Unemployment Risk and Payout Policies

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Abstract

This paper argues that workers' unemployment risk may induce firms to adopt conservative payout policies. I show that firms increase their dividend payout following sharp increases in unemployment insurance generosity, that reduce workers' personal losses due to layoffs. Firms increase payout by about 6% following positive changes in protection for unemployed workers that are plausibly unrelated to macroeconomic conditions. This effect is driven by firms with poor growth prospects, high labor intensity, and in more volatile industries, suggesting that public insurance crowds out private insurance by firms. Thus, labor market considerations play an important role in shaping firms' payout decisions.

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1 Introduction

Unemployment risk faced by workers is a crucial determinant of firms' financial policies, as shown both in theoretical (Titman (1984) and Berk, Stanton, and Zechner (2010)) and empirical (Agrawal and Matsa (2013)) research. While shareholders can diversify away all the idiosyncratic risk, a worker's human capital is fully invested in the firm she is employed with. Since unemployment risk originated by a firm's distress would be priced in higher wages, firms may find it optimal to reduce fluctuations in workers' wages and lower the risk of distress, as predicted by implicit contract models along the lines of Baily (1974) and Azariadis (1975). To accomplish this, managers will engage in more conservative financial policies, such as lower leverage (Agrawal and Matsa (2013)) or R&D expenditures (Ellul, Wang, and Zhang (2015)).

In this paper, I argue that dividend payout policies, as a result of the above logic, may also be shaped by labor market considerations. The intuition is straightforward. When determining the aggressiveness of their payout policies, managers face a trade-off. A higher dividend payout may have benefits such as mitigating agency problems, signaling good earnings prospects, or attracting particular types of investors, like tax-free institutions. Oppositely, aggressive payout policies may reduce a firm's operating flexibility, especially given that dividend policies may be hard to reverse even in periods of distress.

Workers are likely to be among the most affected stakeholders by payout policies. First, a lower cash buffer may render a firm more vulnerable to the entry of competitors in the same market (Fresard (2010)) or to the tightening of financial constraints (Opler, Pinkowitz, Stulz, and Williamson (1999), Bates, Kahle, and Stulz (2009)). Consistent with this idea, firms with volatile earnings are unlikely to pay dividends (Jagannathan, Stephens, and Weisbach (2000)) Second, because managers may be reluctant to cut dividends during downturns to avoid sending negative signals to investors (Allen and Gale (2002)), workers may be exposed to layoffs to free up resources.

As a result, workers may negatively perceive cash transfers to shareholders as they may increase their human capital risk.[1]

Crucially, firms' incentives to give job security to workers are shaped by the generosity of the insurance provided by the public sector. Private insurance will be crowded out by public insurance, so long as the latter is effective in mitigating human costs of unemployment. If this is the case, firms may engage in more aggressive payout policies when the public unemployment insurance (UI) system is more generous. Indeed, an extensive literature, documents that UI provided by the public sector does indeed have first order effects on unemployed workers' welfare, for example by reducing their consumption volatility (as shown in Gruber (1997)).

In the empirical tests that follow, I analyze how dividend payout decisions are affected by large sharp increases in UI generosity across US states between 1991 and 2007. I identify 61 instances where a state legislator increased the maximum weekly benefit and document, in a difference-in-difference framework, that payout increases by about 4 percentage points following each event.

While this preliminary evidence is robust to a number of different empirical specifications, in most of the paper I focus on a restricted sample of events and firms. I proceed as follows. First, I identify the underlying cause and political context that was the basis of the policy shift. I obtain detailed information regarding 18 out of 61 reforms. I then identify 12 instances in which the precise timing of the reforms was neither likely to be related to changing economic conditions nor associated with the adoption of other significant policies. Such events are common, given that political factors unrelated to significant economic events often influence UI policies adopted by states (Blaustein et al. (1993)). Each firm headquartered in a treated state is matched with a control firm similar in

[1] For example, consider the following quote of a union leader from the "Massachusetts Jobs with Justice": *Some CEOs get bonuses if their company pays stockholder dividends above certain levels. The easiest way to increase dividends is to pay out profits rather than reinvesting them in the company's employees. There is no incentive for corporate CEOs to expand hiring and create new jobs with decent wages; instead, CEOs have a strong incentive to cut positions and reduce wages and benefits.* (Tom Iacobucci, "On Labor Day", The Valley Advocate (09/01/2011))

terms of a number of covariates, where both firms operate in the same industry and census region. This matching procedure provides a stringent test, as it controls for a number potential unobserved confounding factors, such as demand shocks, which may vary at the regional or industry level. I find that treated firms increase their payout by roughly 6% relative to matched control firms, although this effect is relatively short-lived. Results are qualitatively similar when restricting the analysis to firms that, previous to the increase in UI generosity, were not paying dividends.

Several robustness checks help rule out the possibility of omitted variables or alternative interpretations. First, the change in payout occurs only *after* a benefit increase is announced; no difference in trends can be detected before the policy change between treated and control firms. Second, treated and control firms are headquartered in states that share similar levels of growth in unemployment and income per capita. This suggests that macroeconomic shocks are unlikely to be a significant confounding factor. Third, I find similar results when I focus on different measures of payout, such as total payout (which includes repurchases), dividend per share, and dividend per asset.

The fact that I obtain similar results when I ignore the potential endogeneity related to the adoption of some of the policies and the possibility that treated and control firms are not well-matched is particularly reassuring for two reasons. First, it suggests that dramatically different results are unlikely when adopting different screening criteria for selecting the policy changes or using a different matching procedure. Second, the smaller magnitude of the effect of UI changes on payout obtained when using this less conservative approach underscores that some of these events, excluded from the main analysis, may be related to the anticipation of economic downturns. This would bias the coefficient downward, as firms may be less prone to increase dividends when anticipating periods of poor economic conditions.

The cross-sectional heterogeneity of these results supports this "implicit insurance" channel. I expect that firms employing extensively labor, as opposed to capital, in their production process will react more strongly to the provision of public insurance. Using two proxies for labor intensity (number of employees divided by either value of assets or value of property, plant, and equipment), I find that the effect on payout is significant and large only in high labor intensity firms.

Moreover, firms operating in less risky industries should be less affected by UI policies, given that in such sectors there should be little reason for insulating workers from adverse demand shocks. Using proxies for earnings and employment volatility at the industry level, I document that the increase in payout is present only in firms in high volatility industries.

Workers in firms with poor growth prospects are also more likely to be exposed to layoff risk. Consistent with this hypothesis, I show that the increase in payout is concentrated among firms with low profitability, profitability growth, or Tobin's Q, which are three strong predictors of future employment growth.

Finally, I show that UI generosity affects firms' payout policies only for firms in industries with low wage or employment growth. Intuitively, workers' costs of unemployment will be higher if the probability of being quickly re-absorbed by the labor market are low. This should be true especially for workers employed in industries with declining employment. Similarly, workers in industries with low wage growth will be unlikely to find attractive jobs after a period of unemployment.

Evidence on additional outcomes provides additional support to this implicit contract hypothesis. Investment growth is not significantly related to changes in UI policies, suggesting that the results are not driven by a decrease in growth opportunities. However, I find some evidence of a decline in cash holding and an increase in leverage and net leverage, albeit only for firms with negative earnings[2], consistent with the findings of Agrawal and Matsa (2013). Moreover, in line with the theoretical predictions of Baily (1974), Azariadis (1975), and with corroborating international

[2] These firms are excluded in the majority of the analysis because their payout ratio is not well-defined.

empirical evidence (Ellul, Pagano, and Schivardi (2014)), employment growth becomes more responsive to industry-level growth opportunities, as measured by the firms' industry median Tobin's Q.

Alternatively, plausible arguments which could in principle be consistent with such evidence find little support. Feldstein (1976) contends that UI policies act as an implicit subsidy to firms that experience large, seasonal shifts in demand. Possibly, this may induce managers to remunerate shareholders with higher dividends. Additionally, firms may find it optimal to reduce firm liquidity in order to counteract workers' bargaining power (Matsa (2010)). None of these alternative explanations find support in the data. The effect of UI policies on payout is largely independent of firms' sales seasonality or their state's degree of unionization.

