

Do Employee Interests Affect Target Board Decisions About Acquisition Offers? Evidence from Changes in Unemployment Insurance

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Keywords: Mergers and Acquisitions, Board of Directors, Stakeholder Orientation, Employee Interests,

JEL Classification: G32, G34, G38, J65

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Abstract

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1. Introduction

Mergers and acquisitions (M&As) intensify conflicts of interest between shareholders and employees in target firms (Dessaint, Golubov and Volpin, 2017). While shareholders usually receive large premiums from 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 their shareholders and exposing their 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 U.S. 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). In this paper, we test if employee interests affect the decision 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 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 directors do so to maximize shareholder welfare. Hart and Zingales (2017) argue that directors should maximize shareholder welfare rather than shareholder value. These two objectives can

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

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 must 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 gives serious weight to the expected cost of unemployment for their workforce when assessing acquisition offers.

Testing this hypothesis is challenging because target board deliberations about acquisition offers are not available in the US or most other economies. However, if we can observe an exogenous shock that changes the cost of unemployment borne by employees, but it does not alter the economic fundamentals affecting takeover activity, then we can test the above hypothesis by observing the change in acquisition likelihood of the treated 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.² Now, imagine that an exogenous shock reduces the average unemployment cost per employee. For target boards that care about the total unemployment cost of employees, they could 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 to a similar level or even reduced. Thus, this shock should lead to greater willingness on the part of these target boards to accept acquisition offers and especially offers involving greater expected restructuring and, consequently, raise the overall acquisition likelihood.

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 cost per laid-off employee (Gruber,

² Or a measure that is highly positively correlated with total unemployment cost of employees calculated this way.

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 their firms. Third, there is large heterogeneity across states in the timing and size of UI benefit adjustments. Lastly, changes in UI benefits must be approved by state legislatures. Due to the uncertain level of legislative support for these changes, the timing of state 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 board 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. Following Agrawal and Matsa (2013) and Hsu, Matsa and Melzer (2018), we calculate a state's UI benefit level (hereafter, UI 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, target firm characteristics, target state economic conditions and gubernatorial elections, and state-industry, industry-year, and region-year fixed effects. 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 headquartered 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 acquisition offers for a sizable proportion of firms.

We carefully show that state UI changes are exogenous to state economic conditions and gubernatorial elections that may otherwise affect takeover activity. We also find no relation between state UI levels and contemporaneous and lagged state takeover ratios in state-panel regressions, which rules out

reverse causality. However, there are two remaining concerns about a causal interpretation of our main result. First, a time-varying omitted variable at the state level could be correlated with both changes in UI level and takeover activity. Second, the exogeneity of UI changes is mainly in their timing. Eventually, UI levels must adjust to inflation, rising wage levels, and rising costs of living. Thus, only if the change in acquisition likelihood occurs shortly after UI changes can we reliably attribute it to UI changes.

To address the above 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 increasing event to that for firms in bordering counties of other states without large increases in UI levels. 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 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's calendar year and remains significantly higher for two of the next three years.

Having established the causal relation from target state UI level changes to the change in acquisition likelihood, we next show that the relation reflects target boards' consideration of employee interests when making decisions on acquisition offers. First, we rule out two alternative mechanisms. The first mechanism is that the UI effect is driven by variation in labor union opposition to acquisitions. Inconsistent with this mechanism, we find that the UI effect is statistically significant for both high and low unionization firms and the difference is not statistically significant. The second mechanism is that acquisition offers are affected by UI benefits and target boards are more likely to accept deals because of these changes in characteristics. For example, acquirers could offer higher takeover premiums for target firms in states with higher UI levels. However, we find that target UI level changes are not associated with significant changes in takeover premiums, method of payment, and several other deal characteristics.

Second, we provide more direct evidence on the relation between target boards' consideration of employee interests and the UI effect by exploiting staggered adoptions of constituency statutes by some U.S. states that raised the stakeholder orientation of boards in affected target firms. These statutes explicitly authorize target firm directors to consider the impact of an acquisition on employees and other non-shareholder stakeholders when evaluating acquisition offers. 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. We find that the adoption of a constituency statute significantly increases the UI effect on acquisition likelihoods by 37%. Since state constituency statute adoption is mostly exogenous, 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 and 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 correlated with a board's propensity to consider the impact of UI on unemployed workers.

First, 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 because 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 boards to break any implicit contracts with current employees so that they can realize an immediate takeover premium.

Second, 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.

Third, we conjecture that 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. 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. Local shareholder preferences would create another incentive for boards to take employee welfare into account.

Lastly, we conjecture that boards with female independent directors (IDs) are more likely to consider employee interests. A large literature from social psychology³ to finance and economics suggests that female leaders are more concerned about employee welfare, including unemployment risk, than male

³ Eagly and Crowley (1986) find that women are more community-minded and more caring about others than are men. Experiments also find that women are more altruistic and long-term oriented than men (Andreoni and Vesterlund, 2001; Silverman, 2003).

leaders are (Eagly and Crowley, 1986; Adams, Licht and Sagiv, 2011; Matsa and Miller, 2013). Thus, we expect firms with female IDs to more frequently consider employee interests in acquisition decisions.

Consistent with the firm characteristics that we identify above capturing meaningful variations in the likelihood of boards seriously considering employee unemployment costs, we find that the UI effect is significantly weaker for firms with high ownership levels by short-term institutional investors. On the other hand, the UI effect is more pronounced for firms in high labor intensity industries or headquartered in counties with low population or high levels of social capital. The UI effect is also stronger for firms with female IDs and it rises as the number of female IDs increases above one.

According to our hypothesis, the main reason for the rise in acquisition likelihood after target UI levels are raised 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 reduction in employee headcount at the combined acquirer and target firms, measured from two years prior through three years after deal completion. Also, as expected, this relation is driven by within-industry acquisitions where workforce reductions and restructurings are a more important source of synergies. Hence, not only are the acquisition likelihood results consistent with our hypothesis, but so are the magnitudes of post-acquisition layoffs.

