders when conflicts of interest between shareholders and other

stakeholders exist. Thus, we expect firms with female IDs to more frequently consider employee interests in acquisition decisions.

Consistent with the characteristics we identify above meaningfully capturing variations in the likelihood of the board to consider employee unemployment costs, we find that the UI effect is more pronounced in firms with higher labor intensity or headquartered in counties with low population or in counties having a high level of social capital. On the other hand, the UI effect is significantly weaker for firms with high ownership levels by short-term institutional investors. We also find a stronger UI effect in firms with female IDs where the UI effect rises as the number of female IDs increases above one.

According to our hypothesis, the main reason for the rise in acquisition likelihood following increases in target UI levels is that a target board is more willing to accept acquisitions with larger expected post-merger layoff plans. This suggests that higher UI levels should be associated on average with larger scale post-merger layoffs. Consistent with this prediction, we find that a target state's UI level in the year prior to a takeover announcement is positively associated (at the 5% significance level) with the fall in the employee headcount of the combined acquirer and target firms, measured from two years before through three years after deal completion. Moreover, as expected, this relation is driven by within-industry acquisitions for which workforce reductions and restructurings are a more important source of synergies. Hence, not only are the acquisition likelihood results consistent with our hypothesis, but the size of post-acquisition layoffs is consistent as well.

While so far we have focused on target boards, prior studies suggest that labor unions in target firms can also affect takeover outcomes (Pagano and Volpin, 2005; Tian and Wang, 2021). Although none of the studies suggests that labor union oppoisition to acquisitions may vary with union member unemployment costs, if this is the case, then an alternative mechanism to explain the positive correlation of acquisition likelihoods with UI levels would be suggested, namely that labor unions soften their opposition to acquisitions after UI benefits are enhanced.

To assess the validity of this alternative explanation, we divide firms into high and low union coverage groups based on their industry union coverage rates and estimate a regression in which we allow UI effects

to vary across these two groups. We find that the coefficient of the interaction term of UI level and the high union coverage indicator is positive and statistically significant, suggesting that labor unions can explain some of the UI effect we document. However, since the UI coefficient for firms in the low union coverage group is also statistically significant and its magnitude is only slightly smaller than that in our baseline regression, we conclude that a target board's concern for its employees still plays an important role in driving the target UI level-acquisition likelihood relation. Moreover, union opposition cannot explain our findings concerning the adoption of constituency statutes since these statutes do not change a labor union's incentives to interfere with target board acquisition offer decisions. It is also difficult for labor union opposition to explain cross-sectional variations in UI effects across different types of firms. Overall, while labor unions appear to respond to UI changes and thus, play a role in explaining our baseline relation, they do not negate the effect of target boards' concern for their employees when responding to acquisition offers.

In the online appendix, we report an array of robustness tests to strengthen the evidence supporting our conclusions. We find that our main result continues to hold after controlling for various measures of manager-shareholder conflicts of interests in target firms and the effects of UI changes on firm leverage and CEO incentive pay. We also find that the relation between target UI levels and acquisition likelihoods is mainly driven by within-industry acquisitions rather than diversifying acquisitions. Furthermore, we find that the positive relation between target state UI level and acquisition likelihood and the effect of constituency statutes on strengthening this relation are observed when we aggregate acquisitions to the state level. Lastly, the UI effect is also more pronounced in industries that employ more low-skilled workers who face higher post-merger layoff risk and rely more on UI benefits for living during unemployment spells than do high-skilled workers.

1.1 Relate Literature

Our study challenges a dominant view in the M&A literature that target firm directors only consider the interests of shareholders and management (including directors) when assessing an acquisition offer. Our evidence suggests that expected employee unemployment costs also affect the decisions of a sizable proportion of boards to accept acquisition offers. As individual unemployment cost falls, these boards

become more willing to accept acquisitions with larger layoff plans, which increases the probability that shareholders will receive takeover premiums. This brings the finance literature closer to the management literature, which has a long history of considering various stakeholder interests (Donaldson and Preston, 1995), and the observations of many practitioners that management (including directors) in practice weigh the interests of non-shareholder stakeholders when making major decisions. Consistent with this view, the Business Roundtable changed its corporate purpose statement in 2019 to state that the purpose of a corporation is to deliver value to all its stakeholders. Many outside observers and the media acclaim the new statement to be a fundamental departure from how companies were run in the past, while expressing skepticism that there would be any real departures from the previous narrow focus on maximizing shareholder value. Nevertheless, many CEOs of the signatory companies consider the change as far less dramatic because they claim that the new statement simply better reflects existing practices in running their companies.[4] Our evidence lends some support to their claims.

Our study also contributes to the literature examining the effect of employment protections and employee rights on takeover outcomes. Dessaint, Golubov and Volpin (2017) find that strong national employment protection laws significantly reduce domestic takeover activity as well as the expected synergies resulting from these acquisitions. Tian and Wang (2021) find that strong employee rights from unionization reduce the attractiveness of a firm as an acquisition target, and shareholders in these firms receive lower takeover premiums when their firms are acquired. John, Knyazeva and Knyazeva (2015) report that strong labor rights at U.S. acquirers are associated with lower acquirer returns. Although these studies find that employment protection laws and unions reduce takeover market efficiency, the protection they provide to employees may yield other benefits to society. Our evidence suggests that UI can be an attractive alternative means of providing employee protection than imposing legal restrictions on layoffs

[4] Alex Gorsky, CEO of Johnson & Johnson and Chair of the Business Roundtable Corporate Governance Committee wrote: "BRT has always maintained that investing in employees and communities is an essential part of generating value for shareholders. But, the fact is, words matter. And our own language was not consistent with the ways our member CEOs strive to run their companies every day." (Source: https://www.linkedin.com/pulse/why-business-roundtable-redefined-purpose-corporation-alex-gorsky/)

and dismissals or empowering employees to oppose the management since it also increases acquisition activity. This insight is similar in spirit to Pagano and Picariello (2021), who show that UI dominates legal protections against dismissals in protecting workers against layoffs in talent-intensive industries since employee protection laws penalize talent-intensive firms and thus, depress a firm's expected productivity.

Our study also contributes to the vast literature that examines the effect of unemployment insurance on individual behavior, firm decisions, and the economy as a whole. UI is found to distort labor supply (Meyer, 1990; Katz and Meyer, 1990; Chetty, 2008), but smooths individual consumption (Gruber, 1997), reduces mortgage defaults (Hsu, Matsa and Melzer, 2018), attenuates the effect of adverse labor demand shocks on the local economy (Di Maggio and Kermani, 2015), and spurs entrepreneurial activity (Hombert *et al.*, 2020). For firm decision-making, Agrawal and Matsa (2013) find that more generous UI benefits increase firm leverage, while Ellul, Pagano and Schivardi (2018) report that firms provide less employment stability to employees when public UI benefits are higher. Our study highlights another efficiency benefit of UI: the improved efficiency of the takeover market.

2. Unemployment Insurance

2.1. Institutional Background

The unemployment insurance system in the U.S. provides temporary income to eligible workers who are laid off from their jobs. It is one of the largest social security programs in the U.S. by the level of spending and it covers more than 97% of U.S. wage and salary workers. The primary objective of UI programs is to alleviate hardships that result from a loss of wage income during periods of unemployment.

The UI system is organized as a joint federal-state system where states have extensive flexibility in determining the parameters of their UI programs, such as the eligibility, weekly benefit amount, and the maximum duration of the benefits. State legislatures set UI program provisions by passing changes to state UI laws. Due to this state-level flexibility, states differ significantly in their average weekly UI benefits and, to a lesser extent, in their eligibility criteria, although most states limit the duration of benefits during our sample period to 26 weeks. UI benefits are mainly provided under this regular program. However, during periods of high state-level unemployment, unemployed workers may be eligible for extended benefits.

Since the provisions for this additional benefit is endogenously related to the state-level unemployment rate, we exclude them in our calculation of UI levels to maintain the exogeneity of our measure of UI changes.

UI payments are primarily financed by unemployment insurance premiums paid by employers. The premiums are collected and aggregated over time into state trust funds and a federal trust fund. When workers file claims after being laid off, UI benefits are first paid out of the state trust fund, and after it is exhausted, the state can then tap into the federal trust fund for additional support. A company's UI premiums are experienced rated in that employers whose workers filed more claims in the past are supposed to pay higher rates, where premiums are paid on the "taxable wage base", which is usually a certain percentage of overall wages.[5]

Prior studies find that UI benefits have a substantial impact on unemployed workers. Meyer (1990) finds that higher UI benefits reduce the incentives of unemployed workers to leave unemployment because of the lower opportunity cost of continued job search and leisure. Specifically, he finds that a 10% increase in UI benefits is associated with an 8.8% decrease in the hazard rate of an unemployed worker leaving unemployment. Gruber (1997) finds that UI provides significant consumption smoothing benefits to workers. His estimate shows that in the absence of UI, the consumption of the unemployed would fall by over three times the average fall in the presence of UI. Hsu, Matsa and Melzer (2018) find that a $3,600 increase in the UI level reduces the average layoff-related rise in mortgage delinquency by 13%.

2.2. Exogeneity of UI changes

At the outset of state UI programs in the late 1930s, most states targeted a benefit level that replaces about 50% of an individual's prior wages for a limited duration. Over time, these levels began to lag behind wage levels, the inflation rate, and costs of living. Consequently, state legislatures periodically adjusted UI benefit levels, mostly upwards. However, since the passage of UI law changes is a legislative process, whose initiation and outcome are influenced by many non-economic factors, it causes the timing and size of these adjustments to deviate substantially from underlying economic fundamentals in the state. In other

[5] See https://tcf.org/content/commentary/increasing-taxable-wage-base-unlocks-door-lasting-unemployment-insurance-reform/?agreed=1 for more information.

words, UI changes within a state are largely exogenous to a state's economic conditions. Given its importance for identification, we next provide evidence to support the validity of this assumption.

Following Agrawal and Matsa (2013) and Hsu, Matsa and Melzer (2018), we measure the UI benefit level in a state-year by the maximum total UI benefits that an unemployed worker is eligible for under the UI schedule for that state-year. In Figure 1, we plot changes in UI level and GDP per capita in U.S. states over four equally divided subperiods between 1986 and 2018. States are classified into quartiles based on changes in their UI levels and GDP per capita growth rates over each subperiod respectively. One clear observation from the figure is that there is no discernible relation between UI growth and GDP per capita growth at the state level for any of the subperiods. For example, in 1993-2000, California and its bordering states (OR, NV, AZ) all exhibited top quartile GDP per capita growth; however, their UI level growth rates ranged from the bottom to the third quartiles. In particular, California was in the top quartile of GDP per capita growth, but its UI level was unchanged over this subperiod.

In Table 1, we estimate individual correlations of state UI levels with state GDP, state unemployment rate, state union coverage, and gubernatorial elections after conditioning on state and year fixed effects. The results, which are reported in columns 1-5 of Table 1, show no evidence of any significant correlation between changes in state UI level and changes in any of these measures of statewide macroeconomic conditions and political uncertainty during our 1986-2018 sample period. This evidence suggests that the changes in UI levels are exactly the type of exogenous shocks that we need for testing our hypothesis: they change employee unemployment costs but are largely uncorrelated with state economic conditions that could affect takeover likelihoods. However, a remaining concern is that greater takeover activity in a state may prompt the state legislature to raise UI benefits. If this is the case, then any positive relation between state UI level and acquisition likelihood could be driven by reverse causality. In columns 6-10 of Table 1, we show in state-panel regressions that state UI levels are not correlated with contemporaneous or lagged statewide acquisition ratios of the firms headquartered in the state based on the Compustat database. Thus, we conclude that this form of reverse causality does not undercut the validity of our experiment.