This paper is related to recent work in Corporate Finance that argues that labor market considerations are important determinants of financial decisions. For example, (Serfling (2016) and Simintzi, Vig, and Volpin (2015)), particularly those that emphasize the effects of UI provisions on managers' choices (Agrawal and Matsa (2013), Ellul et al. (2015), Ellul et al. (2014), Dou, Khan, and Zou (2016)). Additionally, empirical work has documented that unions' power may influence payout decisions (He, Tian, and Yang (2016), DeAngelo and DeAngelo (1991), Chino (2016)). Even so, none of those papers have focused explicitly on the implicit contract channel I propose.

By documenting a relation between payout decisions and unemployment costs, I complement this empirical and theoretical work by analyzing another, important dimension through which the labor market impacts firms' actions. Moreover, while most of the literature on dividends payout focuses primarily on shareholders' and managers' objectives, I argue that other stakeholders' interests, such as those of workers, are important determinants of payout policies.

This paper is structured as follows. Section 2 surveys the related literature. Section 3 presents data and the empirical strategy. Section 4 presents the main empirical results and Section 5 analyzes the cross-sectional heterogeneity in the effect of UI on payout decisions. Finally, Section 6 concludes.

2 Literature Review

An extensive body of empirical work has documented that labor market considerations impact firms' financial decisions. Serfling (2016) and Simintzi et al. (2015) document that leverage is inversely related to labor rigidities due to higher firing costs both in the US and in a cross-section of countries. Matsa (2010) shows how firms use leverage strategically in order to improve their bargaining position vis-a-vis unionized workers, whereas Schmalz (2015) studies close unionization elections and finds that this relationship depends on a firm's degree of financial constraints.

Most studies at the intersection of labor and dividend payout policies focus on managers' conflict with unions. Associated work includes DeAngelo and DeAngelo (1991), who show that unionized firms sharply cut dividend payments in period of distress, and He et al. (2016), who illustrate how firms adopt conservative payout policies following unionization to preserve operating flexibility.

This paper is also related to empirical work on the effect of UI policies on unemployed workers. There is strong evidence that UI has the beneficial effect of allowing laid off workers to smooth their consumption (Gruber (1997), Bloemen and Stancanelli (2005), Browning and Crossley (2001)) or stabilize the business cycle (Di Maggio and Kermani (2015)).

Other papers have focused on the possible moral hazards created by more generous UI policies, which may induce workers to reduce effort in their job search (Solon (1985), Card and Levine (2000), Card, Chetty, and Weber (2007)). While there is overall support to the idea that UI may subsidize longer unemployment spells, there is disagreement about the actual magnitude of this effect. Chetty (2008) explicitly derives a formula for the optimal UI benefit which accounts for this trade-off. Supplemental work has focused on the potential improvement in matching resulting from the ability of the unemployed workers to search for a better job for longer periods of time, with ambiguous results (see for example Card et al. (2007)). Overall, this evidence supports the idea that UI provisions represent meaningful shocks to the labor market environment.

Theoretical and, more recently, empirical work, have found that it may be optimal for firms to provide partial insurance to workers in order to reduce wage premia related to unemployment risk. Theoretical papers include Baily (1974) and Azariadis (1975), which show that firms may be willing to avoid fluctuations in wages and, under certain conditions, employment by absorbing shocks connected with uncertain demand.

Relatedly, Titman (1984) has demonstrated that the optimal capital structure is related to the costs of distress borne by workers and other stakeholders (such as customers and suppliers). Similar predictions are obtained in a dynamic moral hazard model by Berk et al. (2010). Empirical work has found support for these predictions, relying on the observation that the more generous the public UI system, the lower unemployment costs. Agrawal and Matsa (2013) show that leverage is positively correlated with unemployment benefits in the US. Similarly, Ellul et al. (2015) show that a larger fraction of CEOs' compensation is based on stock or options when UI generosity is higher, suggesting that shareholders may induce managers to exploit riskier growth opportunities. Finally, Ellul et al. (2014) find that in countries with lower UI generosity, firms are less likely to lay off workers following negative industry shocks.

Additional evidence on the effects of UI policies on firms' decisions includes Dou et al. (2016), who find that firms are less likely to manage earnings in the presence of more generous UI policies. This suggests that firms try to improve employee perceptions of employment security when human costs of unemployment are large. Overall, the empirical evidence presented here complements this work and suggests an additional important channel through which the labor market affects managers' choices.

My results relate, more broadly, also to papers analyzing factors influencing firms' payout. A number of reasons may affect payout policies, such as signaling motives, agency conflict considerations, and management incentives (recent surveys include Farre-Mensa, Michaely, and Schmalz (2014) and DeAngelo, DeAngelo, and Skinner (2008)). Recent empirical work has, however, emphasized how such benefits are traded-off against costs in terms of loss of financial flexibility, the ability to avoid costly financial distress, as well as underinvestment.

Firms favoring dividends payment over repurchases are more likely to hedge risk using financial instruments, such as derivatives (Bonaim, Hankins, and Harford (2014)). These also tend to have more volatile cash flows (Jagannathan et al. (2000)). While these papers have typically emphasized either shareholders' or managers' payoffs, I find support for the hypothesis that others stakeholders' interests, such as those of workers, have a first-order influence on financial decisions, which was a concept posited theoretically by Titman (1984) and Cornell and Shapiro (1987).

3 Data and Descriptive Statistics

3.1 Data

Since the adoption of the Social Security Act in 1935, the US has established a joint federal-state system to provide benefits to unemployed workers. Each state sets the generosity of its UI program, which is funded through taxes levied on employers. Such taxes are "experience rated," meaning that firms more prone to lay off workers suffer higher marginal rates.

Such UI policies in the US represent a relevant setting to test my hypothesis. First, as suggested by the evidence surveyed in the previous section, they are salient and economically meaningful to workers. Second, they exhibit substantial variation, both across states and over time.

I obtain data on UI benefits for each state and year from the Department of Labor website[3] for each state and year. Similarly to Di Maggio and Kermani (2015) and Agrawal and Matsa (2013), I focus on the maximum benefit (maximum weeks × maximum weekly benefit amount) for each state. This measure is likely to be the most salient to workers and exhibits the highest variation across time and state.

Not all changes in UI benefits necessarily correspond to actual legislative acts for two reasons. First, in some states UI benefit changes are adjusted periodically and are linked to the macroeconomic environment. For example, the maximum weekly benefit changes in every year of my sample in Colorado. This is because UI benefits are revised annually and are typically mechanically indexed to the average wage in the manufacturing sector. Second, UI revisions can be pre-announced. For example, in 1990 the State of California raised the maximum weekly benefit from $166 to $190. However, it was also announced that subsequent increases would follow in 1991 ($210) and 1992 ($230).

In the following, I examine the response of dividend payout following sharp changes in UI generosity. Therefore, I exclude these instances because, in the first case, they are mechanically correlated with the macroeconomic outcomes of the state where firms are headquartered. In the second case, they are fully anticipated by managers. I obtain relevant UI changes from several editions of the changes in UI legislation bulletins published by the Bureau of Labor Statistics for the period of 1991 to 2007. I code all the legislative changes remaining after adopting the previous two filters. In order to focus only on meaningful changes, I also set a minimum threshold of $100 (in 2010 dollars) in the maximum UI benefit change. This results in 61 changes across 26 states over the sample period. I complement this dataset with basic macroeconomic variables, measured at the state-level, from the US Bureau of Economic Analysis.

[3] http://workforcesecurity.doleta.gov/unemploy/statelaws.asp

The data and is then merged with Compustat annual data, using each firm's historical headquarter. As aforementioned, the time frame covers the years 1991 to 2007. 1991 is the first year of my sample due to the fact that I use the *historical* headquarter of firms in my analysis. In the Compustat database, the headquarter is backfilled, and by not accounting for this, I may end up coding as treated firms that should belong to the control group, and vice versa (see Heider and Ljunqvist (2015)). However, I can access historical information on a firm's headquarter starting from 1991 onwards from the "Compustat Historical Database."