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 target 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 levels and acquisition likelihoods 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 Related Literature

Our study challenges a dominant view in the M&A literature that target firm directors only consider the interests of shareholders and senior 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 on whether to accept acquisition offers. As individual unemployment cost falls, these boards become more willing to accept acquisitions with larger layoff plans. 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 in practice management (including directors) weigh the interests of non-shareholder stakeholders when making major decisions. Our findings lend support to a view expressed by some CEOs on the Business Roundtable after it changed its corporate purpose statement in 2019 from maximizing shareholder value to delivering value to all stakeholders, that the new Business Roundtable statement better reflects existing practices in running their companies, rather than a major departure from current practices.⁴

Our study also contributes to the literature that shows labor policies and laws have significant impacts 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 is associated with lower acquirer returns. Our evidence suggests that unemployment insurance can be an attractive means of providing employee protection

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/>)

because, unlike employment protection laws and labor unions that reduce takeover market activities, UI facilitates the reallocation of resources through mergers and acquisitions. 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 UI on individual and firm decisions and the economy. UI is found to distort labor supply (Meyer, 1990; Katz and Meyer, 1990; Chetty, 2008), but smooth individual consumption (Gruber, 1997), reduce mortgage defaults (Hsu, Matsa and Melzer, 2018), attenuate the effect of adverse labor demand shocks on the local economy (Di Maggio and Kermani, 2015), and spur entrepreneurial activity (Hombert *et al.*, 2020). With respect to firm decision-making, Agrawal and Matsa (2013) find that more generous UI benefits are associated with increases in firm leverage, while Ellul, Pagano and Schivardi (2018) report that family firms provide less employment stability to employees when public UI benefits are higher. Our study highlights another efficiency benefit of UI: it improves the 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 covers more than 97% of U.S. wage and salary workers and is one of the largest social security programs in the U.S. by the level of spending. The primary objective of UI programs is to alleviate hardships that are a result of lost wage income during periods of unemployment. The UI system is organized as a joint federal-state system where states have extensive flexibility in determining UI program parameters, such as the eligibility, weekly benefit amount, and the maximum duration of 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 over 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 are 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

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 the 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 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 IA.1 in the Internet Appendix, 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

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

subperiod respectively. One clear observation from this 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.

To verify the exogeneity of state UI level changes, we estimate individual correlations of state UI levels with state GDP, state unemployment rate, state union coverage, and gubernatorial elections conditioning on state and year fixed effects. The results, which are reported in the Internet Appendix Table IA.1, show no evidence of any significant correlation between changes in state UI levels 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 precisely the type of exogenous shocks that we need for testing our hypothesis: they change employee unemployment costs, but they are largely uncorrelated with state economic and political 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 in subsequent years. If this is the case, then any positive relation between state UI levels and acquisition likelihoods could be driven by reverse causality. In columns 6-10 of Appendix Table IA.1, we show in state-panel regressions that state UI levels are not correlated with contemporaneous or lagged statewide acquisition ratios of firms headquartered in the state. 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 target firms are better able to control takeover bid outcomes from then on. By 1986, many states had passed antitakeover

statutes. Shareholder right plans also came into widespread practice after the Delaware Supreme Court affirmed the legality of shareholder rights plans in a landmark 1985 case,⁶ which led to many other states recognizing shareholder right plans (Cain, McKeon and Solomon, 2017). 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. 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 it is limited by 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.⁷ 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.⁸ 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 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

⁶ Moran v. Household International, Inc., 500 A.2d 1346 (Del. 1985).

⁷ 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 85,723) associated with firms changing headquarters states.

⁸ 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.

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 employing this added 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, recapitalizations and leveraged buyouts (LBOs).⁹ Deals that meet the above criteria are included in our CSRP-Compustat firm-year panel dataset. Our key dependent variable, *Acquisition*, is set to one in the deal announcement year for each affected target firm and is otherwise set to zero. After eliminating observations with missing firm and deal characteristics, the final panel dataset consists of 85,723 firm-year observations, involving 3,479 acquisitions. The unconditional likelihood of an ultimately successful acquisition is 4% per annum.

We control for several state-level macroeconomic variables and an indicator for gubernatorial election years in our regressions.¹⁰ State monthly unemployment rates are obtained from the monthly U.S. Current Population Survey database. We then take the average of the monthly rates to obtain the state's average unemployment rate 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.

⁹ We exclude LBOs from our sample because they are distinctively different from the common types of acquisitions and thus, are typically excluded in general M&A studies. Nevertheless, since LBOs are widely perceived to be accompanied by significant labor restructuring, it would be interesting for future research to examine whether the effect we identify in this study holds in the LBO sample.

¹⁰ Jens (2017) finds that political uncertainty in the period prior to U.S. gubernatorial elections affects both state-wide investment and financing activities.

Table 1 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. Further information about UI levels is provided in the internet appendix. Table IA.2 reports the summary statistics for UI levels by state. The table shows significant differences across states in mean UI levels. UI levels also exhibit large time-series variations within each state 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, j, s, t index firm, industry, headquarters state, and year, respectively. 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 and it is 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 target firm characteristics typically 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, Golubov and Volpin, 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 state 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 at the state level. Since the main source of variation in UI levels 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). In unreported results, we find that our main results are robust to clustering at the state-year level. This model provides consistent estimates of the marginal effect of UI.

4. UI Level and Acquisition Likelihood

4.1. Preliminary Evidence

Before we estimate Equation (1), we first plot the average abnormal annual change in a state's acquisition ratio against the average abnormal annual change in the state's UI level for each state in Figure 1. The state-level acquisition ratio is defined as the proportion of firms headquartered in the state according to the Compustat database that are acquired each year. To reduce noise in the state-level acquisition ratio, we exclude state-years with fewer than 10 Compustat firm acquisitions.¹¹ The abnormal annual change in the state-level acquisition ratio equals the residual from a regression of the annual change in the state-level acquisition ratio on year fixed effects. The abnormal annual change in state UI level is calculated similarly as the residual from a regression of the annual change in the state UI level on year fixed effects. The abnormal annual changes for both variables are respectively averaged over time to obtain the average abnormal annual change for each of them. Figure 1 shows a clear positive relation between the two variables, which is consistent with state UI level increases leading to a higher fraction of firms being acquired in the state.¹² This simple correlation, however, is only suggestive and can reflect other differences across states. We next examine the relation more rigorously using regressions.