3. Data and Empirical Framework

3.1. Data and Sample

Our sample consists of all firm-year observations over the 1986 - 2018 period in the CRSP-Compustat merged database where a firm must be listed on NYSE, AMEX or NASDAQ and is not in the highly regulated financial or utilities industries. We begin our sample period in 1986 since the adoption of antitakeover laws and judicial recognition of shareholder rights plans by many states occurred by this date, which made bid acceptance by target boards almost indispensable for bid success. This condition is important given our interest in how employee unemployment costs affect a target board's response to a takeover bid. Firm financial statement data are taken from S&P's Compustat database and corporate governance data are from the Institutional Shareholder Services (ISS) database.

We obtain data on each state's UI benefit schedule from the U.S. Department of Labor's "Significant Provisions of State UI Laws". Each schedule specifies the weekly benefit amount based on a worker's prior weekly wage over a base period and the maximum duration of these benefits. In general, the higher the worker's prior wage, the higher the weekly benefit amount, but limited up to a maximum cap. Following Agrawal and Matsa (2013), we measure the overall UI benefit level in a state-year by the product of the maximum weekly benefit amount and the maximum duration of benefits under the regular UI program of each state. Agrawal and Matsa (2013) and Hsu, Matsa and Melzer (2018) find that increases in this measure are associated with greater aggregate state UI payouts and thus it is a useful measure of the level of UI benefits expected by terminated workers.

We match state UI levels with firms based on a firm's historical headquarters state in the matching year.[6] Prior studies such as Henderson and Ono (2008) and Pirinsky and Wang (2006) find that a firm's major plants and operations are usually close to its headquarters.[7] Hence, the UI level in a firm's headquarters state should be a good measure of UI benefits available to employees of most firms in our

[6] The decision to change headquarters states is endogenous and could be influenced by state UI levels. Thus, for robustness, we rerun our main tests excluding these observations and we find that our main results remain unchanged. There are only 4,193 firm-year observations (out of 85,732) associated with firms changing headquarters states.

[7] According to UI laws, UI claims should be filed in the state where a claimant works. However, since we do not have establishment level data, we use UI benefits in a firm's headquarters state as an approximation.

sample. A small number of industries are known for having geographically dispersed workforces. Using headquarters-state UI levels for firms in these industries could introduce larger measurement errors. Econometrically, this measurement error should attenuate the UI level coefficient estimates, thus biasing against finding a significant UI coefficient. Given that we find a significant UI coefficient, it suggests that the relation between target UI level and acquisition likelihood is very strong. However, for robustness, we rerun our regressions in most tables excluding firms in the three industries known to have very geographically dispersed workforces, i.e., wholesale, retail and transportation industries, following Agrawal and Matsa (2013). Our results are generally strengthened when adding this data filter.

To identify firm-years where a firm is the target of an acquisition, we use the Securities Data Corporation (SDC)'s mergers and acquisitions (M&A) database. We begin with all completed M&A deals between U.S. public firms announced between January 1, 1986, and December 31, 2018. Following the prior literature (Moeller, Schlingemann, and Stulz, 2004; Masulis, Wang and Xie, 2007), we require a deal to be valued at $1 million or more and the acquirer to own less than 50% of a target's outstanding shares before the bid and own 100% at deal completion. We exclude self-tenders, exchange offers, repurchases, spinoffs, leveraged buyouts (LBOs), and recapitalizations. Deals that meet the above criteria are included in our CSRP-Compustat firm-year panel dataset in the deal announcement year and our key dependent variable, *Acquisition*, is set to one. For firm years not associated with a deal announcement, the dependent variable, *Acquisition*, is set to zero. After eliminating observations with missing firm and deal characteristics, the final panel dataset consists of 85,732 firm-year observations, involving 3,477 acquisitions. The unconditional likelihood of an eventually successful acquisition is 4% per annum.

Lastly, we control for several state-level macroeconomic variables and an indicator for gubernatorial election years.[8] State monthly unemployment rates are obtained from the monthly US Current Population Survey database. We then take the average of the monthly rates to obtain the average unemployment rate

[8] Jens (2017) finds that political uncertainty in the period prior to U.S. gubernatorial elections affects both state-wide investment and financing activities.

for the year. State GDP per capita data are obtained from the U.S. Bureau of Economic Analysis. Gubernatorial election year data are collected from internet searches.

Table 2 reports the descriptive statistics of our firm-year sample. The summary statistics for all variables are largely consistent with prior work. UI levels unadjusted for inflation have a mean (median) of $9,626 ($8,606) and a standard deviation of $4,600. The mean (median) natural logarithm of the UI levels is 9.08 (9.06). For brevity, we omit further discussion of them. In Table 3, we report the summary statistics for UI levels by state. The table shows significant differences across states in mean UI levels. Within a state, UI levels also exhibit large time-series variations as indicated by the large standard deviations shown in column 5. Although most within-state UI changes are moderate, large changes (defined as a change exceeding 10% of the UI level in the prior year) are not uncommon (see column 6). Among the top five headquarters states by the number of firm-year observations in our sample, California had five large adjustments, New York had four, Massachusetts and Texas each had two, and Illinois had one.

3.2. Empirical Model

We conduct our analysis at the firm level so that we can control for firm characteristics to mitigate the effect of differences in firm characteristics across years on takeover likelihoods. Our regression model is specified as a linear probability model as described below:

$$y_{ijst} = \beta_1 UI_{s,t-1} + \beta_2 X_{i,t-1} + \beta_3 Z_{s,t-1} + \mu_{sj} + \tau_{jt} + \varepsilon_{ijst} \quad (1)$$

where *i* indexes firm, *j* indexes industry, *s* indexes headquarters state, and *t* indexes year. The dependent variable y_{ijst} is an indicator that equals 1 if firm *i* is a target of an acquisition bid announced in year *t*, and 0 otherwise. $UI_{s,t-1}$ is our key explanatory variable, defined as the UI level ('000 $) in state *s* for year $t-1$.

The control variables in this model are separated into two groups. The first group, $X_{i,t-1}$, contains firm characteristics found in earlier work to be correlated with takeover likelihoods, such as firm size, firm age, market-to-book ratio, etc. (e.g., Song and Walkling, 2000; Jenter and Lewellen, 2015; Dessaint et al., 2017). This group also includes corporate governance attributes such as staggered board, board independence, and CEO ownership when this data is available. The second group, $Z_{s,t-1}$, contains time-varying state economic

conditions and political uncertainty, which we measure by the state unemployment rate, real GDP per capita, and a gubernatorial election-year indicator. μ_{sj} and τ_{jt} are state-industry and industry-year fixed effects, respectively. Since acquisition activities typically vary by industry, we control for time-invariant heterogeneity at the state-industry level rather than simply at the state level. Since the main source of variation is at the headquarters state level, we cluster standard errors by headquarters state to account for potential cross-firm correlations within a state (Petersen, 2009).

We choose the linear probability model over a logit or probit model because we are only interested in the marginal effects. For estimating marginal effects, these models perform similarly, but the linear probability model offers some advantages in ease of interpretation of marginal effects and flexibility regarding the inclusion of high-dimensional fixed effects. In robustness, if we estimate a logit or probit model, we include at least state and year fixed effects, but we do not rely on these as our primary estimates since it is well known that logit or probit models with fixed effects work poorly under serially correlated error terms, which are common in corporate finance data.

4. UI Level and Acquisition Likelihood

4.1. Preliminary Evidence

Before we estimate the linear probability model specified in Equation (1), we plot the average abnormal annual change in the state-level acquisition ratio against the average abnormal annual change in the state's UI level in each state which is shown in Figure 2. The state-level acquisition ratio is defined as the proportion of firms headquartered in the state according to the Computstat database that are acquired in a given year. To reduce noise in the state-level acquisition ratio, we exclude state-years with fewer than 10 Compustat firms.[9] The abnormal annual changes in the state-level acquisition ratio equal the residuals from a regression of annual changes in the state-level acquisition ratio on year fixed effects. The abnormal annual changes in state UI level are calculated similarly as residuals from a regression of annual changes in the state UI level on year fixed effects. Figure 2 shows a clear positive relation between the two variables,

[9] This sample restriction excludes 67 state-year observations from our sample.

which is consistent with a state UI level increase leading to a higher fraction of firms being acquired in the state on average. This simple correlation is only suggestive, but it does not provide evidence of a causal relation since it could easily reflect other differences across states. To provide stronger evidence, we estimate a linear probability model in which we include firm and state controls and various fixed effects.

4.2. Baseline Results

We begin by estimating a version of Equation (1) that excludes the controls for state economic conditions and gubernatorial elections. We report two estimates. The first is estimated using all firms, while the second is estimated after excluding firms in industries known to have geographically dispersed workforces. Table 4 presents the results. We find that the coefficient of the target state UI level is positive and statistically significant at the 5% level in both columns 1 and 2. The size of the coefficient estimate is slightly larger in column 2. This is consistent with the UI levels in column 2 containing smaller measurement errors due to the exclusion of firms in industries known to have geographically dispersed workforces.[10] We then add state-level controls to the regression in columns 3 and 4 and re-estimate Equation (1). Although UI changes are mostly exogenous, controlling for these state-level variables further mitigates the concern that some UI changes may contain residual correlations with the state's macroeconomic conditions. We find that both the statistical and economic significance of the UI level coefficient rises with these added statewide controls. These results support the prediction of our hypothesis and suggest that a sizable proportion of boards of directors consider employee unemployment cost when evaluating acquisition bids.

The coefficient of 0.004 on the target state UI level in column 3 suggests that, for a $1,000 increase in the state UI level, the likelihood of a firm headquartered in the state being acquired rises by 40 basis points. Given the unconditional acquisition likelihood for our sample is 4%, this represents a 10% increase in a typical firm's acquisition likelihood. To put the magnitude of this estimate in perspective, we briefly review some findings in the M&A and UI literature. Among M&A studies, Tian and Wang (2021) find that firms

[10] This pattern also holds in most of our later reported tests. For brevity, we refrain from discussing these comparisons hereafter. This pattern is consistent with the well-known attenuation bias associated with measurement errors.

whose employees vote to unionize receive 48% fewer takeover bids in the subsequent 3 years. Jenter and Lewellen (2015) find that CEOs of retirement age (i.e. aged 64-66) are 32% more likely to sell their firms than CEOs who are just below the 65 retirement age. Machlin, Choe and Miles (1993) find that golden parachutes increase the odds of CEOs selling their firms by 114%. Since the estimate in Machlin, Choe and Miles (1993) is based on a logit model, we also estimate a logit model to make our estimate easily comparable to theirs. Our logit estimate suggests that a $1000 increase in the state UI level raises the odds of a firm in the state being acquired by about 10%. Hence, the effect of a $1000 increase in UI level on the takeover likelihood is significantly smaller than that of these other factors being studied.

Among UI studies, Meyer (1990) finds that a 10% increase in the weekly UI benefits leads to an 8.8% fall in the hazard rate of leaving unemployment. Gruber (1997) finds that, at the individual level, a 10% increase in a worker's weekly UI benefits relative to her average weekly wage before unemployment reduces the drop in her consumption upon unemployment by 40% of the sample average drop. Hsu, Matsa and Melzer (2018) find that a one-standard-deviation increase in the maximum UI benefits ($3,600) reduces the likelihood of mortgage delinquency among laid-off workers by 12% of the lay-off related rise in the delinquency likelihood. These findings suggest that a 10% change in UI benefits, although it appears to be small, can have a significant marginal effect on the quality of life of unemployed workers. Thus, it appears reasonable that a $1000 increase in UI level can change a target board's concern for its employees. Overall, this brief review suggests that the economic magnitude of the estimated UI effect appears plausible.