The last year of the dataset is 2007 to avoid the years of the Great Recession, which involved a series of temporary UI extensions (Di Maggio and Kermani (2015)). While most of a firm's workers are likely to operate in the state where the firm is headquartered, it is plausible that some of its workforce will be located in different states, especially in large firms. To attenuate measurement error, in most of the analysis I exclude firms operating in "dispersed" industries, as identified by Agrawal and Matsa (2013) (wholesale, trade, retail), that is, firms likely to have a geographically dispersed workforce.[4]

I also exclude from the sample financial and utility related firms because accounting variables are not directly comparable between them and firms in other industries. Moreover, since all the control variables are expressed as ratios with total assets as denominator, the inclusion of very small firms in the sample tends to add noise to the estimates. Therefore, following the example of Baker, Stein, and Wurgler (2003), I exclude the smallest companies in the sample. I do so by computing the mean asset value over the full sample period and dropping the firms in the bottom decile of the distribution.

[4] In Table 3 I explicitly take into account this problem by using plant level data. See Section 4.1 for details.

3.2 Descriptive Statistics

Descriptive statistics for the outcome variables and the main control variables in the full sample are in Table 1. As further explained below, it will be convenient to estimate a model in first differences in order to absorb unobserved heterogeneity across firms. To account for this, I report descriptive statistics on both levels and first differences of each variable. The main dependent variable is the change in dividend payout, measured as the common dividend over net income, with all multiplied by 100 for ease of interpretation. This ratio is meaningful only for positive values of earnings, ergo, in each year I keep only firms with positive earnings in year t and $t+1$, as well as non-missing control variables and dividend payments.

The full sample includes 22,595 firm-year observations and 3,738 unique firms. The 61 UI increases affect 1,273 firm-years (931 unique firms). These 61 "shocks" are associated with an average increase in the maximum benefit of $1,008. In terms of the replacement rate, defined as the maxium benefit divided by the state income per capita, the average increase is 3.37%. These are meaningful figures compared to the sample means of $14,503 and 40.2%, respectively. The average dividend payout is 15.58%, and its average growth is 0.32%. More details regarding construction and sources of the other variables are in the following sections and in Appendix A.1.

3.3 Empirical Strategy

I estimate a model of corporate investment in first differences, similar to Heider and Ljunqvist (2015)). The advantage of this empirical framework is that it allows control for unobserved heterogeneity. Furthermore, it can easily accommodate multiple shocks occurring in the same state over time. The baseline empirical specification is:

$$\Delta DIV_{i,t+1} = \beta UI_{s,t} + \delta' \Delta X_{i,t} + \theta' \Delta Z_{s,t} + \vartheta_s + \delta_t + \varepsilon_{i,t+1} \tag{1}$$

where the i, s, j and t subscripts correspond to each firm, state, and year, respectively, and Δ is the first-difference operator.

The dependent variable is the change in the dividend payout, defined as common dividends divided by net income, with all multiplied by 100. UI is a dummy equal to 1 if one of the 61 events identified according to the procedure described in Section 3.1 occurs in state s and year t. In Section 4.2 and in most of the paper, however, I will restrict the analysis to 12 events plausibly unrelated to macroeconomic conditions and on a matched sample of firms. $\Delta Z_{s,t-1}$ is a vector of state-level control variables. Because unemployment benefits are likely to grow in response to an increase in the number of unemployed workers and will be correlated with workers' income, I include the two arguably most relevant controls following Agrawal and Matsa (2013): per capita GDP growth and unemployment rate growth.[5] Beyond that, I also include state-level dummies, which absorb time-invariant state-level characteristics and year dummies.

$\Delta X_{i,t-1}$ is a vector of firm-level controls, also expressed in first differences. It includes Q, ROA, size, and debt to asset ratios, along with industry dummies (defined using the 2-digit SIC classification). Q is defined as the total assets plus the market value of the firm (number of shares outstanding $\times$ fiscal year-end price) minus common value of equity, all divided by total assets. This captures a firm's investment opportunities. Cash flow is net income over lagged total assets. It can correlate with dividend payout either because of financial constraints or because it is associated with investment opportunities not adequately captured by Q. Debt to assets (long-term plus short-term liabilities, all divided by total assets) is included as an additional control variable and as proxy

[5] In unreported tests, I include additional changes in a number of potential determinants for UI policies, such as the state balance deficit, the state-level percentage of unionized workers, and the political affiliation of the governor. Results are unaffected by the inclusion of these additional variables.

for a firm's financial soundness. Finally, I include size, defined as Log(total assets), because large, mature firms are on average more likely to issue dividends (Jagannathan et al. (2000)). Following the recommendations of Bertrand, Duflo, and Mullainathan (2004), standard errors are clustered at the state-level in all the regressions.

4 Unemployment Insurance and Dividend Payout Policies

4.1 Preliminary Evidence

Estimates of equation 1 are reported in Table 2, which shows five different specifications, ranging from the least to the most conservative. In specification 1, I include only the UI dummy and year and state dummies. The relevant coefficient is significant at the 5% level and equal to 3.60. Column 2 adds state controls (the change in the logarithm of per capita income and unemployment rate), with the UI coefficient being essentially unaffected. Column 3 includes industry dummies, and column 4 firm-level controls.

The coefficients on the control variables have the expected sign: larger and more profitable firms are more likely to issue dividends, while riskier firms (in terms of change in debt to assets), or firms with stronger future prospects (higher ΔQ) are less likely to do so. Firm controls are endogenous, so they may bias the estimates, but it is reassuring that both the point estimate and the statistical significance of the UI coefficient are unaffected.[6] To interpret the economic magnitude of this effect, consider that the standard deviation of the dividend payout ratio in the full sample is 23.57, and the standard deviation of ΔDiv is 41.01. Therefore, a coefficient of 4.214 (last specification) corresponds to an increase in the dividend payout ratio equal to roughly 10% of a standard deviation of the

[6] For brevity, in all the tests that follow I will include all the control variables, but all the results are very similar if control variables are excluded from the regressions.

dependent variable.

Panels A, B and C of Figure 1 plot the average maximum benefit, replacement rate, and dividend payout, respectively, for treated and control firms in the five years surrounding a UI increase. There is no evidence of differences in trends before year *t+1* although the levels of the three variables are much lower for the eventually treated firms. The gap in UI generosity narrows in year *t+1*, and dividend payout for treated firms jumps by about three and a half percentage points, so much that its mean level is higher than that of control firms. In year *t+1*, there is a slight reversion and the two lines intersect each other.

Table 3 presents additional variations over the baseline specification of equation 1 (coefficients on the control variables are omitted for brevity). Column 1 presents the baseline model. Column 2 includes dispersed industries (resale, wholesale and trade). Here the coefficient drops slightly in magnitude (2.59), as expected, but remains marginally significant (p-value¡0.10). Column 3 includes only firms in states that are eventually treated, as in Bertrand and Mullainathan (2003), with little effect on the baseline point estimate.[7] Column 4 includes year-industry dummies, and column 5 adds year-industry-census region dummies. The results are robust, with the point estimates of the UI coefficient being again unchanged relative to the baseline specification of column 1.

In column 6, I replace UI with the fraction of each firm's workers treated by the UI benefit increase. I obtain data on establishment location and number of employees from Dun and Bradstreet NETS, and manually match firms in Compustat with the D&B data using their names.[8] Unfortunately, such data is likely to be quite noisy.[9] Moreover, I was not able to match all the firms and the sample size drop by approximately 20%. Still, results to this additional test are quite robust. In

[7] With only 26 clusters, standard errors are likely to be biased. However, clustering by firm produces very similar standard errors.

[8] More precisely, I use a fuzzy matching algorithm to link company names of the two datasets (using the Stata command *reclink* developed by Michael Blasnik) and verify manually the accuracy of each match.

[9] The correlation between the total number of employees reported in Compustat and NETS is 46.91%, quite large, yet still far from one.

particular, they suggest that a firm that has 100% of its workers subject to a UI generosity increase will increase its dividend payout by 4.64%.

4.2 A "Narrative Approach"

Although the results found in the previous section appear robust to different specifications and choices of control variables, unobserved heterogeneity in underlying economic conditions may cast doubt on a causal interpretation. If policymakers adjust UI policies in order to respond to varying macroeconomic conditions or to meet their expectations of future events, the establishment of an association between corporate decisions and UI policies may be driven simply by underlying changes in the macroeconomic environment that affect both firms' and policymakers' actions.