¹¹ This sample restriction excludes 67 state-year observations from our sample.

¹² In Table IA.10 of the Internet Appendix, we conduct a formal test of the relation between UI level and acquisition likelihood at the state level controlling for state and year fixed effects. We find that the relation is statistically significant, particularly when modeled in first differences. We thank an anonymous referee for suggesting this test.

4.2. Baseline Results

We begin by estimating Equation (1) without controlling for state economic conditions and gubernatorial elections in columns 1 and 2 of Table 2. Since the UI benefits available to target employees are less accurately measured for firms with geographically dispersed operations, we estimate Equation (1) first for the full sample (column 1) and then for the subsample that excludes firms in industries with geographically dispersed operations (column 2). We find that the coefficients of the target state UI level are 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, which is consistent with the target UI levels in column 2 containing smaller measurement errors due to the exclusion of firms in industries known to have geographically dispersed workforces.¹³ We then add state-level controls to the regressions in columns 3 and 4 and re-estimate Equation (1) for the two samples. Controlling for these state-level variables 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 target UI level coefficient rises with these added statewide controls. These results support the prediction of our main hypothesis and suggest that a sizable proportion of target firm boards of directors consider employee unemployment costs when evaluating acquisition bids.

In terms of economic importance, 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. This represents a 10% increase over the unconditional acquisition likelihood for a typical firm in our sample. The magnitude of the UI effect appears reasonable in the context of existing findings in the UI and M&A literature. First, existing UI studies suggest that a \$1000 or a 10% increase in UI benefits can have a significant marginal effect on the quality of life of unemployed workers. For example, Meyer (1990) 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)

¹³ 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.

estimates that in the absence of UI, consumption by 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%. Thus, it is reasonable that a \$1000 increase in UI level can change a target board's concern for its employees' unemployment risk.

Second, the estimated UI effect is considerably smaller than the effects of labor unions and both a target CEO's preferences and golden parachute on acquisition likelihoods. For example, Tian and Wang (2021) find that firms 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 below the average 65 years old retirement age. Machlin, Choe and Miles (1993) find that golden parachutes increase the odds of CEOs selling their firms by 114%.¹⁴ Compared with these estimates, the 10% rise in acquisition likelihood for a \$1000 or 10% increase in UI levels is more modest. This is expected because the target board's protection of employees is unlikely to be as strong as the union's protection of employees, and the target board is likely to give a target CEO's welfare more consideration than its employees when making such decisions, especially when the CEO is entrenched.

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 the long run they should 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 rise in UI level represents an exogenous decrease in unemployment costs of laid-off workers in California, especially in the first few years after

¹⁴ 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%.

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 increasing 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.

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-year fixed effects in Equation (1) partially mitigates this concern, the U.S. census region is quite large. The heterogeneity in economic shocks across states within the same region may still explain our results. Including state-year fixed effects is not an option because they would completely subsume the coefficient on the target state UI level. To better address this econometric 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 rise). This approach assumes that firms headquartered in bordering counties on 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 a rise in UI level of 10% or more from the previous year and call these large UI rise events. For each large UI rise 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 of the treatment firms as potential control firms and further require that the states of these bordering counties do not have a large UI rise 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 rise event (i.e. event year -1) as follows:

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

where i, j, s, c, t index firm, industry, headquarters state, headquarters county, year, respectively. $Large\ UI\ Rise_{s,k}$ equals one if the firm's headquarters state s has a large UI rise event and the observation is k years from the base year and equals zero otherwise. The coefficient estimates of 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 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 .

In Figure 2, we plot coefficient estimates of β_k 's together with their 95% confidence intervals. The acquisition likelihoods for treatment and control firms appear to follow the same time trend prior to the large UI rise events. However, one year after the large UI rise 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. The details of the regression results are reported in Internet Appendix Table IA.3.

5. Establishing the Link between the UI effect and Target Boards of Directors

The causal relation from target state UI levels to acquisition likelihoods can potentially be driven by multiple factors. In this section, we provide evidence to link it to the target boards' consideration of employee unemployment costs. First, we rule out two alternative explanations, Then, we explore an exogenous shock to show a causal impact of target boards' stakeholder orientation on the relation between the target state UI levels and acquisition likelihoods.

5.1. Can Labor Union Opposition Explain the UI Effect?

One factor that may drive the positive relation between target state UI levels and acquisition likelihoods is labor union opposition in target firms. When employees are represented by labor unions, the

labor unions can help to protect their interests. Prior studies suggest that labor unions can affect acquisition outcomes through lobbying and demonstrations (Pagano and Volpin, 2005) or hindering post-merger labor restructuring (Tian and Wang, 2021). Since an increase in UI level lowers the unemployment costs of laid-off workers, it may reduce the incentive of labor unions to oppose takeovers or to collude with managers to protect the firm from hostile bidders and thus, raises the likelihood of unionized firms being acquired.¹⁵

To test this, we collect industry-level data on the percentages of workers covered by collective bargaining agreements with their employers (Hirsch and Macpherson, 2003). 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 classify firms into high and low unionization groups based on whether during our sample period, the time-series average of union coverage rates in a firm's primary 2-digit SIC industry is above the sample median of the time-series averages across all industries or not. We rely on the cross-sectional variation in time-series average union coverage rates to define high and low unionization groups because Chen, Kacperczyk and Ortiz-Molina (2011) show that the primary source of variation in union coverage rates is the between-industry variation and they use the time-series average union coverage rates to be their main measure of unionization. The labor union opposition channel predicts that the UI effect should be mainly driven by firms in the high union coverage group.