4.3. The Timing of the UI effect Around Large UI Level Increases

The exogeneity of the UI changes that we exploit is mainly in their timing since over a long period they should tend to track economic fundamentals. One indication of this is that nominal UI levels move up over time in all states. Keeping the benefit levels commensurate with economic fundamentals is likely to be a major driver of this upward trend. However, the timing of the adjustments does not follow economic fundamentals closely due to the noise in the state legislative process. It is this noise that provides us with the exogeneity in timing. For example, California's UI level was not adjusted for almost a decade, then in 2002 California suddenly raised its UI level by 43%. This increase in UI level represents an exogenous

decrease in unemployment costs of laid-off workers in California, especially in the first few years after the UI change. Hence, if UI changes cause changes in acquisition likelihood, the changes in acquisition likelihood should appear relatively soon after the UI changes. If instead, the change in acquisition likelihood is delayed by many years after the UI change, then changes in economic fundamentals over the intervening years can confound the result.

In our baseline regressions, the UI level is lagged by one year relative to the dependent variable, *Acquisition*, which means we are assuming that UI change in year t-1 will have an immediate effect on acquisitions in the following year t. This specification, however, does not allow us to observe the precise timing of the UI effect. In this section, we focus on large UI change events and estimate the UI effect in event years by using a difference-in-differences model for estimating dynamic effects. This model allows us to observe the timing of the UI effect and thus, test the prediction that the UI effect should appear relatively soon after the UI changes as our hypothesis predicts.

Another endogeneity concern that we have not yet addressed is that a time-varying omitted variable at the state level is correlated with both state UI changes and time-varying state acquisition activity. Although the inclusion of the U.S. census region by year fixed effects in Equation (1) partially mitigates this concern, the U.S. census region is quite large, so this can allow significant heterogeneity in economic shocks across states in the same U.S. census region. Including state-year fixed effects would completely subsume the coefficient on the target state UI level and thus, this is not a solution. To also address this concern, we require that the control firms in this difference-in-differences analysis are headquartered in bordering counties of other non-treatment states (i.e. states without a large UI increase). This approach assumes that firms headquartered in bordering counties on the opposite sides of a state line should be subject to similar local economic shocks.

To implement this test, we first identify all state-years with an increase in UI level of 10% or more from the previous year and call these large UI increase events. For each large UI increase event, we define firms headquartered in the state as treatment firms and keep their data over the years [-4, +4] around the event. We then identify firms headquartered in bordering counties to the counties of the treatment firms as

potential control firms and further require that the states of these bordering counties do not have a large UI increase event over event years [-6, +6]. We then estimate a dynamic difference-in-differences model where the base year is chosen to be the year before the large UI increase event (i.e. event year -1) as follows:

$$y_{ijsct} = \sum_{k=-4}^{k=-2} \beta_k \times LargeInc_{s,k} + \sum_{k=0}^{k=+4} \beta_k \times LargeInc_{s,k} + \Gamma X_{i,t-1} + \Theta Z_{s,t-1} + \mu_{cj} + \tau_t + \varepsilon_{ijsct} \quad (2)$$

where i indexes firm, j indexes industry, s indexes headquarters state, c index headquarter county, and t indexes year. $LargeInc_{s,k}$ equals one if the firm's headquarters state s has a large UI increase event and the observation is k years from the base year and equals zero otherwise. In the regressions, we control for firm characteristics $X_{i,t-1}$ and state economic and political conditions $Z_{s,t-1}$. We also include headquarters county-industry fixed effects μ_{cj} and year fixed effects τ_t.

Estimates of Equation (2) are reported in Table 5. The coefficient estimates β_k's measure the change in acquisition likelihood between the event year and the base year (event year -1) for treatment firms relative to the control firms. In Figure 4, we plot these coefficient estimates together with their 95% confidence intervals. As we can see, the acquisition likelihood for treatment and control firms appears to follow the same time trend prior to the large UI increase events. However, one year after the large UI increase events, the acquisition likelihood of the treatment firms rises significantly relative to that of the control firms and remains higher for the next three years.

5. Target board stakeholder orientation and the UI Effect

So far our results strongly support a causal relation from target state UI levels to acquisition likelihoods. During our sample period, hostile takeovers became rare, thanks to state court acceptance of the poison pill defense beginning in 1982 and the widespread adoption of state antitakeover laws in the 1980s. Hence, almost all successful acquisitions required the consent of the target board of directors. Given these circumstances, the most plausible explanation for the causal relation between UI levels and acquisition likelihoods is that after increases in target state UI benefit levels, target boards approve more acquisitions (presumably these additional acquisitions included deals with larger layoff plans). In the following sections,

we present more revealing evidence that the relation between target state UI levels and acquisition likelihoods is mainly driven by target boards considering employee interests when assessing takeover offers.

5.1. The Adoption of Constituency Statutes

We first exploit the staggered adoption of constituency statutes by U.S. states.[11] These statutes explicitly authorize directors to consider the impact of their decisions on not only shareholders, but also other constituency groups such as employees, suppliers, customers, creditors, and the local community (Bainbridge, 2005). By the end of our sample period, 35 states have adopted a constituency stature. In all 35 states, the statute applies to the decision on whether to accept or reject an acquisition bid. In 28 of these states, this authorization extends to all board decisions. Prior studies find that the adoption of a constituency statute significantly raises firms' stakeholder orientation (Flammer and Kacperczyk, 2016; Cremers, Guernsey and Sepe, 2019). Hence, we expect the adoption of these statutes to encourage more firms incorporated in these states to consider the impact of acquisitions on employee unemployment risk, which should lead to an increase in the average UI effect.

A significant advantage of this setting is that the adoption of constituency statutes is exogenous to the affected firms.[12] Hence, the treatment effect should mainly be driven by an increase in target boards' stakeholder orientation. To assess the treatment effect of the constituency statute on the UI effect, we estimate the following difference-in-differences regression:

$$y_{ihst} = \alpha UI_{h,t-1} + \beta CS_{s,t-1} + \gamma UI_{h,t-1} \times CS_{s,t-1} + \delta X_{i,t-1} + \mu_{hj} + \mu_{jt} + \mu_s + \varepsilon_{ihst} \quad (2)$$

where i indexes firm, j industry, h headquarters state, s incorporation state, and t year. The binary dependent variable y_{ihst} equals 1 if firm i is an acquisition target in year t. $CS_{s,t-1}$ equals 1 if the firm's incorporation state s passed a constituency statute by year $t-1$ and is 0 otherwise. $UI_{h,t-1}$ equals the UI level in headquarters state h in year $t-1$. We demean $UI_{h,t-1}$with respect to its sample mean before

[11] These laws typically amend the existing statutory statement about a director's duty of care and thus they are also known as directors' duties laws

[12] Karpoff and Wittry (2018) identify 8 firms in 5 states that lobbied for the passage of the constituency statute in their state. To avoid endogeneity, we exclude these firms from our estimation.

including it in the equation, so the coefficient β measures the effect of passing a constituency statute on a firm's acquisition likelihood in a state with the sample average UI level. The coefficient of interest is γ, which measures the change in the UI effect following the adoption of a constituency statute. $X_{i,t-1}$ is defined as the same vector of state-level controls discussed in Section 3.1. The regression model includes headquarters state-industry, industry-year, and incorporation state fixed effects. We double cluster standard errors by headquarters and incorporation states.

Table 6 reports the estimates from three specifications of Equation (2). In the first specification (columns 1 and 2), we only include target state UI levels, an indicator for the adoption of a constituency statute in the firm's incorporation state, and the interactions of the two, along with all the fixed effects in Equation (2). Angrist and Pischke (2009) note that when evaluating the effect of a policy change, controlling for firm characteristics can be undesirable since these variables themselves can be affected by the policy. In the second specification (columns 3 and 4), we add controls for the adoption of other second-generation state antitakeover laws (Karpoff and Wittry, 2018). In the last specification (columns 5 and 6), we add state-level controls and firm characteristics, which are the same as in Table 3. We find that the coefficient on $UI_{h,t-1} \times CS_{s,t-1}$ is positive and statistically significant in all six models and the magnitude of the coefficient is larger in columns where we control for the status of other second-generation antitakeover laws. The point estimates in column 5 indicate that the adoption of constituency statutes increased the UI effect by 41% (0.0029/0.0071 = 0.41).

5.2. Labor Intensity

Employees in labor-intensive firms are in general exposed to a greater risk of post-merger layoffs than employees in capital-intensive firms since there is greater potential for cost-cutting layoffs in firms that are labor intensive. Except for high-tech firms, employees in labor-intensive firms tend to have low skills and wages. Thus, changes in UI benefits are likely to have a bigger impact on the lives of unemployed workers in labor-intensive firms. Hence, we expect boards of labor-intensive firms to be more likely to consider employee unemployment costs when assessing a takeover offer than boards in capital-intensive firms.

Following John, Knyazeva and Knyazeva (2015), we measure labor intensity at the 2-digit SIC industry level. It is calculated as the ratio of compensation expenses to output based on BEA industry Accounts data, where we convert the data based on North America Industry Classification System (NAICS) to that based on 2-digit SIC industry definitions. A firm is defined to have high labor intensity if the value of this ratio is above the sample median. We then test the differential impacts of target UI levels on the acquisition likelihoods of high versus low labor intensity firms by estimating Equation (1) with the addition of two explanatory variables: a high labor intensity indicator and its interaction with the UI level. Columns1 and 2 of Table 7 present the results. We find that the coefficient on *UI level* × *High Labor Intensity* is positive and statistically significant, suggesting that UI changes have a larger effect on the acquisition likelihood of firms with high labor intensity. The coefficient estimate in column 1 indicates that the UI effect is 42% (0.0009/0.0031 = 0.29) larger for firms with high labor intensity than for firms with low labor intensity. The coefficient of *High Labor Intensity* indicator is subsumed by the industry-year fixed effects so it does not appear in the table.

5.3. Ownership by short-term institutions

Institutional investors constitute the largest investor group in the U.S. equity market and are usually considered the pivotal investor group in takeovers (Gaspar, Massa and Matos, 2005). Prior studies find that institutional investors have different horizons, and ownership by institutions with different horizons can have different implications for major firm decisions (Bushee, 1998; Derrien, Kecskés and Thesmar, 2013). We conjecture that the target UI level-acquisition likelihood relation is weaker in target firms with high short-term institutional ownership. First, short-term institutional investors are more willing to tender their shares to hostile bidders (Gaspar, Massa and Matos, 2005). Thus, high ownership by short-term institutions can weaken a board's ability to resist a takeover bid. Second, short-term institutional investors are more likely to put pressure on a board to break existing implicit contracts with employees that are preventing the board from accepting an acquisition offer.

Following the prior literature (e.g., Gaspar, Massa and Matos, 2005; Yan and Zhang, 2009), we classify institutional investors into short-term and long-term based on their average portfolio turnover. We obtain

institutional investor portfolio information from 13F filings. The data contains the positions of all the institutions with more than $100 million under management. We calculate for each institutional investor a measure of their portfolio turnover as follows. First, we calculate the net purchase and sale (churn rate) by institutional investors k in quarter t using the following equations:

$$CR\_buy_{k,t} = \sum_{i=1}^{N_k} |S_{k,i,t}P_{i,t} - S_{k,i,t-1}P_{i,t-1} - S_{k,i,t-1}\Delta P_{i,t}|, \quad if\ S_{k,i,t} > S_{k,i,t-1}$$

$$CR\_sell_{k,t} = \sum_{i=1}^{N_k} |S_{k,i,t}P_{i,t} - S_{k,i,t-1}P_{i,t-1} - S_{k,i,t-1}\Delta P_{i,t}|, \quad if\ S_{k,i,t} \leq S_{k,i,t-1}$$

where $P_{i,t}$ and $P_{i,t-1}$ represent the share prices of stock i at the end of quarter t and $t-1$. $S_{k,i,t}$, $S_{k,i,t-1}$ are the number of shares of stock i held by investor k at time t and $t-1$, respectively. Then, we calculate the churn rate of institution k in quarter t as:

$$CR_{k,t} = \frac{\min(CR\_buy_{k,t}, CR\_sell_{k,t})}{\frac{\sum_{i=1}^{N_k} S_{k,i,t}P_{i,t} + S_{k,i,t-1}P_{i,t-1}}{2}}$$

Third, we calculate institution k's average churn rate over the past four quarters. We then sort all institutional investors into three terciles based on their average churn rates. Investors that are ranked in the top tercile are classified as short-term institutional investors.