Fortunately, a number of factors unrelated to the economic environment have been suggested to affect the variation of UI policies across time and states (Blaustein et al. (1993)). Subsequently, it is possible, in practice, to identify instances where economic considerations were likely to play little or no role in shaping the timing of UI benefits revisions.

In this Section I provide more robust evidence by adopting a "narrative approach" in the spirit of Romer and Romer (2010). I perform a search of news articles through several sources (Factiva, Lexis Nexis, and search engines), and I collect information regarding the political and economic environment at the root of these bills together with any concurrent significant reforms simultaneously implemented.

This kind of strategy, popular in the Labor Economics and Macroeconomics literature, is becoming common also in recent Corporate Finance work. For example, Giroud and Rauh (2015) analyze changes in tax policies across US states and Simintzi et al. (2015) study major labor reforms in a sample of developed countries. I am deliberately conservative in my search, and leave legislations without sufficient information out of my sample. I was able to obtain adequately detailed information for 18 out of 61 cases. Not surprisingly, they involve relative large states, which have more extensive media coverage. Therefore, they cover about half of the treated firm-years (639 out of 1,273).

I screen the UI increases according to two additional filters. First, I exclude every legislative change whose timing is clearly related to changing economic conditions. Second, I exclude those which coincide with confounding reforms, particularly where tax cuts or raises are concerned. It turns out that this second filter is the most important one of the two, with macro conditions playing little role in driving the policy changes. These filters leave me with 12 out 18 shocks (and 505 treated firm-years). The most common reasons for these UI increases are federal funds inflows, upcoming elections, or isolated economic events, such as layoffs at a single large plant.

Admittedly, this procedure requires some degree of discretion. For this reason, I briefly describe the political environment surrounding each of the 18 changes in Appendix A.2. A review of these legislative changes shows that the 12 UI increases I am left with suggest that they provide a reasonably adequate setting to test for a causal link between unemployment risk and financial policies.[10] None of them is linked to changes in corporate or UI tax rates, or to reforms one would expect to substantially affect corporate behavior. The underlying causes vary, but they can hardly be linked to dramatic developments in the corporate or macroeconomic environment and can be roughly classified in four categories. Three legislative changes were motivated by layoffs or strikes in plants belonging to a single company (Missouri 1997, Virginia 2000, New Hampshire 2002).[11] In three other cases, the main driver was likely the upcoming gubernatorial election (New York 1998, California 2001, Georgia 2002). The third category includes UI increases resulting from

[10] State and year of each event are: FL (1992), MO (1997), NY (1998), VA (2000), CA (2001) TN (2001), GA (2002), MD (2002), MI (2002), NH (2002), AZ (2004), and AL (2006).

[11] These companies are McDonnell Douglas, Tultex and Fraser Papers, respectively. Fraser Papers was headquartered in Canada, so it is not included in the sample. Excluding the remaining two firms does not change the results. One concern could be that the difficulties these companies were going through were related to broader industry level demand shocks. However, the matching procedure in practice controls for time-varying industry trends.

lengthy negotiations between business lobbies and unions, which produced substantial uncertainty regarding the final outcome (Florida 1992, Tennessee 2001, Michigan 2002, Arizona 2004). The final group includes policy changes motivated by events unrelated to the local state economy, such as the expiration of federal funds (Maryland 2002) and political pressure to "match" other states UI policies (Alabama 2006).

4.3 Matching Procedure

Having identified these reforms, I adopt a standard matching procedure where each firm in a treated state is matched with a firm similar in terms of several covariates and operating in the same industry and geographic region. To the extent that treated firms share the same growth opportunities and operate in relatively homogeneous labor markets relative to firms operating in the same industry and geographic region, this test should absorb much of the unobserved heterogeneity across firms. This will force control and treated firms to share similar firm-level covariates, further increasing the stringency of the test.

I use a logistic regression to estimate the probability of being a treated firm. Propensity scores are estimated using the levels of the control variables at *t-1*. Each treatment firm is matched to a control firm, matching on year, industry, census region, and closest propensity score (with a maximum difference between propensity scores of 0.01). Ideally, one would like to get as close as possible to a randomized experiment, where firms similar in terms of observable characteristics, same industry shocks, and relatively homogeneous labor market conditions are subject to different UI policies.

The matching is performed with replacement, resulting in a lower number of control firms relative to those that are treated. The final sample includes 782 firms: 469 treated and 313 control firms.

As Table 4 shows, the two groups are well-matched in terms of covariates.[12] Interestingly, the levels of the unemployment rate and Log(income) of the states of treated and control firms also do not differ significantly.

The second Panel of Table 4 tests whether treated firms and control firms differ in terms of the trends of covariates by comparing their rate of growth between year *t-1* and *t*. The parallel trend assumption for any of the control variables cannot be dismissed, as well as the state-level controls. Moreover, all the differences are economically small. In Appendix A.3, I test whether proxies for analysts' expectations differ in their trends or levels between treated and control groups. Following Fresard and Valta (2003) I focus on sell/buy recommendations, earnings per share, and long term growth forecasts. Again, there is no statistically significant difference between the two sets of firms in any of the three measures.

The UI reforms announced in year t are associated with sharp and large changes in UI generosity, as expressed in Panels A and B of Figure 2. Visual evidence shows that treated firms are headquartered in states with lower replacement rates and maximum weekly benefits relative to control firms, but this gap narrows quite sharply afterwards.

In unreported results, using data from the Bureau of Economic Analysis, I find that total UI payment increases by 18% in treated states, relative to control states. This suggests that changes in maximum benefits are strong predictors of actual UI payments. Panel C shows the evolution of the dividend payout of treated and control companies in the five years surrounding the UI generosity increases by plotting the mean payout of each subgroup. There is little evidence of anticipation. If

[12] In unreported tests, I also find that trends in other potential confounding elements, such as the fact that top corporate income taxes or UI taxes do not differ between treated and control firms. I obtain corporate taxes from Heider and Ljunqvist (2015). For the second variable, I follow Di Maggio and Kermani (2015) and compute a simple approximation of the firm tax schedule as the maximum minus the minimum tax rate in each state. Di Maggio and Kermani (2015) find that this measure is strongly correlated with an industry-weighted average of Card and Levine (1994)'s measure of mean marginal tax costs in 1979-1987, constructed using confidential data. Unfortunately, I am able to construct this proxy only for the latter part of the sample, starting from 2003 onwards, and not for all the states.

anything, control firms have a higher payout in year *t-2*.

Interestingly, the levels of payout across groups are also similar, suggesting that the matching procedure does quite well in controlling for unobserved heterogeneity across treated and control firms. Payout jumps by about six percentage points in year *t+1* in treated firms relative to control firms. Similarly to the evidence of Figure 1, this gap is reduced in year *t+2*, up to about 2%, which suggests that the effect of more generous UI provisions is large but relatively short-lived. However, the matching procedure requires dividend payout to be defined (and net earnings to be correspondingly positive) only in years *t* and *t+1*, so attrition concerns may invalidate strong inferences regarding the behavior of firms far from the event year.[13]

4.4 Evidence from the Matched Sample

Table 5 replicates the baseline tests of Table 2. In this restricted sample, the results are very similar and, if anything, slightly stronger. Column 1 includes only the UI dummy; column 2 adds industry fixed-effects; column 3 adds state controls, and column 4 firm-level controls. The inclusion of a different set of control variables does not affect the point estimate of the coefficient of interest. The estimates suggest that treated firms increase dividend payout by 5.8 to 6 percentage points.

This effect is larger than what is found in Section 4. One possible reason for this is that some of the UI increases included in the full sample and excluded here are adopted in anticipation of poor economic conditions. This may not have been adequately captured by the control variables. If this is the case, firms may lower dividend payout in order to preserve financial liquidity.

Because the 12 different events correspond to UI revisions different in amount, the tests thus far described do not allow an easy interpretation of the quantitative effects of UI on payout policies. To do so, I project changes in UI benefits on the UI dummy, and then regress changes in dividend

[13] In particular, while the full sample employed in the regression includes 782 observations, there are only 654 observations in year *t-2* and 651 in year *t+2*.

payout on the predicted value from this first stage regression in a simple IV framework.