In columns 1 - 4 of Table 3, we report the results from estimating Equation (1) in subsamples of firms in high and low unionization industries respectively. We find that the coefficient of UI level is positive and statistically significant in both subsamples, although the coefficient estimate is slightly larger for high unionization firms. In columns 5 and 6, we combine the two subsamples into one and test

¹⁵ 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 exceed the increase in UI benefits. Second, only a very small percentage of firms have ESOP plans in place among U.S. public firms. Kim and Ouimet (2014) find only 4,597 firm-year observations with ESOP plans between 1981-2001 among U.S. public firms. Hence, ESOPs are very unlikely to drive our results.

whether the UI effects are significantly different for high and low unionization firms by including an interaction term between the *Target UI level* and an indicator for firms in High Unionization industries in Equation (1). We find that the coefficient of the interaction term is statistically insignificant, suggesting that the UI effects in the high and low unionization subsamples are not statistically different. Overall, the evidence is inconsistent with labor union opposition being the main driver of the relation between target state UI levels and acquisition likelihoods.

5.2. Can Changes in Deal Characteristics Explain the UI Effect?

The second possible explanation for the relation between target state UI levels and acquisition likelihood is that it is driven by changes in deal characteristics. Since we control for target firm characteristics in our acquisition likelihood regressions, the UI effect is unlikely to be driven by changes in target firm characteristics. However, suppose that there are changes in deal characteristics that make the deals more attractive to target boards, as for example, when there is an increase in the takeover premium, then a rise in acquisition likelihood may be attributable to changes in deal characteristics rather than to the weight that a target board's places on employee interests.

To test this channel, we regress a host of deal characteristics on target state UI levels while controlling for asset sizes and book-to-market ratios of acquirer and target firms, target state macroeconomic conditions, and state and year fixed effects. The takeover premium is taken from the SDC database. It equals the difference between the offer price and a target's stock price four weeks prior to the deal announcement date divided by this latter price. For robustness, we also use the target's cumulative market-adjusted abnormal stock returns (CAR) over the event window (-20, 1), where day 0 is defined as the takeover announcement date, as a second measure of the takeover premium. The payment method is measured using both an indicator for an all-stock deal and the percentage of cash payment offered. The deal duration is calculated as the number of days between the announcement date of a deal and its effective or completion date. The deal value and deal attitude are obtained directly from the SDC database.

We find that the coefficient of the *Target State UI Level* is statistically insignificant for all the deal characteristics above. This is inconsistent with changes in deal characteristics driving the target state UI

level – acquisition likelihood relation. For brevity, we only report in Table 4 the estimates for the sample of acquisitions where the target UI levels are more accurately measured (i.e., excluding acquisitions where the target firms are in industries with geographically dispersed operations).¹⁶

5.3. *The Adoption of the Constituency Statute and the UI Effect*

The rejection of the two alternative channels above leaves the target boards' consideration of employee interests as the most likely explanation for the UI effect. In this section, we exploit the staggered adoption of constituency statutes by U.S. states to establish a causal relation from target boards' consideration of employee interests to the UI effect on acquisition likelihoods.¹⁷ The constituency statutes explicitly authorize directors of target firm boards to consider the impact of their decisions on all firm stakeholders, including employees and the local community (Bainbridge, 2005).¹⁸ 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 give greater weight to the impact of acquisitions on employee unemployment risk, which should lead to an observed increase in the average UI effect. Note that a firm headquarters state is often different from its incorporation state, where Delaware is by far the most frequent.

A significant advantage of this setting is that the adoption of constituency statutes is exogenous to the affected firms.¹⁹ To assess the treatment effect of the constituency statute on the UI effect, we estimate the following difference-in-differences regression:

$$y_{ijhst} = \alpha UI_{h,t-1} + \sum \beta_k Law_{s,t-1}^k + \sum \gamma_k UI_{h,t-1} \times Law_{s,t-1}^k + \delta X_{i,t-1} + \mu_{hj} + \mu_{jt} + \mu_s + \varepsilon_{ihst}$$

(3)

¹⁶ The full sample result is quantitatively similar and reported in the Internet Appendix Table IA.4.

¹⁷ 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.

¹⁸ By the end of our sample period, 35 states have adopted a constituency statute. 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.

¹⁹ 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.

where i, j, h, s, t, k index firm, industry, headquarters state, incorporation state, year, and the type of antitakeover law. The binary dependent variable y_{ijhst} equals 1 if firm i is an acquisition target in year t . Although our interest is in the constituency statute, we control for the passage of other second-generation antitakeover laws, i.e., the business combination law, poison pill law, control share law, and fair price law. (Karpoff and Wittry, 2018). $Law_{s,t-1}^k$ equals 1 if the firm's incorporation state s passed antitakeover law k 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 including it in the equation, so that the coefficient β_k measures the effect of passing state law k in a target firm's incorporation state on its acquisition likelihood when the UI level in its headquarters state equals the sample average UI level. $X_{i,t-1}$ is defined as the same vector of state-level and firm-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 a target firm's headquarters and incorporation states.

We predict that the coefficient γ for the constituency statute is positive and statistically significant. For the other laws, the predictions on the sign of γ is ex-ante ambiguous. On the one hand, these laws can give target boards more room to consider the interests of employees by giving them more control over the takeover outcomes, which predicts a positive γ . On the other hand, these laws can increase managerial entrenchment. Entrenched managers may place greater weight on their own self-interests and less on the interests of shareholders and employees. If this effect dominates, γ will be negative.