Using the classifications of short-term institutions as explained above, we calculate the percentage of shares held by short-term institutional investors in each firm year. If the ownership by short-term institutions is above the sample median of all firms for the year, then we define the firm as having high short-term institutional ownership. In Table 7, columns 3 - 4, we report the estimates from Equation (1) augmented by an indicator for high short-term institutional ownership and its interaction with the target firm's UI level. Consistent with our expectation, we find that the relation between target UI level and acquisition likelihood is 22% (0.0010/0.0046 = 0.22) weaker for firms with high short-term institutional ownership. Consistent with Gaspar, Massa and Matos (2005), we find that firms with high short-term institutional ownership are more likely to be acquired, as indicated by the positive coefficient on the high short-term institutional ownership indicator.

5.4. Population of Firm Headquarters County

Managers and directors are likely to feel a stronger sense of responsibility to employees when their firm is headquartered in a less populated county. There are several possible reasons for this. First, there are tighter social connections between the management and employees in less populated counties (Landier, Nair and Wulf, 2009). Second, smaller communities can foster a cooperative culture where the management and employees mutually support each other. Consistent with this perspective, a large number of studies in psychology find that helping is more likely to occur in a nonurban than in an urban context (Steblay, 1987). Lastly, layoffs in one firm tend to have a larger negative impact on the local economy when the firm is headquartered in a less populated county than when it is in a large metropolitan area. Hence, the board is more likely to consider the negative impact of an acquisition on the local community when the firm is headquartered in a less populated county.

The UI level can affect the decisions of boards in less populated counties to accept or reject an acquisition offer because higher UI benefits not only reduce the direct unemployment cost of employees but also the adverse economic impact of post-merger layoffs on the local community. Di Maggio and Kermani (2015) estimate that a one standard deviation increase in UI benefits reduces the effect of adverse local labor demand shocks on the employment growth in a county by 7% and the earnings growth in a county by 6%. The reduced negative demand shock brings two benefits to the unemployed workers. First, their spouses or other family members working in local firms are more likely to keep their jobs. Second, they are more likely to find a job in other firms in the local area.

To test this conjecture, we obtain data on the annual county population from the U.S. Census and define a county as having a low population if it is ranked in the smallest quartile of the distribution of county populations in our sample for the year.[13] In Table 7, columns 5 - 6, we report the estimates from Equation (1) augmented by an indicator for firms headquartered in less populated counties and its interaction with these firms' state UI levels. We find that the target state UI level - acquisition likelihood relation is more

[13] The results are robust to using median population as cut-off.

pronounced for firms headquartered in less populated counties. The coefficient on the less populated county indicator is negative and statistically significant, suggesting that firms headquartered in less populated counties have a lower likelihood of being acquired to begin with, which is consistent with our conjecture that the boards in these firms are more concerned about the impacts of layoffs.

5.5. Social Capital in Firm Headquarters County

The likelihood that a board will consider employee interests can also be positively correlated with the level of social capital in a firm's headquarters region. Following Guiso, Sapienza and Zingales (2011), we define social capital as the norms and networks that encourage cooperation. Jha and Cox (2015) show that social capital is a useful social construct to capture altruistic inclinations. Consistent with this, they find that the level of social capital in a firm's headquarters county is positively correlated with the firm's corporate social responsibility (CSR) ratings. Hence, we use the level of social capital as a proxy for the prosocial norm in the firm's headquarters county.

The prosocial norm in the firm's headquarters county is likely to shape the board's concern for employees through three channels. First, if there are local directors on the board, their social values are likely to be congruent with that of the general population in the firm's location. For directors who do not live locally, they are likely to share similar values as people living in the firm's headquarters region due to matching. That is to say, boards are more likely to appoint directors that share their values and culture while directors are more likely to join boards that share their values. Second, local norms shape the societal expectations in the region about the firm's business behavior and outcomes. Violating these norms can lead to external sanctions (e.g., shame) leveled on a firm's directors, and especially local directors. Third, social capital is also found to limit opportunistic behaviors in transactions by individuals and firms (Coleman, 1988; Knack and Keefer, 1997). For example, Hasan *et al.* (2017) find that higher social capital in a firm's headquarters county reduces a firm's opportunistic behavior in debt contracting. In our context, this means that higher social capital can foster the use of implicit contracts that increase firm value ex ante, but ex post obligate the board to consider employee interests when assessing acquisition offers. Since employees in

high social capital counties can place greater trust in a board not to renege on their promises ex post, employees are more likely to enter implicit contracts ex ante (Shleifer and Summers, 1988).

Following Jha and Cox (2015) and Hasan *et al.* (2017), we use the social capital index of U.S. counties from Rupasingha, Goetz and Freshwater (2006 with updates) as our measure of social capital. This index is constructed from two measures of cooperative norms and two measures of the density of local social networks. The two measures of norms are the census mail response rate and the votes cast in the most recent presidential election by eligible voters in the county. Guiso, Sapienza and Zingales (2011) argue that because there are no legal or economic incentives for people to vote or to mail back census surveys, these two measures are more likely to capture social norms that emphasize cooperative behaviors. The two measures of networks are the number of social organizations and the number of nonprofit organizations per 10,000 people. Dense social networks promote cooperation and reinforce the attendant norms of the network. Rupasingha, Goetz and Freshwater (2006 with updates) conduct a principal component analysis on these four measures and extract the first component to use as their social index. The value of this index is available for the years 1990, 1997, 2005, 2009, and 2014. We fill in the missing social capital levels in the intervening years using the index level for the preceding year when the index is available, following Hasan *et al.* (2017). For example, we fill in the missing social capital index for the years between 1991 and 1996 using the index in 1990 and so on. We then define a high social capital indicator which equals one if the social capital of the county in the year is in the top quartile of our entire sample and zero otherwise.

In Table 7, columns 7 - 8, we report the estimates of Equation (1) augmented by an indicator for high headquarters county social capital and its interaction with the target firm's UI level. We find that the relation between target UI level and acquisition likelihood is more pronounced for firms headquartered in high local social capital counties than in low local social capital counties. High local social capital itself is associated with a lower acquisition likelihood. Both are consistent with the boards in high local social capital counties being more protective of their employees.

5.6. Boards with Female Directors

Many studies find that female leaders are more concerned about employee welfare, including unemployment risk than male leaders are. Adams, Licht and Sagiv (2011) and Adams and Funk (2012) suggest that female directors are more likely to side with non-equity stakeholders when conflicts of interest between shareholders and other stakeholders exist. Rubinstein (2006)'s survey of readers of a leading business daily newspaper in Israel shows that, when presented with the option of maximizing profits by laying off half the workforce versus making do with lower profits by laying off fewer workers, women disproportionally made the latter choice relative to men. Matsa and Miller (2013) find that within two years after the passage of the 2006 Norwegian law requiring a minimum of 40% female representation on corporate boards, Norwegian firms undertook significantly fewer workforce reductions than matched samples of private Norwegian firms or private and public firms in other Nordic countries. Finally, Matsa and Miller (2014) find that female-owned firms are less likely to downsize their workforce in the 2007-2009 Global Financial Crisis than male-owned firms.

We expect that boards with female independent directors (IDs) are more likely to consider employee unemployment costs when assessing an acquisition offer. To test this proposition, we estimate our baseline model specified in Equation (1) within different subsamples of firms based on the number of female IDs on the board. The sample used in this table begins in 1996, which is the first year that director data is available in the ISS database. In all these regressions, we also control for board independence and board size, two variables that are found to influence many board decisions in prior studies.

The results are reported in Table 8. Columns 1 - 2 report the estimates of Equation (1) for the subsample of firms with no female IDs, columns 3 - 4 report the estimates for the subsample of firms with at least one female ID, and columns 5 - 6 report the estimates for the subsample of firms with at least two female IDs. About one-third of the firm-year observations do not have any female IDs and one-third have more than one female ID. We find that the UI coefficient is positive in all columns. However, it is statistically insignificant in the subsample of firms without female IDs. In contrast, the UI coefficient is both larger and statistically significant in the subsample of firms with at least one female ID. As we increase the minimum number of female IDs to two, the UI coefficient increases further in size and remains statistically significant.

These results are consistent with firms with female IDs being more aft to consider employee interests when evaluating an acquisition offer.

5.7. Labor Union Opposition Channel

An alternative explanation for our result is that the UI effect is driven by variation in labor union opposition in target firms. Prior studies suggest two ways through which labor unions can affect acquisition outcomes. First, unions can oppose an acquisition through lobbying and demonstrations (Pagano and Volpin, 2005). Second, unions can make it more difficult for acquirers to extract synergies from labor restructuring ex post in part due to union contractual rights, which reduces the attractiveness of the company as an ex ante acquisition target (Tian and Wang, 2021). An increase in UI level may reduce the incentive of labor unions to oppose takeovers and post-merger restructuring because of either the lower unemployment cost of employees or a lower willingness of employees to collude with managers to protect the firm from hostile takeovers (Pagano and Volpin, 2005) and thus, raise the likelihood of unionized firms being acquired.[14]

To test whether employee opposition to acquisition offers and post-merger restructuring through labor union actions plays a role in the UI level-acquisition likelihood relation, we classify firms into high and low union coverage groups based on whether the union coverage rate in the firm's primary 2-digit SIC industry is above the sample median or not (Hirsch and Macpherson, 2003; Chen, Kacperczyk and Ortiz-Molina, 2011; John, Knyazeva and Knyazeva, 2015). We use industry-level union coverage rates since firm-level unionization data is difficult to find and using industry-level union coverage rates has the advantage that they are less likely to be co-determined with firm-specific factors that correlate with acquisition likelihoods. We then add the indicator for high union coverage industries and its interaction with UI levels to Equation (1). The results are reported in Table 10.

[14] Another way in which employees can protect managers from hostile takeovers is to refuse to sell their shares to the raider when they own a significant block of shares (Pagano and Volpin, 2005). This channel, however, is unlikely to drive our result for two reasons. First, the takeover premium that employees will receive on their ESOP holdings when an acquisition goes through can easily exceed the increase in UI benefits. Second, only a very small percentage of firms have ESOP plans in place among U.S. public firms, so ESOPs are very unlikely to drive our results. Kim and Ouimet (2014) find only 4,597 firm-year observations with ESOP plans between 1981-2001 among U.S. public firms.

In columns 1 - 2 of Table 10, we include state-industry fixed effects and year fixed effects. We find that being in a high union coverage industry reduces a firm's acquisition likelihood, which is consistent with Tian and Wang (2021). Importantly, the coefficient on the interaction term of *Target UI Level*× *High Union Coverage* is positive and statistically significant, which is consistent with labor unions varying their opposition to acquisitions based on the state's UI level. In columns 3 - 4, we replace the year fixed effects with industry-year fixed effects to address the concern that the result may be driven by unobserved industry shocks. We find that the coefficient on the interaction term of *Target UI Level*× *High Union Coverage* remains positive and statistically significant.