More formally, I first estimate:

$$\Delta Benefits_{s,t+1} = \beta UI_{s,t} + \delta' \Delta X_{i,t} + \theta' \Delta Z_{s,t} + \delta_t + \varepsilon_{i,t+1} \tag{2}$$

where $\Delta Benefits_{i,t+1}$ is the change in the replacement rate or in the maximum benefit occurring between year t and $t+1$ in state s. Then I use the predicted value from this regression $\Delta \widehat{Benefits}_{i,t+1}$ to estimate the following model:

$$\Delta DIV_{i,t+1} = \beta \Delta \widehat{Benefits}_{i,t+1} + \delta' \Delta X_{i,t} + \theta' \Delta Z_{s,t} + \delta_t + \varepsilon_{i,t+1} \tag{3}$$

The first two columns of Table 6 reflect the simple OLS regression of growth in dividend payout on change in benefits (either the change in the maximum amount, in thousands of dollars, or the change in the replacement rate), plus the usual controls. Columns 3 and 4 report the estimates of equation 2, with the dependent variable being the growth in maximum benefits or the replacement rate, respectively.

In both cases the t-statistics are well over 5, suggesting that the instrument is quite strong. Columns 5 and 6 estimate the IV regression of equation 3. The estimates from the OLS and the IV models are quite similar, but tend to be larger in the latter case and suggest that a rise in UI benefits of $1,000, or in the replacement rate of 1% causes an increase in dividend payout of 4.75 basis points, or 1.86%, respectively.

4.5 Additional Payout Measures and Other Outcomes

Table 7 includes results obtained using different measures of payout and additional firm-level outcomes. Another common measure of payout is total payout, defined as the sum of dividend payment and repurchases scaled by earnings (see for example He et al. (2016)). The results, reported in column 1, are qualitatively similar when using this alternative definition of the dependent variable. These suggest that an increase in UI generosity causes an increase in total payout of 20% relative to a standard deviation of 145% in the full sample. Column 3 shows that repurchases (again, scaled by earnings) do not react to changes in UI in a similar fashion. This is consistent with repurchases being driven primarily by considerations other than risk, such as temporary mispricing.

One possible concern could be that these results are driven by changes in earnings (the denominator of the dividend payout ratio) rather than actual changes in earnings. Appendix A.3 has robustness checks that are inconsistent with this alternative explanation. I redefine the dependent variable as the difference between dividend between periods *t+1* and *t* scaled by earnings in period *t*.[14] Alternatively, one can scale dividend payments by asset or use the change in dividend per share as the dependent variable.

Table A3 in the Appendix shows that using either of these three measures as dependent variable delivers qualitatively similar results. In the same table, I also analyze to which extent UI increases affect the intensive versus the extensive margin, by splitting the sample according to whether the firms in the sample have paid any dividends at time *t*. If such results were driven by a fall in demand, and so in earnings, we would not expect firms which are not paying dividends in year *t* to start doing so in the following year. Table A3, however, shows that the coefficients on the UI dummy is significant in both subgroups, although with different magnitudes.

Given this evidence, it is natural to ask whether the rise in dividend is associated with a drop in cash holdings. In column 3, I show that cash (defined as cash and short-term securities scaled by assets) does indeed drop by 0.007 (with the standard deviation equal to 0.082), although this

[14] This approach has the additional advantage of avoiding potential forward-looking bias due to the requirement that firms have positive earnings in the year that follows the treatment, resulting in the sample size increasing to 983 observations. An alternative way to address this concern consists in adding the contemporaneous change in earnings as regressor. Including $ROA_{t+1} - ROA_t$ as additional control variable does not affect the results.

change is not statistically significant. In column 4 I use a more refined measure of cash holding: "Z-cash" (as in Fresard (2010)), wherein the industry mean is subtracted from the cash holding and the difference is standardized by the industry standard deviation. This accounts for differences in cash needs across industries (due to either technological factors or different degrees of market competition) in a more direct way. The coefficient is now statistically significant at the 5%, and equal to -0.069, relative to a standard deviation of 0.557.

I also investigate whether higher payout is driven by crowding out of investment due to low growth opportunities. In columns 5 and 6, where the dependent variables are the change in investment (defined as capital expenditure scaled by lagged total assets) and growth in employment, respectively, there is no evidence that this is the case. Both coefficients are small and insignificant.

Ellul et al. (2014) show that, in countries where the UI system is more generous, firm employment is more sensitive to industry shocks. I follow them in regressing employment growth on industry revenue growth[15] and interact this measure with the UI dummy. Alternatively, I use the median Q in the industry as a proxy for industry growth opportunities. Intuitively, we expect both interactions of the coefficients to be positive. As UI becomes more generous, firms should have fewer incentives to smooth fluctuations in employment, and so will be more likely to exploit potential profit opportunities.[16]

In columns 7 and 8 of Table 7, both measures are de-meaned and divided by their standard deviation for ease of interpretation. As expected, both coefficients are positive and relatively large, although the coefficient for the interaction between industry sale growth and the UI dummy is not significant. The coefficients on the non-interacted industry proxies are omitted for brevity. The coefficient for the interaction term between median industry Q and the UI dummy is, on the

[15] Following Sraer and Thesmar (2007), I compute the mean rate of growth for all industry in a given industry and year rather than computing the growth in total sales to account for attrition among listed firms.

[16] Each proxy is also included in the regressions as standalone variable (not shown).

other hand, large and significant. This suggests that firms in industries with a median Q one standard deviation above the mean increase employment by 4.8% if headquartered in states with a UI increase.

In Appendix A.3, I also test whether UI generosity is associated with an increase in leverage.[17] Consistent with Agrawal and Matsa (2013), who find that the positive association between leverage and UI generosity is driven primarily by firms with low cash flow, I find no evidence of a change in leverage for treated firms in my sample, all of which are required to have positive earnings (column 1 of Table A4). When I repeat the matching procedure outlined in Section 4.3 on firms with negative earnings and re-estimate equation 1 in this sample of firms, I find that firms in the treatment group increase book leverage by 2.2% (relative to a standard deviation of 25.25% in the dependent variable). This effect is only marginally statistically significant (t-statistic=1.73). I also estimate the same model using net leverage as a dependent variable, which subtracts cash holding from the numerator. This measure is of additional interest, given the evidence on cash holding and total payout presented in Table 7. I find that net leverage drops by 3.1% (t-statistic=2.42), which is about a tenth of a standard deviation of the dependent variable (equal to 29.9%) in the negative earnings subsample but, again, I find no effet among firms with positive earnings.

5 Cross-Sectional Heterogeneity

5.1 Volatility in Real Activities

This section contains a number of cross-sectional tests giving empirical support to the hypothesis that conservative payout policies are related to the desire of managers to provide insurance to workers in presence of unemployment risk. To be conservative, I focus on the matched sample, selected

[17] The construction of book leverage follows Baker and Wurgler (2002); see details in Table A1 in Appendix A.1.

according to the procedure outlined in Section 4.3. Results are largely similar when extending the analysis to the full sample considered in Section 4.1.

First, I hypothesize that the effect of UI generosity will be weaker in industries where firms expect to enjoy relatively stable earnings streams. Committing to a high dividend policy is unlikely to reduce operating flexibility in such firms because severe negative shocks are unlikely to occur (Jagannathan et al. (2000)). Following Brealey, Hodges, and Capron (1976) and other authors, including more recent work by Matsa (2010), I construct a simple measure of earnings variability by taking the standard deviation of the change in ROA for each firm in Compustat computed over the full sample period and requiring at least five observations per firm. I then average this measure across firms belonging to the same industry. The theoretical justification for using this measure is related to work by Baily (1974), who argues that firms may have incentives to stabilize workers' employment, primarily when facing frequent spells of high or low demand, also reflected in earnings.

I also construct more direct measures, aimed at proxying for employment volatility, its input being the change in Log(employment), to capture the actual unemployment risk faced by workers. The two measures exhibit a strong positive association (the correlation coefficient is 64.6%) but are related to different aspects of industry-level risk.