Table 5 reports the regression estimates from Equation (3). Columns 1 and 2 include state-level controls but not firm-level controls, while columns 3 and 4 include both state-level and firm-level controls. The coefficient γ for the constituency statute is positive and statistically significant at the 1% level in all columns, consistent with it strengthening the positive relationship between target state UI levels and acquisition likelihoods. The point estimates in column 2 indicate that the adoption of constituency statutes increased the UI effect by 36.6% ($0.0026/0.0071 = 0.366$). The coefficients γ for several other laws have mixed signs. The negative γ for the business combination and the poison pill laws is consistent with prior

evidence that the passage of these laws significantly increases agency problems due to management entrenchment (Bertrand and Mullainathan, 2003; Giroud and Mueller, 2010; Karpoff and Wittry, 2018). The positive γ for the fair price law is consistent with Cain, McKeon and Solomon (2017), who find that fair price law reduced hostile takeovers, while business combination and poison pill laws have no discernible impact on hostile takeover activity.

5.4. What Firms Are More Likely to Consider Employee Interests?

Our hypothesis does not assert that all corporate boards seriously consider employee interests when evaluating acquisition offers. Guided by existing economic and social theories and empirical findings, in this section, we identify five firm characteristics that should be correlated with the boards' propensity to consider the impact of an acquisition on their employees and examine the variation in the UI effect on acquisition likelihoods under these five firm characteristics.

5.4.1. 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 realizing a sizable takeover premium for shareholders by accepting the 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:

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

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

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 6, columns 1 - 2, we report the estimates from Equation (1) augmented by an indicator for high short-term institutional ownership and its interaction with the *Target UI level*. Consistent with our expectation, we find that the relation between the target UI level and the 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 significant positive coefficient on the high short-term institutional ownership indicator.

5.4.2. 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 labor-intensive firms. Except for high-tech industries, 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 give greater weight to employees' expected unemployment costs when assessing an acquisition offer than boards of 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 Bureau of Economic Analysis Industry Accounts data, where we convert the data based on the 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 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 *Target UI level*.

Columns 3 and 4 of Table 6 present the results of the above test. We find that the coefficient on *UI level* $\times$ *High Labor Intensity* is positive and statistically significant, suggesting that UI changes have a larger positive effect on the acquisition likelihood of firms with high labor intensity. The coefficient estimate in column 3 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 the *High Labor Intensity* indicator is subsumed by the industry-year fixed effects, so it does not appear in the table.

5.4.3. Population of a 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 and their families 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 (Stebly, 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 likely to give more weight to 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 the county by 6%. The reduced negative demand shock brings two benefits to 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 another firm in the local area.

To test this conjecture, we obtain data on annual county populations from the U.S. Census and define a county as having a low population if it is ranked in the bottom quartile of all counties in the year.²⁰ In Table 6, 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 the *Target UI level*. We find that the target state UI level - acquisition likelihood relation is more pronounced for firms headquartered in less populated counties. In addition, 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. Both findings are consistent with our conjecture that the boards in these firms are more concerned about the impacts of layoffs.

²⁰ The results are robust to using median population as cut-off.

5.4.4. Social Capital in a 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) 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 director-firm matching on social values. 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) levied on a firm's directors, especially local directors. Third, social capital is 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 implicit contract use that increases firm value ex-ante, but ex-post obligates the board to give sizable weight to employee interests when assessing acquisition offers. Since employees in high social capital counties are apt to place greater trust in a board not to renege on their promises ex-post, employees are more likely to willingly enter implicit contracts ex-ante.

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 rates 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 in the county. 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 missing social capital levels in the intervening years using the level for the closest prior year when the index is available, following Hasan *et al.* (2017). For example, we fill in missing social capital levels for the years between 1991 and 1996 using the index level in 1990 and so on. We define a high social capital indicator to equal one if the social capital of the county in that year is in the top quartile of our entire sample and zero otherwise.

In Table 6, 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 coefficient of this interaction term is positive and statistically significant at the 1% level. 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.

5.4.5. Boards with Female Independent 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 arise. 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 disproportionately made the latter choice relative to men. Matsa and Miller (2013, 2014) find that firms with female directors or owners are less likely to downsize their workforce even during a crisis.

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 (Masulis, 2020).

The results are reported in Table 7. Columns 1 and 2 report the estimates of Equation (1) for the subsample of firms with no female IDs, columns 3 and 4 report the estimates for the subsample of firms with at least one female ID, and columns 5 and 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 apt to consider employee interests when evaluating an acquisition offer.

6. UI Level and Post-Merger Workforce Restructuring

According to our hypothesis, the rise in acquisition likelihood following an increase in the target state UI level should be mainly driven by acquisitions involving larger post-merger layoff plans. In this section, we test whether the scale of post-merger workforce restructuring is positively related to the target state UI levels. Ideally, workforce restructuring should be measured by employee turnover levels, however, the Compustat database only reports total employment levels for listed firms at the end of each fiscal year. Thus, we measure workforce restructuring by the change in acquirer plus target employee

headcounts from the year before to the three years after deal completion, although this measure has some obvious limitations.²¹

Following Dessaint, Golubov and Volpin (2017), we track the combined number of acquirer and target firm 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 an 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's 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 target state's UI level in event year -1 and the post-acquisition indicator. In all models, we control for deal fixed effects so that the combined number of employees is compared within a given deal. We also control for industry-year fixed effects so that the change in the combined number of employees to be explained by UI is the abnormal change relative to that occurring in firms in the same industry in this sample that have not yet been acquired.

The target state's UI level is demeaned relative to the overall sample mean. We do so to make the coefficient of the post-acquisition indicator easier to interpret. With the demeaning, the coefficient of this indicator measures the abnormal percentage change in the combined number of employees following an acquisition for a target firm that has the sample average UI level.²² The demeaning, however, does not

²¹ 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.

²² Without demeaning, the coefficient measures the abnormal percentage change in the combined number of employees where the target firm has zero UI benefits.

change the coefficient of the interaction term between the target state's UI level and the post-acquisition period indicator.

Table 8 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 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-Merger* 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 high and low unionization subsamples. The relation between target UI levels and workforce reductions is statistically significant in both subsamples. The significant relation in the low unionization subsample is consistent with our earlier finding that labor union opposition does not appear to explain the UI effect. Thus, the target board's weight placed on employee interests plays an indispensable role in driving the target 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 to laid-off employees, 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 pays usually amount 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. Although initiating severance pay is mostly a voluntary decision of employers, under the following circumstances, employers are required to provide severance pay to their employees. First, if it is contained in the employee's employment contract. Second, if it is mentioned in the employee handbook. Third, if it was 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 all its employees in future terminations. In this latter case, the cost of including a first-time 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 to offset 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 it is paid.