The above evidence is consistent with labor union opposition driving some of the UI effects we observe. Nevertheless, the main effect of the target state UI level remains positive and statistically significant in all models and its magnitude is only slightly smaller than that in our baseline regression reported in Table 3. Hence, labor union opposition has only a very small impact on our baseline estimates of UI effects. More importantly, our hypothesis that employee interests affect the target boards' decisions to accept acquisition offers in a sizable proportion of firms continues to hold. Given the recent significant decline in U.S. union membership, the UI effect is likely to be mainly driven by target boards' consideration of employee unemployment costs. Moreover, this labor union opposition channel cannot explain our finding that state adoptions of constituency statutes strengthen the relation between target UI level and acquisition likelihood.

6. UI Level and Post-Merger Workforce Restructuring

According to our hypothesis, the rise in acquisition likelihood following an increase in target state UI level should be mainly driven by acquisitions involving larger post-merger layoffs plans. This suggests that we should observe a positive relation between target state UI levels and the scale of post-merger workforce restructuring. In this section, we test this prediction.

Ideally, workforce restructuring should be measured by employee turnover levels. The Compustat database, however, only reports total employment levels for listed firms at the end of each fiscal year. Thus, we have to measure workforce restructuring by the change in acquirer plus target employee headcounts

from before to after deal completion, although this measure has some obvious limitations.[15] With these caveats in mind, we proceed to examine the effect of a target state's UI level on the change in the combined firm's employee headcount around acquisitions.

Following Dessaint, Golubov and Volpin (2017), we track the combined number of acquirer and target employees over a five-year window around the deal completion year, which is coded as event year 0. A post-merger indicator is set equal to 1 for event years +1 through +3 and 0 for event years -1 and 0. To make sure that the change in employee headcount around acquisition is not confounded by other acquisitions made by the acquirer within the 5-year event window, we exclude deals in which the same acquirer announced another merger or acquisition within three years before or after this acquisition announcement. Using the remaining deal-year panel, we estimate OLS regressions where the dependent variable is the natural logarithm of the combined number of acquirer and target employees, and the key explanatory variable is an interaction between the demeaned target state's UI level in event year -1 and the post-acquisition indicator. We demean the target state's UI level by the sample mean so that the coefficient of the post-acquisition indicator measures the change in the combined number of acquirer and target employees from before to after deal completion for acquisitions in the state, conditional on the average UI level in our firm-year sample. In all these models, we control for deal fixed effects so that employee headcount is compared within a given deal. We also control for industry-year fixed effects so that the post-acquisition indicator represents the abnormal percentage change in the number of employees following the acquisition relative to that occurring in other firms in the same industry that are not acquired that year.

Table 11 reports estimates for different samples. Columns 1 - 2 report the results for the sample of all qualified deals. We find that the coefficient of the post-acquisition indicator is negative and statistically

[15] Several limitations of this measure should be recognized. First, employee headcounts reflect the net effect of layoffs and new hires. If a firm simultaneously discharges and hires workers, the change in employee headcount would understate the actual scale of restructuring. Second, although we expect most layoffs to occur in target firms, our restructuring measure also includes head counts for the acquirer. Since target firms are generally much smaller than acquirers, the noise in the combined employee figures can be larger relative to target layoffs as the relative deal size declines, making it more difficult to detect statistical significance. Third, Compustat employee headcounts include part-time and seasonal workers, which adds further noise to our restructuring measure, so that if permanent employees are replaced by part-time ones, the total number of employees would show no change although layoffs occurred.

significant, suggesting that on average acquisitions in our sample are followed by a significant workforce reduction. In support of our hypothesis, the coefficient of *Target UI Level*× *Post-Acquire* is negative and statistically significant, indicating a greater reduction in employee headcounts when target UI benefits are more generous. Columns 3 - 6 report the same analysis for within-industry and diversifying deal subsamples. We find that the relation between target UI levels and workforce reductions is mainly driven by within-industry deals, where there is greater potential for synergies from workforce restructuring and reductions.

Columns 7 – 10 report the same analysis for target firms in high and low union coverage industries separately. We find that the relation between target UI levels and workforce reductions is statistically significant in both the high and low union coverage subsamples. Since the relation does not just exist in unionized firms, this evidence is consistent with our earlier conclusion that a target board's consideration of employee interests plays an indispensable role in driving the UI level–acquisition likelihood relation.

7. A Discussion of the Interaction of Severance Pay and UI Benefits

Although federal and state laws in the U.S. do not require firms to provide severance, many firms do so as a gesture of goodwill and in exchange for the release of the company from future lawsuits and negative publicity. Severance pay usually amounts to a week or two of pay for each year of employee service to the company. The severance pay may be much higher for senior executives. There are several situations in which employers are required to provide severance pay. First, if it is contained in the employee's employment contract. Second, if it is mentioned in the employee handbook. Third, if it has been provided in past acquisitions. Hence, if the acquirer has a policy of providing severance pay to its employees, target employees can expect to get severance pay in addition to state UI benefits when they are laid off in the post-merger integration process. If the acquirer does not have a policy of providing severance pay, then including a severance pay provision in the acquisition agreement can potentially obligate the acquirer to pay severance to employees in future terminations. In this latter case, the cost of including a severance pay provision in the acquisition agreement can be much higher than the direct amount that it plans to pay target employees who lose their jobs during post-acquisition workforce restructurings. Whatever the case, as long as the

acquirer does not adjust its severance policy according to the UI changes in the target firm's state, UI changes should continue to have an exogenous incremental effect on acquisition likelihood.

If the acquirers adjust their severance policies according to the changes in target UI levels, then, in theory, we should not be able to find any UI effect. However, in practice, the likelihood of acquirers doing so is very low and sometimes it is legally proscribed. For example, most UI changes in our sample are UI level increases. This means that acquirers would need to cut their severance pay to offset the effect of UI level increases. For acquirers with an existing severance policy, lowering the severance pay is likely to trigger strong opposition from existing employees. For acquirers without an existing severance policy, there is nothing to be lowered. Overall, although we do not explicitly consider severance pay in our study, it is unlikely to affect the inferences of our study regardless of whether or not it is paid.

8. Conclusions

We empirically examine whether employee interests affect the evaluation of acquisition offers by target boards of directors by exploiting an ideal econometric setting provided by state UI benefit changes. As this study and prior studies show, state UI benefit changes affect employee unemployment costs, but they are not associated with short-run changes in economic fundamentals in the state. This allows us to isolate the effect of changes in employee unemployment costs on acquisition likelihoods from other macroeconomic changes that affect acquisition likelihoods. Consistent with target boards considering employee unemployment costs when assessing acquisition offers, we find that increases in target state UI benefits significantly raise the acquisition likelihood of firms headquartered in the state. This relation is robust to controlling for time-invariance heterogeneity at the target state-industry level, industry shocks, local economic shocks, and reverse causality.

The adoption of constituency statutes by U.S. states, which explicitly authorizes target directors to consider non-shareholder stakeholder interests when deciding on whether to reject an acquisition offer, significantly increases the UI effect on the firm-level acquisition likelihood. We also find that the UI level-acquisition likelihood relation is more pronounced in the subsample of firms in labor intensive industries, headquartered in less populated or high social capital counties, and having female independent directors on

the board, while the relation is less pronounced in the subsample of firms with high ownership by short-term institutional investors. Lastly, consistent with lower unemployment costs reducing target boards' concern for post-acquisition layoffs, we find that a target state's UI level in the year prior to an acquisition is positively related to the scale of post-acquisition layoffs.

Besides board of directors, another party that has incentives to protect target employee interests and can have an impact on acquisition outcomes are labor unions. We find some evidence that labor unions appear less resistant to takeovers when the target state's UI level is higher. However, this labor union effect can only explain a small fraction of our baseline relation between target UI level and acquisition likelihood.

Our finding that some target boards consider employee interests when assessing acquisitions calls for further research into the specific forces that drive a board's incentive to consider employee interests. The implicit contract theory of Shleifer and Summers (1988) and the shareholder welfare maximization theory of Hart and Zingales (2017) provide two potential explanations. The cross-sectional variations in the state UI level-acquisition likelihood relation that we document, can primarily be understood in light of these two theories. Beyond these two theories, there are many other possible reasons why target firm boards of directors would consider employee interests when evaluating acquisition offers. For example, other reasons could include political considerations, the reputation concerns of managers and directors, community pressure, etc. Future research is called for to further explore this important question.

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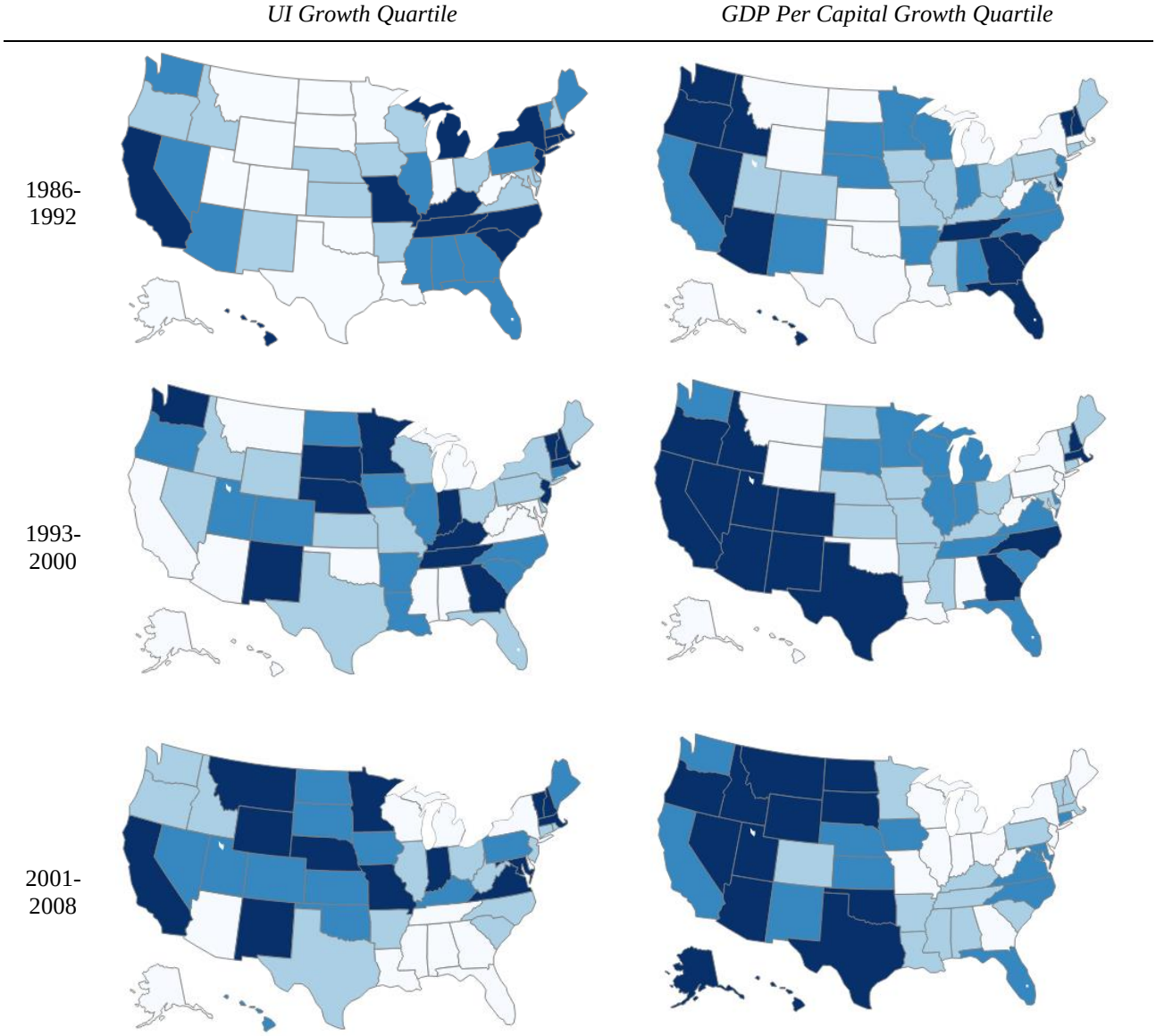
Figure 1
This figure plots quartiles of UI growth and GDP per capita growth over four equally divided subperiods between 1986 and 2018. The UI growth rate over each subperiod is calculated as the state UI level at the end of the subperiod minus that at the beginning of the subperiod divided by the state UI level at the beginning of the period. The GDP per capita growth over each subperiod is calculated similarly. We then classify states within each subperiod into four quartiles based on the UI level growth rate and plot them on the left panel of the figure. Similarly, we classify states within each subperiod into four quartiles based on the GDP per capita growth rate and plot them on the right panel of the figure.