I then replicate the main analysis of Section 4.4 in four distinct subgroups by sorting firms according to whether the industry or the employment volatility proxies are above or below the sample median and compare the coefficients of interest. Table 8 shows evidence consistent with the expected pattern. In particular, the coefficient on the UI dummy more than doubles when moving from low to high volatility industries and is significant only in the latter two subgroups.

The last two rows of the table report χ^2-statistics computed under the null hypothesis that the two coefficients are equal, along with their p-values. Even though such tests do not reject the null hypothesis of equality at conventional significance levels, the differences are economically meaningful.

5.2 Growth Prospects

Firms lacking encouraging growth prospects are likely to respond strongly to changes in unemployment costs. They may have high incentives to issue dividend from the start: companies that are performing well are unlikely to reinvest the proceeds of previous investments if their growth opportunities are scarce. Moreover, free cash flow concerns may be relevant, as managers may be tempted to use their internal funds to finance "pet projects" or diversify their personal risk through investments unrelated to their core business activity (Gormley and Matsa (2016)). More importantly, in these firms workers face higher unemployment risk because of their poor performance.

In the analysis that follows, I employ three proxies for growth opportunities. The first is simply a firm's Tobin's Q, which is likely to be most appropriate because it incorporates investors' expectations about future investment opportunities. I also employ two backward looking measures based on earnings: the current ROA and the current change in ROA (that is, $ROA_t - ROA_{t-1}$). Importantly, all three measures are strong predictors of future employment growth.[18]

For each year I sort firms according to whether each proxy for growth opportunities is above or below the sample median. Results of this exercise are in Table 9. The coefficient on the UI dummy is about three times larger when moving from the low versus high Q firms (3.15 versus 10.08), and is five times larger when moving from the low versus high ROA firms (2.096 versus 10.74). The UI coefficient is, instead, slightly negative in the high ROA growth group, relative to 15.54 in the low ROA growth group. These differences are statistically significant at conventional levels, except when firms are sorted according to Q (p-value of the χ^2-statistic=0.061).

[18] I test this hypothesis by regressing the change in Log(employment) between *t+1* and *t* on each of the three measures separately, and on firm and year dummies. A decline of a standard deviation in Q, ROA and ΔROA is associated with a fall in employment growth equal to 4.16%, 2.83% and 0.87%. These coefficients are highly statistically significant.

5.3 Evidence on the Labor Channel

A more direct test of the labor insurance channel consists in examining the response of firms that differ in terms of workers' importance to their production function. I expect firms that use labor more heavily to be more affected by changes in UI generosity. I follow Serfling (2016) and use the number of employees, scaled by either assets or property, plants, and equipment (PP&E) as proxies for labor intensity. Table 10 shows that, once sorting by either measure, the coefficient on the UI dummy is significant only in the high labor intensity firms, and almost triples in magnitude relative to the low labor intensity firms.

The effect of insurance policies on corporate decisions is likely to be shaped by the underlying structure of the labor market and, in particular, by the probability of being quickly re-employed. Indeed, as Anderson and Meyer (1997) document, many unemployed workers do not take any UI benefits because they are likely to find a new job in a relatively short period of time.

Intuitively, this should be true especially in industries where labor demand is falling. I proxy for trends in labor demand by measuring the percentage rate of change in employment in each industry. In addition, because the welfare gain from being re-employed after a layoff depends also on how attractive a job is in terms of wage offers, I also analyze the effect of UI generosity on payout in industries with different average wage growth. I construct measures of employment and wage growth at the industry level (at the 4-digit NAICS level), using data from the Bureau of Labor Statistics.

Table 11 analyzes the response of payout policies in different industries, sorting as usual in two groups according to either characteristic (employment growth or wage growth). The coefficient on the UI dummy is significant only in low employment and wage growth industries (coefficients are 10.06 and 12.82, respectively), whereas UI generosity is unrelated to payout policies in better performing industries. The differences in the coefficients across subgroups are not only economically large but also statistically significant, as shown in the last two rows of Table 11.

5.4 Alternative Interpretations

Although results so far are consistent with an implicit contract interpretation, different conjectures are possible. Feldstein (1976) suggests that UI acts as an implicit subsidy to firms with large seasonal shifts in demand. Such firms can temporarily lay off workers during low demand spells and re-hire them in periods of high demand. A rise in UI payments improves workers' well-being during periods of temporary unemployment, enabling employers to pay lower wages when workers are re-hired.

In Table 12, columns 1 and 2, I sort firms according to their degree of sales seasonality. I measure seasonality as the standard deviation of the Log(revenues) changes in the previous four quarters. If anything, low seasonality firms seem to respond with greater intensity to higher UI, although this effect is not very precisely estimated.

A second possibility is that higher UI increases workers' bargaining power by raising their reservation wages. Managers may be willing to reduce a firm's liquidity in order to gain a stronger bargaining position and avoid wage concessions, as in DeAngelo and DeAngelo (1991). Although it is not obvious how to rule out this possibility, it is plausible that unionized workers, thanks to their superior organization, may be more likely to exploit this advantage in the bargaining process. As Agrawal and Matsa (2013) note, employed workers would not be eligible for UI payments if they refused to work when denied a wage raise, but UI provisions may still affect wage negotiations for unionized workers. In columns 3 and 4 of Table 12 I sort firms according to the degree of unionization of the state they are headquartered in. Again, the two point estimates are very similar.

6 Conclusion

In this paper, I have argued that firms engage in conservative payout policies partly to protect workers from the unemployment risk that derives from concerns about a firm's operating flexibility. Consistent with this hypothesis, firms increase payout following increases in UI generosity, which provide meaningful shocks to human costs from unemployment. This evidence on payout policies complements previous work on leverage, earnings management, and sensitivity of employment to industry shocks. Cross-sectional tests support this conjecture by focusing on heterogeneity in firms across several dimensions, such as labor intensity, volatility in real activities, growth prospects, and labor opportunities for unemployed workers. Furthermore, alternative, plausible hypotheses do not find support in the data.

I suggest two possible extensions to this work. It may be worthwhile to test whether these results hold worldwide. More generally, it would be interesting to ask how much of the cross-country heterogeneity in financial policies (cash holding, leverage, payout, etc.) is explained by unemployment regulations, which would complement recent work that uses cross-country data (Simintzi et al. (2015) and Ellul et al. (2014) among others).

Second, this paper has hypothesized that labor market considerations affect the *cost* of issuing dividends. In principle, they could also shape the *benefits* deriving from such choices. For example, managers may screen for risk-loving workers by adopting riskier financial policies. This and other hypotheses would require more refined matched employee-employer data to be tested.

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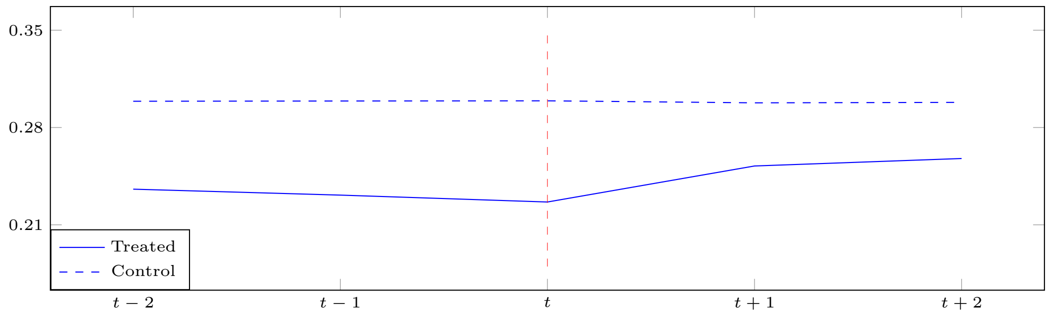
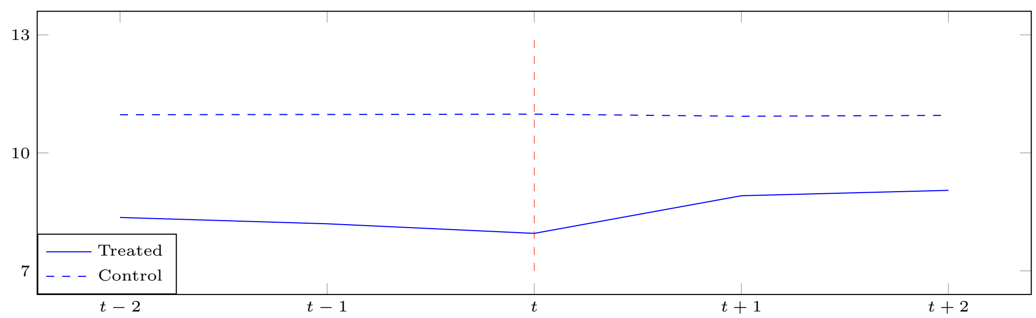
7 Figures and Tables

Figure 1
UI Generosity and Payout around Policy Changes: Full Sample
Panels A, B and C of Figure 1 plot the yearly means of maximum benefits, replacement rate and dividend payout, respectively, for treated (solid line) and control (dotted line) firms in the five years surrounding a UI increase (see Section 3.1 for details.)