8. Conclusions

The shareholder primacy doctrine has dominated corporate governance discussions for decades. As a result, for decades, stakeholder issues have received little attention in finance research. However, in recent years stakeholder issues have become topics of heated debate among politicians, business leaders, and academics in the context of rising concern for the negative impact of corporations on non-shareholder stakeholders. The political and public pressure on companies to take a stand on this general debate has led the Business Roundtable to change its corporate purpose statement in 2019. While both sides of the debate agree that corporations should serve the interests of both shareholders and non-shareholder stakeholders, they disagree on the best way to achieve the goal. One important question in this debate is

whether corporate boards already consider non-shareholder stakeholders' interests when making decisions under current corporate law.

This paper shed light on this question by examining whether employee interests affect target board decisions in mergers and acquisitions. By exploiting an ideal econometric setting provided by state UI benefit changes, we find that a sizeable proportion of target boards consider employee unemployment costs when making decisions on acquisition offers. Target boards are more willing to accept acquisition offers when the unemployment costs of employees are lower *ceteris paribus*. We conduct a litany of tests to establish that the relation is causal and attributable to the target boards' consideration of employee interests. This relation is driven by boards in at least five types of firms, including firms in labor-intensive industries, firms headquartered in less populated or high social capital counties, firms with female independent directors, and firms with low ownership by short-term institutional investors.

Our finding that a sizeable proportion of target boards consider employee interests when assessing acquisition offers suggests that corporate board decision-making is more complex than what existing finance models suggest. Although some theories have been developed to rationalize corporate boards' consideration of non-shareholder stakeholders' interests, such as Hart and Zingales (2017), more research is called for to better understand the specific forces that drive corporate boards' incentives.

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Figure 1: Average Change in State Acquisition Ratios vs. Average Change in State UI Levels (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 the 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 and 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.

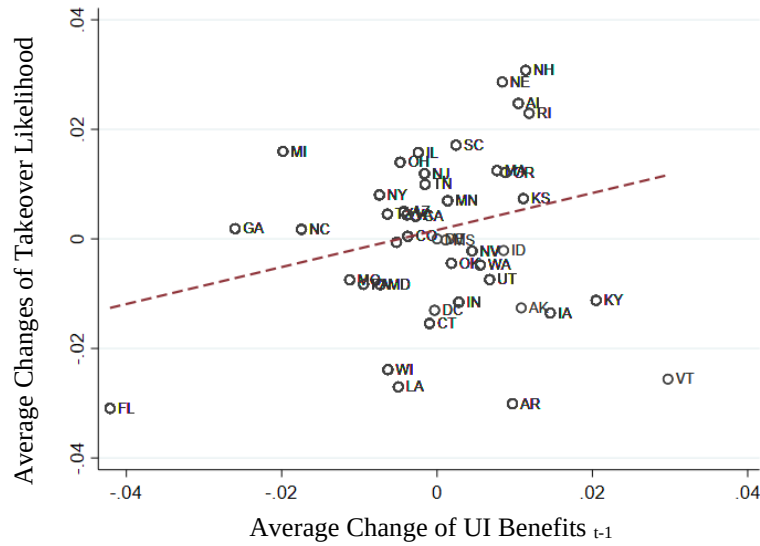


Figure 2: Dynamic Treatment Effects Around Large UI Rises

This figure plots the point estimates and their 95% confidence intervals of the effects of a large state UI rise on firm-level acquisition likelihood by year over the $[-4, +4]$ year event window around the large UI rise event in year 0 estimated from Equation (2). A large UI rise 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.

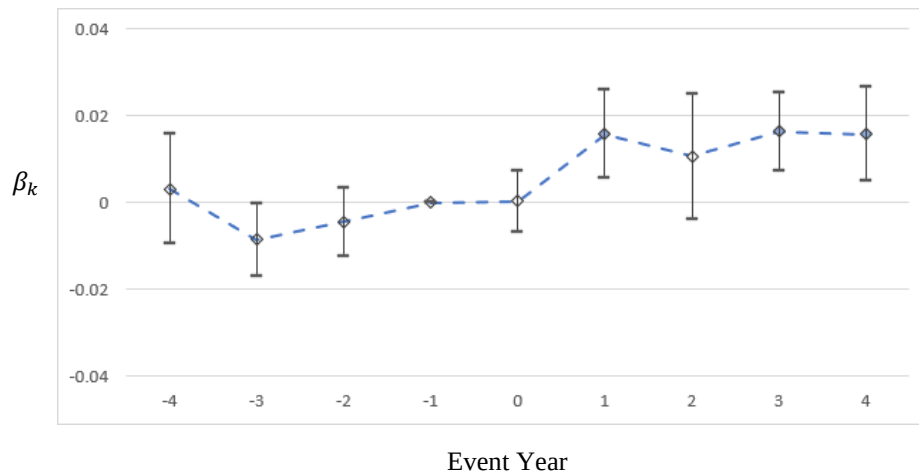


Table 1: Summary Statistics

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 Internet Appendix Table IA.13. All continuous variables except UI levels are winsorized at the 1st and