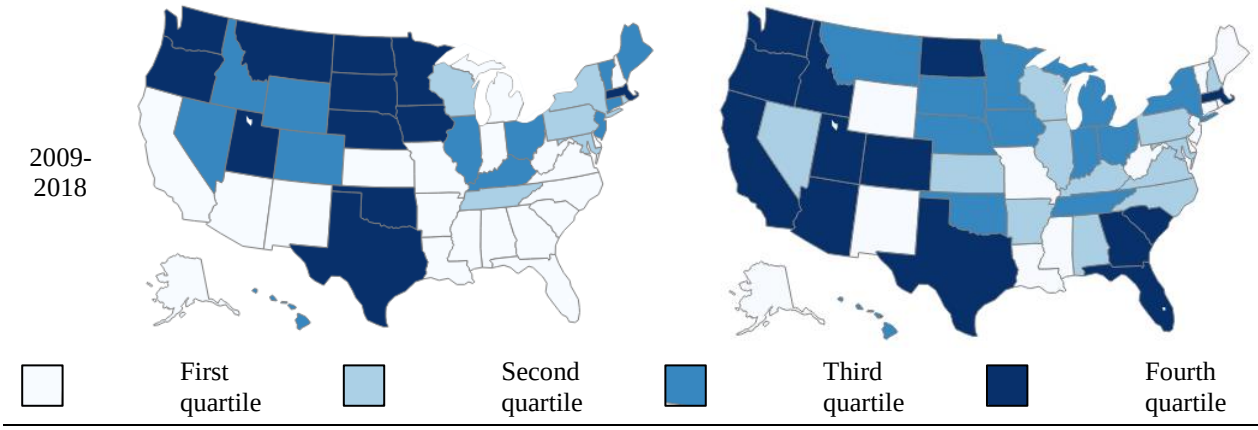


Figure 2 Average Annual Change of State Acquisition Ratio vs. Average Annual Percentage Change of State UI Level (1986-2018)

This figure plots the average annual change of state acquisition ratio against the average annual percentage change of state UI level over the 1986-2018 period for each U.S. state. The state acquisition ratio is defined as the proportion of firms in the Compustat database headquartered in the state that are acquired in a given year. We regress the annual changes in this ratio for all state-years with at least 10 firms on year fixed effects and define the residuals from this regression as the annual changes of state acquisition ratios. The average annual percentage change of state UI level is calculated similarly. We first run a regression of the annual percentage changes of UI level for all state-years with at least 10 firms on year fixed effects. We then average the residuals for each state from 1986 to 2018 to obtain the average annual percentage change of state UI level for each state. Each circle in the plot represents one state. The dashed line represents the best linear fit through the observations.

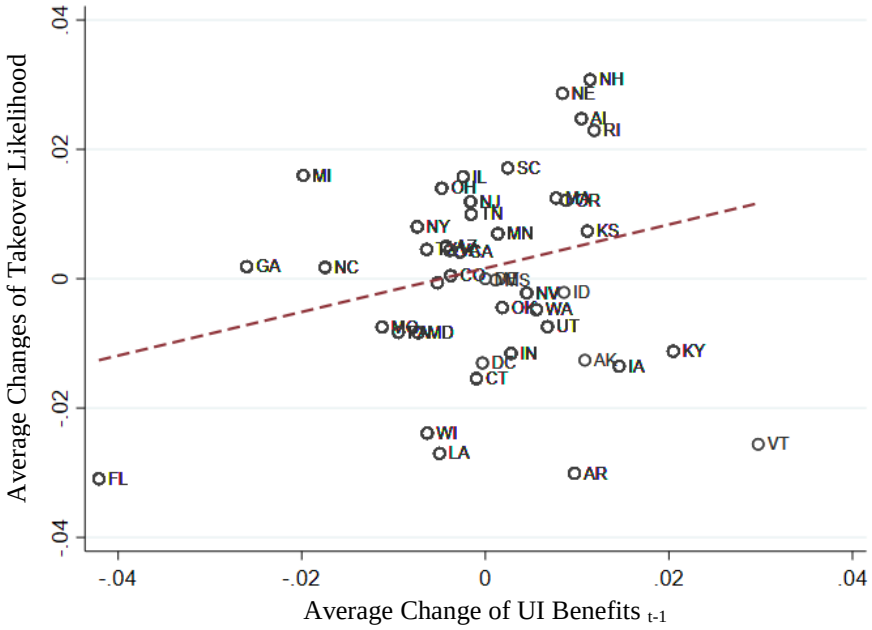


Figure 3: Timing of the Treatment Effects Around Large UI Increases

This figure plots the point estimates and their 95% confidence intervals of the effects of a large state UI increase on firm-level acquisition likelihood by year over the [-4,+4] year event window around the large UI increase event in year 0 estimated from Equation (2). A large UI increase event is defined to be an annual percentage change of state UI level of 10% or more. The point estimate in event year t represents the change in firm-level acquisition likelihood from the base year (event year -1) to that event year t for treatment firms relative to that for control firms.

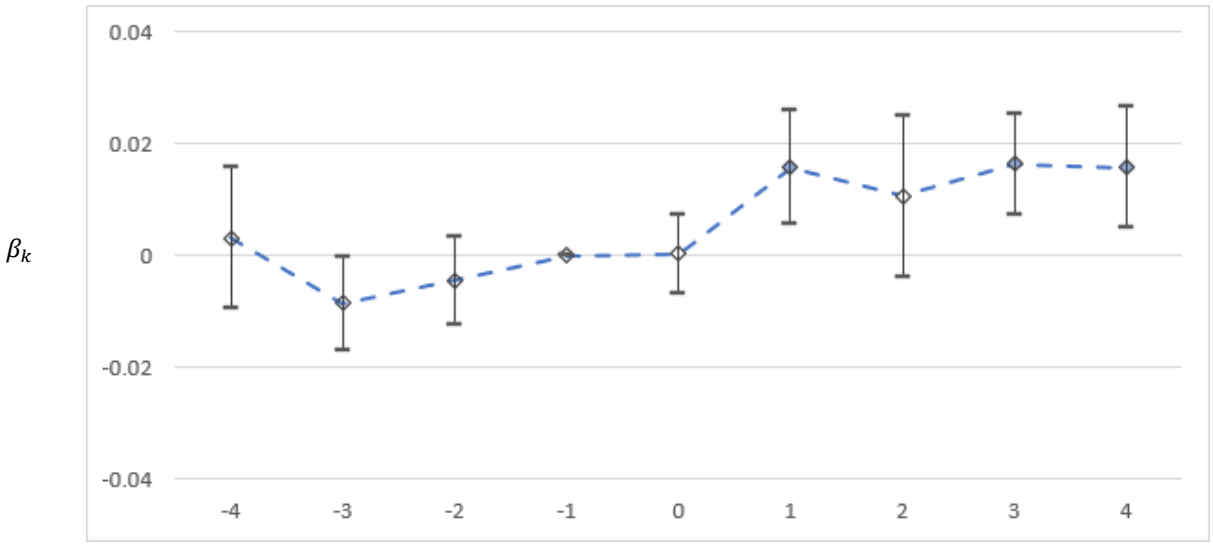


Table 1: State UI level and State Economic Conditions and Gubernatorial Election

This table summarizes the results from state-panel regressions of state UI level, measured in thousands of dollars, on controls for state economics conditions, state gubernatorial election year indicator, and lagged and contemporaneous state acquisition ratios and state and year fixed effects. The state acquisition ratio equals the proportion of firms in Compustat database headquartered in a state that are acquired in a given year. Variable definitions are provided in the Appendix. All continuous variables (except UI levels) are winsorized at the 1st and 99th percentiles. Robust standard error clustered at the state level is reported in the parenthesis. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels using two-tailed tests, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					UI level (‘000)					
Log GDP Per Capita	1.4019									
	(1.4722)									
Average Wage (‘000)		0.1216								
		(0.1123)								
Election Year			-0.0016							
			(0.0328)							
Unemployment Rate				0.1088						
				(0.0768)						
Union Coverage					-0.0033					
					(0.0818)					
Deal Ratio t						0.2185				0.1679
						(0.3825)				(0.4463)
Deal Ratio t-1							-0.0060			-0.0061
							(0.3841)			(0.4019)
Deal Ratio t-2								0.0700		0.0746
								(0.3555)		(0.3781)
Deal Ratio t-3									0.0557	0.0694
									(0.3313)	(0.3768)
Observations	1,683	1,683	1,683	1,683	1,683	1,683	1,683	1,683	1,632	1,632
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.8506	0.8587	0.8496	0.8503	0.8496	0.8496	0.8496	0.8496	0.8512	0.8509

Table 2 Summary Statistics

This table presents summary statistics of firm, corporate governance, state, and deal characteristics. The sample consists of 85,723 firm-year observations from the CRSP-Compustat Merged Database from 1986 to 2018. We require the firms to be listed on NYSE, AMEX, or NASDAQ in the year and exclude financial and utilities firms. Among them, 3,479 firm years are associated with the announcement of a qualified takeover bid in which the firm is the target of an eventually successful acquisition. Corporate governance data are available from 1996 onwards. Variable definitions are provided in the Appendix. All continuous variables except UI levels are winsorized at the 1st and 99th percentiles.

	N	Mean	Std. Dev	Q1	Median	Q3
Panel A: Firm Characteristics						
Acquisition [0, 1]	85,723	0.04	0.20	0	0	0
Firm Age (years)	85,723	14.63	12.04	5	11	22
PPE/Assets	85,723	0.31	0.27	0.10	0.23	0.43
Leverage	85,723	0.18	0.19	0.00	0.13	0.29
Market-to-Book	85,723	2.41	2.33	1.16	1.67	2.66
R&D/Assets	85,723	0.06	0.11	0.00	0.00	0.07
ROA	85,723	0.11	0.20	0.06	0.13	0.20
Sales Growth	85,723	0.18	0.47	-0.01	0.09	0.23
Firm Size ($ million)	85,723	2274.82	6388.63	71.46	284.82	1275.60
Union Coverage	85,555	0.13	0.13	0.04	0.09	0.17
Constituency Statute	84,211	0.26	0.44	0	0	1
Labor Intensity	69,133	0.27	0.12	0.14	0.29	0.32
Short-Term IO	19,982	0.29	0.20	0.29	0.38	0.29
Combined No. of Employees	8,838	42.95	4.00	12.69	49.72	8838
Panel B: Governance Characteristics						
Board Size	22,115	9.05	2.26	7	9	10
Independent Board	22,115	0.80	0.40	1	1	1
Female ID	22,115	0.99	0.97	0	1	2
Panel C: State-Level Variables						
Acquisition Ratio	1,683	0.04	0.07	0.00	0.01	0.05
Election Year Indicator	85,723	0.25	0.43	0	0	0
Log GDP Per Capita	85,723	10.70	0.22	10.55	10.70	10.86
Unemployment Rate	85,723	0.06	0.02	0.05	0.05	0.07
Social Capital	69,954	0.00	-0.82	-0.64	-0.51	69954
Local Population	71,501	1517576	524960	934140	1663167	71501
Panel D: Unemployment Insurance						
Log (Target UI level)	85,723	9.08	0.41	8.76	9.06	9.37
Target UI level (‘000$)	85,723	9.63	4.60	6.37	8.61	11.70

Table 3 Summary of Unemployment Insurance Levels by state

This table reports the summary statistics of UI levels by state. The UI level is calculated as the product of the maximum weekly UI benefit amount and the maximum duration of the benefits in weeks under each UI schedule for the regular UI program. This value is averaged over the two published schedules each year to get the UI level for each state-year. Column 2 reports the distribution of our sample observations by firm headquarters state. Columns 3 to 5 report the number of deals, mean, and standard deviation of UI levels by state, measured in thousands of dollars. The last column reports the number of large annual UI changes (>10%) in either the plus or minus direction during our sample period from 1986 to 2018.