Panel A. Replacement Rate



Panel B. Maximum Benefits ($1000s)



Panel C. Dividend Payout

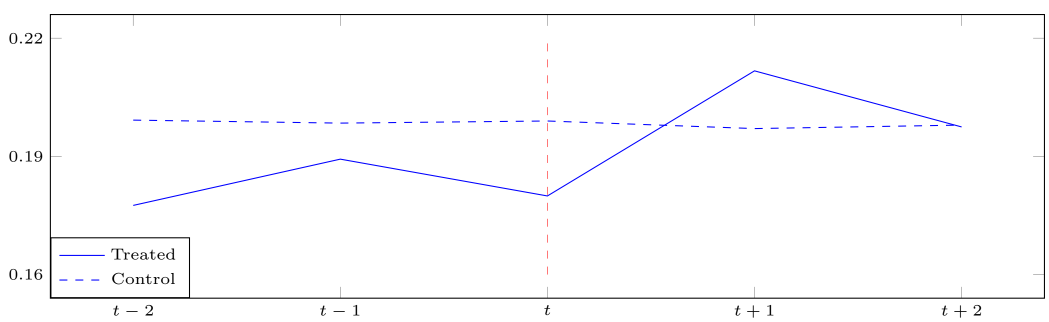
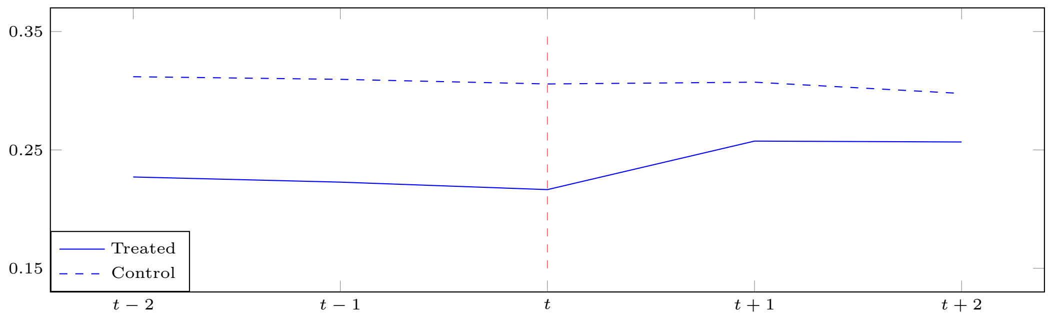


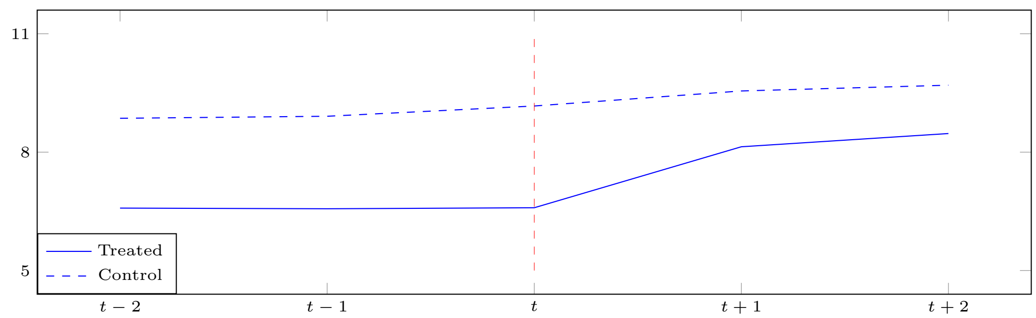
Figure 2
UI Generosity and Payout around Policy Changes:
Matched Sample

Panels A, B and C of Figure 2 plot the yearly means of maximum benefits, replacement rate and dividend payout, respectively, for treated (solid line) and control (dotted line) firms in the five years surrounding a UI increase (see Section 3.1 for details). The sample includes only firms selected according to the procedure outlined in Section 4.3.

Panel A. Replacement Rate



Panel B. Maximum Benefits ($1000s)



Panel C. Dividend Payout

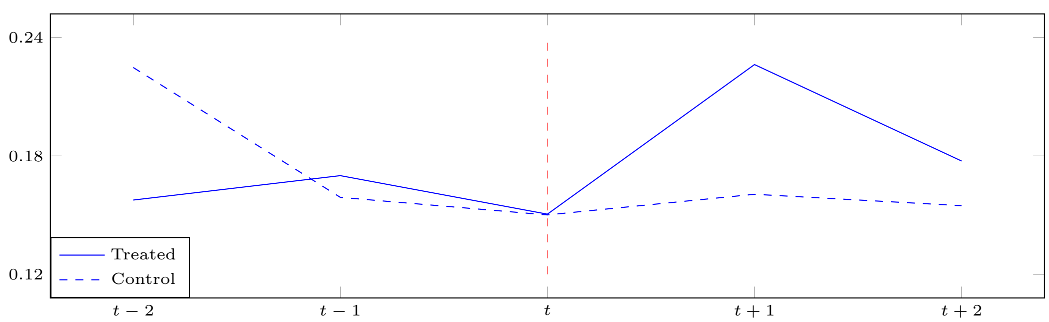


Table 1
Summary Statistics

Table 1 presents the variables used in the firm-level regressions. Dividend payout is defined as common dividend over net income. Total payout is defined as commond dividend plus repurchases, all divided by net income. Investment is defined as capital expenditures divided by lagged total assets. Debt to assets is defined as long-term debt plus debt in current liabilities, all divided by total assets. Q is defined as total assets plus market value of the firm (number of shares outstanding $\times$ fiscal year-end price) minus common value of equity, all divided by total assets. ROA is earnings before interest, taxes, depreciation and amortization divided by lagged total assets. UI is a dummy equal to 1 if the firm's headquarter is in a state that experienced a UI benefit increase of at least $100 (see Section 3.1 for details). Log(Income) is the logarithm of the per capita income at the state level. Unemployment is the state level unemployment. Max benefits is defined as maximum number of weeks of unemployment coverage times maximum benefit. Δ is the first-difference operator.