	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	2359.63	6525.01	68.41	301.04	1338.07
Union Coverage	85,550	0.12	0.09	0.04	0.11	0.12
Constituency Statute (0/1)	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 Institutional Ownership	19,982	0.29	0.14	0.20	0.29	0.38
Combined No. of Employees (000s)	8,838	42.95	72.95	4.00	12.69	49.72
Panel B: Governance Characteristics						
Board Size	22,115	9.05	2.26	7	9	10
Independent Board (0/1)	22,115	0.80	0.40	1	1	1
Number of Female IDs	22,115	0.99	0.97	0	1	2
Panel C: State-Level Variables						
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
Panel D: County-Level Variables						
Social Capital	69,954	0.00	1.64	-0.82	-0.64	-0.51
County Population	71,501	1,517,576	1,783,822	524,960	934,140	1,663,167
Panel E: Deal Characteristics						
SDC Premium	1,494	0.46	0.39	0.22	0.40	0.62
Target CAR	1,779	0.27	0.30	0.07	0.23	0.42
All-Stock (0/1)	1,828	0.26	0.44	0.00	0.00	1.00
% Cash	1,828	53.24	46.45	0.00	64.96	100.00
Deal Duration (days)	1,828	136.66	127.99	66.00	100.00	159.00
Log of Deal Value (\$ million)	1,805	5.78	1.94	4.49	5.76	7.15
Friendly Deal	1,828	0.84	0.37	1.00	1.00	1.00
Panel F: Unemployment Insurance						
Target UI level (\$ thousand)	85,723	9.63	4.60	6.37	8.61	11.70

99th percentiles.

Table 2: Target State UI Levels and Acquisition Likelihoods

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. Variable definitions are provided in the Internet Appendix Table IA.13. 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)
	<i>Acquisition [0, 1]</i>			
Target UI level $t-1$	0.0033** (0.0013)	0.0036** (0.0014)	0.0040*** (0.0015)	0.0043** (0.0016)
ROA	-0.0046 (0.0090)	-0.0003 (0.0095)	-0.0048 (0.0090)	-0.0005 (0.0095)
R&D/Assets	0.0607*** (0.0108)	0.0631*** (0.0120)	0.0617*** (0.0109)	0.0638*** (0.0121)
Leverage	0.0067 (0.0062)	0.0079 (0.0065)	0.0068 (0.0062)	0.0078 (0.0065)
Sales Growth	0.0010 (0.0020)	0.0005 (0.0019)	0.0011 (0.0019)	0.0006 (0.0019)
PPE/Assets	-0.0115*** (0.0042)	-0.0116** (0.0045)	-0.0117*** (0.0042)	-0.0119** (0.0045)
Market-to-Book	-0.0035*** (0.0006)	-0.0036*** (0.0006)	-0.0035*** (0.0006)	-0.0036*** (0.0006)
Firm Size	-0.0002 (0.0006)	-0.0002 (0.0006)	-0.0003 (0.0006)	-0.0003 (0.0007)
Firm Age	-0.0040** (0.0019)	-0.0046** (0.0023)	-0.0039* (0.0020)	-0.0046* (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	73,147	85,723	73,147
Adj. R-squared	0.0208	0.0194	0.0221	0.0207

Table 3: 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 Unionization* is an indicator that equals 1 if the time-series average of the union coverage rates in a firm's 2-digit SIC industry during our sample period is above the sample median of these time-series averages across industries, and 0 otherwise. In all models, we 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 Internet Appendix Table IA.13. 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)	(5)	(6)
			<i>Acquisition [0, 1]</i>			
	High Unionization		Low Unionization		Full Sample	
Target UI level $t-1$	0.0041*** (0.0015)	0.0042*** (0.0015)	0.0035** (0.0017)	0.0040* (0.0020)	0.0034** (0.0017)	0.0039* (0.0020)
Target UI level $t-1 \times$ High Unionization					0.0007 (0.0008)	0.0002 (0.0010)
State Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
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	37,833	34,895	47,717	38,079	85,550	72,974
Adj. R-squared	0.0261	0.0245	0.0201	0.0187	0.0221	0.0208

Table 4: Target State UI Levels and Deal Characteristics

This table examines the relation between target state UI levels and several deal characteristics. The sample consists of all announced deals in our sample, excluding those where the target firm is in an industry with geographically dispersed operations. *SDC premium* is the takeover premium from the SDC database. *Target CAR* equals the target firm's cumulative market-adjusted abnormal stock returns (CAR) over the event window (-20, 1) where day 0 is defined as the takeover announcement date. *All-Stock* is an indicator for all-stock deals. *% Cash* equals the percentage of cash payment in the transaction. *Deal Duration* equals the number of days between the deal announcement and the deal effective date. *Deal Value* equals the natural logarithm of deal value. *Friendly Deal* is an indicator that equals 1 if the deal attitude is friendly and 0 otherwise. All columns include state-level controls (*log annual real GDP per capita*, *unemployment rate*, *gubernatorial year indicator*). Variable definitions are provided in the Internet Appendix Table IA.13. All continuous variables (except UI levels) are winsorized at the 1st and 99th percentiles. The standard error is double-clustered by the acquirer and target headquarters states and is reported in the parenthesis. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels using two-tailed tests, respectively.

	(1) SDC Premium	(2) Target CAR	(3) All-Stock	(4) % Cash	(5) Deal Duration	(6) Deal Value	(7) Friendly Deal
Target UI level $t-1$	0.0026 (0.0052)	0.0028 (0.0038)	-0.0017 (0.0026)	0.5108 (0.5069)	0.9088 (0.6163)	-0.0070 (0.0115)	-0.0016 (0.0030)
Target Size	-0.0595*** (0.0062)	-0.0763*** (0.0032)	0.0009 (0.0083)	-0.9163 (0.8905)	35.8387*** (2.9207)	0.6236*** (0.0353)	-0.1125*** (0.0100)
Acquirer Size	0.0409*** (0.0070)	0.0564*** (0.0028)	-0.0408*** (0.0073)	4.5148*** (0.6691)	-11.1620*** (1.6217)	0.2276*** (0.0278)	0.0620*** (0.0082)
Target Market-to-Book	0.0004 (0.0119)	0.0231** (0.0092)	-0.0247** (0.0102)	-2.6493*** (0.8313)	11.3076*** (1.6844)	0.1086*** (0.0210)	-0.0177*** (0.0044)
Acquirer Market-to-Book	0.0093 (0.0150)	0.0099 (0.0089)	-0.0628*** (0.0135)	2.9006 (1.7446)	-13.1306*** (4.3505)	0.0218 (0.0444)	0.0123 (0.0111)
State Level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude Dispersed Industries	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,494	1,779	1,828	1,828	1,828	1,805	1,828
Adj. R-squared	0.0982	0.2226	0.1374	0.1767	0.2166	0.6604	0.2866