State	No. of Obs	No. of Deals	Mean ('000$)	SD ('000$)	Changes (>10%)
(1)	(2)	(3)	(4)	(5)	(6)
AK	24	1	9.52	1.72	3
AL	396	13	5.13	1.43	2
AR	426	13	8.34	2.08	2
AZ	1348	61	5.83	1.85	0
CA	13932	747	8.73	3.01	5
CO	2551	94	9.89	3.16	1
CT	1996	75	12.02	3.73	0
DC	144	4	8.80	1.11	4
DE	235	10	7.96	1.43	1
FL	3606	143	6.46	1.94	5
GA	2569	96	6.37	1.61	0
HI	127	3	11.12	3.15	2
IA	426	16	9.03	2.82	0
ID	157	10	7.91	2.14	1
IL	4089	157	10.50	3.30	1
IN	897	30	7.85	2.74	2
KS	430	17	8.35	2.24	0
KY	489	16	8.32	2.39	2
LA	417	16	5.91	0.99	2
MA	5078	221	20.78	7.93	2
MD	1420	72	8.02	2.25	3
ME	131	6	9.72	3.49	0
MI	1775	64	7.81	1.31	3
MN	2672	93	10.93	3.69	2
MO	1363	44	5.88	1.58	6
MS	146	9	4.87	0.80	0
MT	63	4	8.33	3.31	0
NC	1830	56	8.43	2.79	4
ND	41	2	7.69	3.64	1
NE	367	5	6.78	2.39	3
NH	384	21	7.88	3.56	2
NJ	3422	159	11.39	3.68	1
NM	66	8	5.71	2.03	4
NV	732	24	8.23	2.24	0
NY	6330	228	8.95	2.11	4

OH	3118	89	10.44	2.96	0
OK	759	32	8.42	2.62	3
OR	869	33	10.17	2.72	0
PA	3492	112	11.14	3.10	0
RI	288	6	13.27	4.13	2
SC	355	13	6.26	1.39	2
SD	89	2	6.18	1.92	0
TN	1292	43	6.39	1.48	2
TX	9090	346	8.56	2.42	0
UT	758	28	9.20	2.84	1
VA	2189	95	7.72	2.40	3
VT	58	4	6.86	2.06	2
WA	1679	86	12.67	3.43	4
WI	1547	48	7.74	1.62	1
WV	61	2	8.78	1.87	0
WY	24	0	9.52	1.72	3
Total	85,723	3,477	9.63	4.60	88

Table 4 Main Result: Acquisition Likelihoods and Target State UI Levels

This table reports the results from our main linear probability model (Equation 1) estimated using all firm-year observations in our sample. The dependent variable is an indicator that equals 1 if the firm is the target of an eventually successful takeover bid announced in the year, and 0 otherwise. The key explanatory variable is the UI level ('000) in the firm's headquarters state in the year prior to the takeover announcement. All independent variables are lagged by one year with respect to the dependent variable. Variable definitions are provided in the Appendix. State-level controls include *log GDP per capita*, *unemployment rate*, and *election year indicator*. For brevity, their coefficients are not reported. All continuous variables (except UI levels) are winsorized at the 1st and 99th percentiles. Industries are defined by two-digit SIC codes. Robust standard error clustered at the state level is reported in the parenthesis. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels using two-tailed tests, respectively.

	(1)	(2)	(3)	(4)
		Acquisition [0, 1]		
Target UI level $_{t-1}$	0.0033**	0.0033**	0.0040***	0.0043**
	(0.0013)	(0.0013)	(0.0015)	(0.0016)
ROA	-0.0046	-0.0046	-0.0048	-0.0005
	(0.0090)	(0.0090)	(0.0090)	(0.0095)
R&D/Assets	0.0607***	0.0607***	0.0617***	0.0638***
	(0.0108)	(0.0108)	(0.0109)	(0.0121)
Leverage	0.0067	0.0067	0.0068	0.0078
	(0.0062)	(0.0062)	(0.0062)	(0.0065)
Sales Growth	0.0010	0.0010	0.0011	0.0006
	(0.0020)	(0.0020)	(0.0019)	(0.0019)
PPE/Assets	-0.0115***	-0.0115***	-0.0117***	-0.0119**
	(0.0042)	(0.0042)	(0.0042)	(0.0045)
Market-to-Book	-0.0035***	-0.0035***	-0.0035***	-0.0036***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Firm Size	-0.0002	-0.0002	-0.0003	-0.0003
	(0.0006)	(0.0006)	(0.0006)	(0.0007)
Firm Age	-0.0040**	-0.0040**	-0.0039*	-0.0046*
	(0.0019)	(0.0019)	(0.0020)	(0.0023)
State Level Controls	No	No	Yes	Yes
State-Industry FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes
Exclude Dispersed Industries	No	Yes	No	Yes
N	85,723	85,723	85,723	73,147
Adj. R-squared	0.0208	0.0208	0.0221	0.0207

Table 5: Timing of the Effect of a Large UI Increases on Acquisition Likelihood

This table presents coefficient estimates from the dynamic difference-in-differences regression in Equation (2). Treatment firms consist of firms headquartered in states with a large UI increase. A large UI increase is defined as an annual increase in UI level from the previous year of at least 10%. For each large UI increase event, we keep the observations in the [-4,+4] year window around the large UI increase where the year of the large UI increase is event year 0. Control firms consist of firms headquartered in bordering counties to the treatment firms in states that do not have a large UI increase. The key independent variables are indicators *Large Inc (-k)* which equals 1 for a treatment firm observation in event year *k* and zero otherwise. In both models, we control for firm and state characteristics. Firm characteristics include *ROA, R&D/Assets, Leverage, Sales Growth, PPE/Assets, Market-to-Book, Firm Size* and *Firm Age*. State-level controls include *log GDP per capita, unemployment rate,* and *election year indicator*. For brevity, they are not reported. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at the headquarters state level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels using two-tailed tests, respectively.

	(1)	(2)
	Acquisition [0, 1]	
Large Inc (-4)	0.0076	-0.0063
	(0.0076)	(0.0114)
Large Inc (-3)	-0.0066	-0.0127*
	(0.0054)	(0.0066)
Large Inc (-2)	-0.0041	-0.0023
	(0.0049)	(0.0055)
Large Inc (0)	-0.0003	-0.0045
	(0.0043)	(0.0046)
Large Inc (+1)	0.0156**	0.0134**
	(0.0061)	(0.0067)
Large Inc (+2)	0.0108	0.0133
	(0.0088)	(0.0087)
Large Inc (+3)	0.0157**	0.0170**
	(0.0061)	(0.0068)
Large Inc (+4)	0.0162**	0.0164*
	(0.0068)	(0.0083)
County-Industry FE	Yes	Yes
Year FE	Yes	Yes
Exclude Dispersed Ind.	No	Yes
N	17,814	14,818
Adj. R-squared	0.1201	0.1135

Table 6: Evidence from the Adoption of Constituency Statutes

This table examines the effect of the state adoption of the constituency statute on the relation between the target UI level and acquisition likelihood at the firm level. The dependent variable equals 1 if the firm is a target of a takeover bid announced in year t, and 0 otherwise. *Target UI Level* is demeaned with respect to the mean for the sample before it interacts with other variables. *Constituency Statute* equals 1 if the incorporation state of the firm adopted the statute before that year, and 0 otherwise. Firm characteristics include *ROA, R&D/Assets, Leverage, Sales Growth, PPE/Assets, Market-to-Book, Firm Size* and *Firm Age*. State-level controls include *log GDP per capita, unemployment rate,* and *election year indicator*. For brevity, coefficient estimates for firm- and state-level control variables are not reported. Industries are defined at the two-digit SIC code level. Variable definitions are provided in the Appendix. All continuous variables (except UI levels) are winsorized at the 1st and 99th percentiles. The standard error is double clustered by headquarters-incorporation state and is reported in the parenthesis. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels using two-tailed tests, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
			Acquisition [0, 1]			
A: Target UI level $_{t-1}$	0.0025*	0.0027*	0.0066***	0.0077***	0.0071***	0.0081***
	(0.0013)	(0.0014)	(0.0020)	(0.0024)	(0.0022)	(0.0026)
Constituency Statute	-0.0019	-0.0034	-0.0005	-0.0020	0.0005	-0.0008
	(0.0038)	(0.0047)	(0.0054)	(0.0062)	(0.0056)	(0.0067)
A × Constituency Statute	0.0006**	0.0006**	0.0028**	0.0034**	0.0029**	0.0035**
	(0.0003)	(0.0003)	(0.0012)	(0.0013)	(0.0013)	(0.0014)
Business Combinations Law			-0.0067	-0.0060	-0.0072	-0.0050
			(0.0055)	(0.0073)	(0.0049)	(0.0071)
A × Business Combinations Law			-0.0039***	-0.0047***	-0.0038***	-0.0045***
			(0.0014)	(0.0017)	(0.0012)	(0.0016)
Control Shares Acquisition Law			0.0075	0.0063	0.0107	0.0095
			(0.0082)	(0.0095)	(0.0094)	(0.0109)
A × Control Share Acquisition Law			0.0013*	0.0014	0.0011	0.0011
			(0.0008)	(0.0008)	(0.0008)	(0.0009)
Fair Price Law			-0.0080	-0.0106	-0.0097	-0.0138*
			(0.0061)	(0.0073)	(0.0069)	(0.0079)
A × Fair Price Law			0.0012*	0.0014**	0.0017**	0.0019***
			(0.0007)	(0.0007)	(0.0007)	(0.0007)
Poison Pill Law			-0.0033	-0.0021	-0.0033	-0.0022
			(0.0054)	(0.0058)	(0.0050)	(0.0057)
A × Poison Pill Law			-0.0041***	-0.0048***	-0.0041***	-0.0049***
			(0.0009)	(0.0011)	(0.0009)	(0.0011)
State-level Control	No	No	No	No	Yes	Yes
Firm-level Control	No	No	No	No	Yes	Yes
State-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Incorporation State FE	Yes	Yes	Yes	Yes	Yes	Yes
Exclude Dispersed Industries	No	Yes	No	Yes	No	Yes
N	84,211	71,940	84,211	71,940	82,347	70,215
Adj. R-squared	0.0193	0.0184	0.0194	0.0186	0.0222	0.0212

Table 7: Cross-Sectional Variation of the Relation between UI Level and Acquisition Likelihood