Table 5: 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 overall sample mean. 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 Internet Appendix Table IA.13. 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)
	<i>Acquisition [0, 1]</i>			
A: Target UI level $t-1$	0.0071*** (0.0017)	0.0081*** (0.0021)	0.0071*** (0.0016)	0.0081*** (0.0020)
Constituency Statute	-0.0003 (0.0047)	-0.0017 (0.0055)	0.0005 (0.0049)	-0.0008 (0.0055)
A × Constituency Statute	0.0026*** (0.0009)	0.0032*** (0.0011)	0.0029** (0.0011)	0.0035*** (0.0012)
Business Combinations Law	-0.0068 (0.0046)	-0.0054 (0.0063)	-0.0072 (0.0044)	-0.0050 (0.0061)
A × Business Combinations Law	-0.0037*** (0.0013)	-0.0045*** (0.0016)	-0.0038*** (0.0013)	-0.0045*** (0.0015)
Poison Pill Law	-0.0033 (0.0055)	-0.0020 (0.0059)	-0.0033 (0.0051)	-0.0022 (0.0057)
A × Poison Pill Law	-0.0040*** (0.0008)	-0.0047*** (0.0009)	-0.0041*** (0.0008)	-0.0049*** (0.0010)
Control Shares Acquisition Law	0.0105 (0.0087)	0.0091 (0.0095)	0.0107 (0.0093)	0.0095 (0.0101)
A × Control Share Acquisition Law	0.0013* (0.0007)	0.0013 (0.0008)	0.0011 (0.0007)	0.0011 (0.0008)
Fair Price Law	-0.0105* (0.0058)	-0.0133* (0.0067)	-0.0097* (0.0057)	-0.0138** (0.0065)
A × Fair Price Law	0.0014 (0.0009)	0.0016* (0.0008)	0.0017* (0.0009)	0.0019** (0.0008)
State-level Control	Yes	Yes	Yes	Yes
Firm-level Control	No	No	Yes	Yes
State-Industry FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Incorporation State FE	Yes	Yes	Yes	Yes
Exclude Dispersed Industries	No	Yes	No	Yes
N	84,043	71,811	82,180	70,087
Adjusted R-squared	0.0152	0.0155	0.0172	0.0174

Table 6: 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 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. *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). *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 Internet Appendix Table IA.13. 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)
	<i>Acquisition [0,1]</i>							
	<i>High Short-Term IO</i>	<i>High Labor Intensity</i>	<i>Low Local Population</i>	<i>High Local Social Capital</i>				
Target UI level $t-1$	0.0046** (0.0022)	0.0048** (0.0021)	0.0031* (0.0017)	0.0032* (0.0017)	0.0029* (0.0015)	0.0031** (0.0015)	0.0035** (0.0014)	0.0037** (0.0014)
Cross-Sectional Variable	0.0235*** (0.0066)	0.0212*** (0.0062)			-0.0336*** (0.0084)	-0.0344*** (0.0091)	-0.0471*** (0.0088)	-0.0546*** (0.0092)
Target UI level $t-1 \times$ Cross-Sectional Variable	-0.0010** (0.0004)	-0.0008** (0.0003)	0.0009*** (0.0003)	0.0008** (0.0003)	0.0029*** (0.0008)	0.0030*** (0.0008)	0.0044*** (0.0007)	0.0048*** (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	19,982	16,594	67,394	62,090	71,501	60,935	69,954	59,619
Adj. R-squared	0.0394	0.0376	0.0245	0.0233	0.0223	0.0218	0.0216	0.0212

Table 7: 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, and columns 2 through 6 are estimated using the subsample of firms with one and more than one female ID 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 Internet Appendix Table IA.13. 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)
	No Female IDs		Acquisition [0, 1] 1 Female ID		2+ Female IDs	
Target UI level $t-1$	0.0021 (0.0025)	0.0020 (0.0025)	0.0042** (0.0018)	0.0050*** (0.0017)	0.0067** (0.0030)	0.0079** (0.0039)
Independent Board	0.0028 (0.0052)	0.0046 (0.0062)	0.0115 (0.0073)	0.0130 (0.0088)	0.0320* (0.0159)	0.0410** (0.0174)
Board Size	-0.0005 (0.0020)	-0.0005 (0.0022)	-0.0002 (0.0012)	0.0002 (0.0014)	0.0001 (0.0015)	0.0010 (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 8: Target state UI levels 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 the overall 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 a high unionization industry, while columns 9 and 10 include only deals where the target firm is in a low unionization industry. High and low unionization are defined in the Variable Definitions Table in the Internet Appendix Table IA.13. All columns include deal fixed effects and industry-year fixed effects. Industries are defined by two-digit SIC codes. 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)
	<i>Log (Combined No. of Employees)</i>									
	All Deals		Within-industry Deals		Diversifying Deals		High Union Coverage		Low Union Coverage	
Post-Merger	-0.112*** (0.013)	-0.097*** (0.014)	-0.159*** (0.016)	-0.142*** (0.017)	-0.004 (0.020)	0.000 (0.021)	-0.154*** (0.010)	-0.152*** (0.010)	-0.083*** (0.007)	-0.056*** (0.007)
Target UI level _{t-1} × Post-Merger	-0.004* (0.002)	-0.004* (0.003)	-0.008** (0.003)	-0.009** (0.003)	-0.000 (0.003)	0.001 (0.004)	-0.006** (0.003)	-0.007** (0.003)	-0.004** (0.002)	-0.004** (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	3,559	3,365	5,269	4,413
Adj. R-squared	0.969	0.969	0.969	0.970	0.965	0.964	0.966	0.966	0.975	0.976

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