This table shows that the relation between UI level and acquisition likelihood is more pronounced for firms that we conjecture are more likely to consider employee interests in acquisitions. The sample period is from 1986 to 2018. The cross-section variations we examine are listed at the top of each column. *High Labor Intense* equals 1 if a firm operates in an industry with above-median labor intensity and 0 otherwise. Labor intensity is measured as the ratio of total employment over total output at the industry level over our sample period (John et al, 2015). *High Short-term IO* is an indicator that equals 1 if a firm's short-term institutional ownership is above the sample median for the year and 0 otherwise. *Low Local Population* is an indicator that equals 1 if a firm's headquarters county's population is in the bottom quartile of the U.S. counties in the year and 0 otherwise. *High Local Social Capital* is an indicator that equals 1 if the level of social capital in the firm's headquarters county is in the top quartile of U.S. counties and 0 otherwise. All models control for firm and state characteristics. For brevity, they are not reported. Firm characteristics include *ROA, R&D/Assets, Leverage, Sales Growth, PPE/Assets, Market-to-Book, Firm Size* and *Firm Age*. State-level controls include *log GDP per capita, unemployment rate,* and *election year indicator*. Variable definitions are provided in the Appendix. All continuous variables (except UI levels) are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered by the headquarters states. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels using two-tailed tests, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Acquisition [0, 1]			
	High Labor Intensity		*High Short-Term IO*		*Low Local Population*		*High Local Social Capital*	
Target UI level $_{t-1}$	0.0031*	0.0032*	0.0046**	0.0048**	0.0029*	0.0031**	0.0035**	0.0037**
	(0.0017)	(0.0017)	(0.0022)	(0.0021)	(0.0015)	(0.0015)	(0.0014)	(0.0014)
Firm Characteristics			0.0235***	0.0212***	-0.0336***	-0.0344***	-0.0471***	-0.0546***
			(0.0066)	(0.0062)	(0.0084)	(0.0091)	(0.0088)	(0.0092)
Target UI level $_{t-1}$× Firm Characteristics	0.0009***	0.0008**	-0.0010**	-0.0008**	0.0029***	0.0030***	0.0044***	0.0048***
	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0008)	(0.0008)	(0.0007)	(0.0007)
State-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude Dispersed Industries	No	Yes	No	Yes	No	Yes	No	Yes
N	67,394	62,090	19,982	16,594	71,501	60,935	69,954	59,619
Adj. R-squared	0.0245	0.0233	0.0394	0.0376	0.0223	0.0218	0.0216	0.0212

Table 8: Takeover Likelihood and Female Independent Directors

This table presents subsample tests of the effect of the presence of different numbers of female independent directors (female IDs) on the target's board on the relation between target UI levels and acquisition likelihood. Columns 1 and 2 are estimated using the subsample of firms without female independent directors, column 2 through 6 are estimated using the subsample of firms with one and more than one female IDs respectively. All models control for firm and state characteristics. For brevity, we do not report them. Firm characteristics include *ROA, R&D/Assets, Leverage, Sales Growth, PPE/Assets, Market-to-Book, Firm Size* and *Firm Age*. State-level controls include *log GDP per capita, unemployment rate,* and *election year indicator*. Industries are defined by two-digit SIC codes. Variable definitions are provided in the Appendix. All continuous variables (except UI levels) are winsorized at the 1st and 99th percentiles. Robust standard error clustered at the state level is reported in the parenthesis. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels using two-tailed tests, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
			Acquisition [0, 1]			
	No Female IDs		1 Female ID		2+ Female IDs	
Target UI level $_{t-1}$	0.0021	0.0020	0.0042**	0.0050***	0.0067**	0.0079**
	(0.0025)	(0.0025)	(0.0018)	(0.0017)	(0.0030)	(0.0039)
Independent Board	0.0028	0.0046	0.0115	0.0130	0.0320*	0.0410**
	(0.0052)	(0.0062)	(0.0073)	(0.0088)	(0.0159)	(0.0174)
Board Size	-0.0005	-0.0005	-0.0002	0.0002	0.0001	0.0010
	(0.0020)	(0.0022)	(0.0012)	(0.0014)	(0.0015)	(0.0017)
State-Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Exclude Dispersed Industries	No	Yes	No	Yes	No	Yes
N	7,827	6,743	13,880	11,303	5,728	4,576
Adj. R-squared	0.0079	0.0119	0.0091	0.0091	0.0051	0.0219

Table 9: Labor Union Opposition

This table presents results on how unionization affects the relation between target UI level and acquisition likelihood. The sample period is from 1986 to 2018. *Highly Union Coverage* is an indicator that equals 1 if the union coverage rate in the firm's primary 2-digit SIC industry is above the sample median and 0 otherwise. All models control for firm and state characteristics. For brevity, they are not reported. Firm characteristics include *ROA, R&D/Assets, Leverage, Sales Growth, PPE/Assets, Market-to-Book, Firm Size* and *Firm Age*. State-level controls include *log GDP per capita, unemployment rate,* and *election year indicator*. Variable definitions are provided in the Appendix. All continuous variables (except UI levels) are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at the headquarters state level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels using two-tailed tests, respectively.

	(1)	(2)	(3)	(4)
	Acquisition [0, 1]			
Target UI level $_{t-1}$	0.0030**	0.0033**	0.0029**	0.0032**
	(0.0013)	(0.0015)	(0.0011)	(0.0012)
High Union Coverage	-0.0180***	-0.0205***		
	(0.0055)	(0.0053)		
Target UI level $_{t-1}$× High Union Coverage	0.0018***	0.0019***	0.0031**	0.0031**
	(0.0005)	(0.0006)	(0.0012)	(0.0012)
State-Industry FE	Yes	Yes	Yes	Yes
Industry-Year FE	No	No	Yes	Yes
Year FE	Yes	Yes	No	No
Exclude Dispersed Industries	No	Yes	No	Yes
N	85,550	72,974	85,550	72,974
Adj. R-squared	0.0267	0.0238	0.0224	0.0211

Table 10: Target UI and post-merger workforce restructuring

This table estimates the relation between the change in acquirer and target combined employment from before to after an acquisition and the target UI level in the year of the takeover announcement. We exclude acquisitions where the acquirer announced another acquisition within three years of the announcement of the previous acquisition. All remaining acquisitions are followed over a five-year period from one year before the deal announcement to three years after deal completion. The dependent variable is the natural logarithm of the combined number of employees in the acquirer and target. *Post-Merger* is an indicator that equals 1 for the three years after deal completion and 0 for the two years before deal completion. *Target UI level* is the UI level in the deal announcement year demeaned by its sample mean. Columns 1 and 2 are estimated using all qualified deals in our sample. Columns 3 and 4 include only within-industry deals, while columns 5 and 6 include only diversifying deals. Columns 7 and 8 include only deals where the target firm is in an industry with above the sample median union coverage rate, while columns 9 and 10 include only deals where the target firm is in an industry with below the sample median union coverage rate. All columns include deal fixed effects and industry-year fixed effects. Industries are defined using Fama-French 48 industry definition. Robust standard errors are double clustered at the deal and year level and reported in parenthesis. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels using two-tailed tests, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log (Combined No. of Employees)									
	All Deals		Within-industry Deals		Diversifying Deals		High Union Coverage		Low Union Coverage	
Post-Merger	-0.112***	-0.097***	-0.159***	-0.142***	-0.004	0.000	-0.167***	-0.171***	-0.065***	-0.026***
	(0.013)	(0.014)	(0.016)	(0.017)	(0.020)	(0.021)	(0.009)	(0.010)	(0.007)	(0.008)
Target UI level $_{t-1}$ × Post-Merger	-0.004*	-0.004*	-0.008**	-0.009**	-0.000	0.001	-0.011***	-0.014***	-0.005***	-0.006***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
Deal FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude Dispersed Industries	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	8,838	7,788	6,157	5,378	2,681	2,410	4,254	3,989	4,574	3,789
Adj. R-squared	0.969	0.969	0.969	0.970	0.965	0.964	0.965	0.965	0.978	0.979

Table A.1: Variable Description

Variable Name	Description
Acquisition [0, 1]	An indicator variable that equals 1 when a firm becomes a target of an ultimately completed bid in a given fiscal year and 0 otherwise. In tests of the likelihood of a firm making a bid, this indicator equals 1 when a firm makes a takeover bid in a given year and 0 otherwise Source: SDC
Board Independence	An indicator variable which equals 1 if over 60% of directors are independent and 0 otherwise Source: ISS
Board Size	The number of directors on the board in each annual board meeting Source: ISS
Constituency Statute	Equals to 1 after a firm's incorporated state passed Constituency Statute (Directors Duty Law) , and 0 otherwise
Business Combination Law	Equals to 1 after a firm's incorporated state passed Business Combination Law, and 0 otherwise
Control Shares Acquisition Law	Equals to 1 after a firm's incorporated state passed Control Shares Acquisition Law, and 0 otherwise
Deal Ratio	The number of deals in each state-level divided by the number of public firms in each state Source: SDC and Compustat
Election Year Indicator	An indicator variable that equals 1 for the year of the gubernatorial election, and 0 otherwise Source: NGA
Fair Price Law	Equals to 1 after a firm's incorporated state passed Fair Price Law, and 0 otherwise
Female ID	An indicator variable which equals 1 if a firm has at least one independent female board member, and 0 otherwise Source: ISS
Firm Age	Number of years since being public Source: CRSP
Firm Size	Natural log of market capitalization of the target (acquirer) Source: Compustat
Log GDP Per Capita	Natural log of state annual real GDP divided by state population Source: BEA
High Labor Intense	Equals 1 if a firm operates in an industry with above-median labor intensity and 0 otherwise. Labor intensity is measured as the ratio of total employment over total output at the industry level across our sample period (John et al, 2015) Source: BLS
Highly Union Coverage	An indicator variable that equals 1 if the union coverage rate of a 2-digit SIC industry is above the sample median and 0 otherwise. Union coverage rate is calculated as the percentage of workers covered by unions in a 2-digit SIC industry year Source: Hirsch and Macpherson (2003)
Large Inc	Equals to 1 for the years that experience at least 10% increase of UI benefits in year t, but do not have other large increases during the [t-6, t+6] year window Source: BEA
Leverage	The sum of long- and short-term liabilities divided by total book value of assets Source: Compustat
Log (Combined No. of Employees)	Natural logarithm of the combined number of employees in the acquirer and the target Source: Compustat

Market to Book	Fiscal-year-end market value of equity plus book value of liabilities divided by total assets Source: Compustat
Poison Pill Law	Equals to 1 after a firm's incorporated state passed Poison Pill Law, and 0 otherwise
Post-Merger	Equals to 1 for the years after deal completion and 0 for the years before deal completion Source: SDC
PPE/Assets	Book value of property, plant and equipment divided by the book value of total assets Source: Compustat
R&D Expense	Research & Development expenditure divided by total assets. If the R&D expenditure is missing, the R&D expense is set to 0 Source: Compustat.
ROA	Operating income before depreciation divided by total assets Source: Compustat
Sales Growth	Sales changes in year *t* relative to year t-1 Source: Compustat
Target UI level	The maximum amount of total UI benefits offered by the regular UI program in a target headquarters state-year. It is calculated as the maximum amount of weekly benefits × the maximum duration of the benefits in number of weeks. We use the average of this amount over the two issues in each year Source: BEA
High Short-Term IO	An indicator that equals 1 if a firm's short-term institutional ownership is above the sample median for the year and 0 otherwise. Short-term institutional ownership is calculated following Gaspar, Massa and Matos (2005) Source: 13F filings
Low Local Population	An indicator that equals 1 if a firm's headquarters county population is in the bottom quartile of the U.S. counties in the year and 0 otherwise Source: BEA
High Local Social Capital	An indicator that equals 1 if the level of social capital in the firm's headquarters county is in the top quartile of the U.S. counties and 0 otherwise. Social capital is the first component from a principal component analysis on census mail response rate, the votes cast in the presidential election, the number of social organizations, and the number of nonprofit organizations per 10,000 people Source: Rupasingha, Goetz and Freshwater (2006)
Unemployment Rate	Average monthly State Unemployment Rate for each year Source: BEA

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