	Obs.	Mean	Median	St. Dev.	1^{st} P.	99^{th} P.
Dividend Payout	22,595	15.582	0	23.548	0	79.944
Q	22,595	2.115	1.639	1.762	0.715	8.463
Debt to Assets	22,595	0.198	0.172	0.184	0	0.722
ROA	22,595	0.097	0.076	0.105	0.002	0.430
Log(Assets)	22,595	5.830	5.702	1.867	2.361	10.504
Investment	22,595	7.384	4.947	8.002	0.201	48.620
Log(Employees)	22,286	0.614	0.543	1.804	-3.612	4.850
Total Payout	21,287	0.549	0.211	0.995	0	6.745
UI	22,595	0.056	0.000	0.231	0	1
Max Benefits	22,595	14,503	14,303	1,755	11,595	17,531
Max Benefits / Income	22,595	0.402	0.397	0.050	0.296	0.523
ΔDividend Payout	22,595	0.322	0	41.008	-191.787	201.881
ΔQ	22,595	0.025	0.025	0.790	-2.837	2.726
ΔDebt to Assets	22,595	-0.008	-0.004	0.079	-0.250	0.280
ΔCash Flow	22,595	0.022	0.007	0.089	-0.182	0.412
Δ Log(Assets)	22,595	0.149	0.098	0.219	-0.256	1.085
ΔInvestment	22,595	0.031	0.012	5.212	-21.307	19.940
ΔLog(Employees)	22,197	8.373	5.009	20.059	-44.274	90.441
ΔTotal Payout	20,832	6.986	0	132.812	-574.353	710.092

Table 2
UI and Payout: Baseline Results

Table 2 reports regressions of changes in dividend payout on a UI dummy and lagged changes in Q, Log(assets), debt to assets, ROA, Log(income) and unemployment rate. Please refer to the Appendix A.1 for a definition of the variables. Each regression includes state and year dummies and, when indicated, industry dummies (defined according to the two digits SIC classification). Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
UI	3.597**	3.582**	3.619**	3.544**	4.214**
	(1.477)	(1.490)	(1.494)	(1.522)	(1.883)
ΔLog(Income)		-0.155	-0.152	-0.147	-0.126
		(0.104)	(0.104)	(0.103)	(0.133)
ΔUnemployment		0.714	0.704	0.688	0.626
		(0.628)	(0.626)	(0.627)	(0.725)
ΔLog(Assets)				4.731***	6.982***
				(0.958)	(1.553)
ΔDebt to Assets				-18.39***	-21.88***
				(4.063)	(4.707)
ΔQ				-1.272***	-1.438***
				(0.360)	(0.367)
ΔCash Flow				13.36***	22.70***
				(2.856)	(4.164)
Observations	22,595	22,595	22,595	22,595	21,970
R-squared	0.006	0.006	0.007	0.010	0.069
Year FE	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES
State Controls	NO	YES	YES	YES	YES
Industry FE	NO	NO	YES	YES	YES
Firm Controls	NO	NO	NO	YES	YES
Firm FE	NO	NO	NO	NO	YES

Table 3
UI and Payout: Robustness Checks

Table 3 reports regressions of changes in dividend payout on a UI dummy and lagged changes in Q, Log(assets), debt to assets, ROA, Log(income) and unemployment rate. Please refer to the Appendix A.1 for a definition of the variables. Each regression includes state, year and industry dummies (defined according to the two digits SIC classification). Column 2 includes in the sample dispersed industries (wholesale, trade and retail). Column 3 includes only firms headquartered in states which are eventually subject to a UI increase during the sample (26 states). Column 4 includes industry-year region fixed effects. Column 5 includes industry-year-census region fixed effects. Column 6 replaces the UI dummy with the fraction of firm's workers in states covered by a UI increase. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1) Baseline	(2) With Disp. Industries	(3) Only Treated	(4) Year-Ind. FE	(5) Reg.-Ind. -Year FE	(6) Fraction Treated
UI	3.544**	2.590*	3.165**	3.650***	3.511***	4.643**
	(1.522)	(1.514)	(1.444)	(1.361)	(1.216)	(1.781)
Observations	22,595	27,336	13,143	22,553	22,200	18,092
R-squared	0.010	0.009	0.010	0.053	0.130	0.012
Year FE	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES
State Controls	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Firm Controls	YES	YES	YES	YES	YES	YES

Table 4
Treated VS Control Firms: Summary Statistics

Table 4 reports means of control variables of treated and control firms, matched according to the procedure outlined in Section 4.3. Variables definitions are in Appendix A.1. Δ is the first difference operator.

	Treated (Std. Error) Obs.=469	Control (Std. Error) Obs.=313	Treated - Control (Std. Error) Obs.=782
Log(Assets)	5.657	5.580	0.076
	(0.152)	(0.094)	(0.159)
Debt to Assets	0.203	0.211	-0.008
	(0.016)	(0.012)	(0.016)
ROA	0.075	0.072	0.002
	(0.011)	(0.005)	(0.011)
Q	2.217	2.085	0.131
	(0.130)	(0.081)	(0.149)
Log(Income)	10.32	10.29	0.032
	(0.051)	(0.033)	(0.055)
Unemployment Rate	5.334	4.819	0.514
	(0.418)	(0.209)	(0.488)
ΔLog(Assets)	0.124	0.135	-0.010
	(0.007)	(0.010)	(0.012)
ΔDebt to Assets	-0.012	-0.005	-0.006
	(0.005)	(0.007)	(0.008)
ΔROA	0.015	0.019	-0.003
	(0.010)	(0.005)	(0.011)
ΔQ	-0.108	-0.023	-0.084
	(0.043)	(0.050)	(0.069)
ΔLog(Income)	5.259	4.578	0.680
	(1.101)	(0.257)	(1.141)
ΔUnemployment Rate	-0.146	-0.121	-0.025
	(0.203)	(0.087)	(0.236)

Table 5
UI and Payout: Evidence from Matched Sample

Table 5 reports regressions of changes in dividend payout on a UI dummy and lagged changes in Q, Log(assets), debt to assets, ROA, Log(income) and unemployment rate. Each regression includes year and industry dummies (defined according to the two digits SIC classification). The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
UI	5.796**	6.025***	5.905***	6.004***
	(2.200)	(2.104)	(1.981)	(2.064)
ΔLog(Income)			-0.206	-0.270
			(0.542)	(0.557)
ΔUnemployment			2.761	3.023
			(3.189)	(3.201)
ΔLog(Assets)				1.190
				(6.152)
ΔDebt to Assets				14.919
				(17.961)
ΔQ				0.502
				(0.957)
ΔCash Flow				-14.634
				(10.377)
Observations	782	782	782	782
R-squared	0.005	0.071	0.071	0.074
Year FE	NO	YES	YES	YES
Industry FE	NO	YES	YES	YES
State Controls	NO	NO	YES	YES
Firm Controls	NO	NO	NO	YES

Table 6
Economic Magnitudes

Table 6 reports, in the first two columns, regressions of changes in dividend payout on changes in the maximum benefit (column 1) and in the replacement rate (column 2). Columns 3 and 4 report regressions of changes in maximum benefit (column 3) and in the replacement rate (column 4) on a UI dummy. Columns 5 and 6 report IV regressions of changes in dividend payout on changes in the maximum benefit (column 5) and in the replacement rate (column 6), where the instrument is the UI dummy. All regressions include, as control variables, lagged changes in Q, Log(assets), debt to assets, ROA, Log(income), unemployment rate, year and industry dummies (defined according to the two digits SIC classification). The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1) OLS	(2) OLS	(3) First Stage Δ Ben.	(4) First Stage Δ Ben. / Inc.	(5) IV	(6) IV
Δ Max Benefit	2.905**				4.745***	
	(1.310)				(1.665)	
Δ Max Benefit / Inc.		1.174**				1.855***
		(0.531)				(0.643)
UI			1.265***	3.237***		
			(0.194)	(0.463)		
Observations	782	782	782	782	782	782
R-squared	0.073	0.073	0.702	0.699	0.071	0.071
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
State Controls	YES	YES	YES	YES	YES	YES
Firm Controls	YES	YES	YES	YES	YES	YES

Table 7
Evidence on Additional Outcomes

Table 7 reports regressions of changes in several dependent variables on a UI dummy and lagged changes in Q, ROA, debt to assets, Log(assets), logarithm of state income per capita, unemployment rate, year and industry dummies (defined according to the two digits SIC classification). The model is estimated in first differences. The dependent variables are: total payout (dividend plus repurchases, all divided by net income) in column 1, repurchases (repurchases divided by net income) in column 2, cash (defined as cash over total assets) in column 3, Z-cash (defined as cash over total asset minus the industry mean and divided by the industry standard deviation) in column 4, investment (capital expenditures divided by lagged total assets) in column 5, Log(employees) in column 6. The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

Dependent Variable:	(1) Total Payout	(2) Repurch.	(3) Cash	(4) Z-Cash	(5) Invest.	(6) Empl.	(7) Empl.	(8) Empl.
UI	20.808**	0.074	-0.007	-0.069**	0.003	-0.012	-0.012	-0.011
	(9.183)	(0.055)	(0.006)	(0.033)	(0.002)	(0.011)	(0.011)	(0.011)
UI × Ind. Growth							0.018	
							(0.017)	
UI × Ind. Q								0.048***
								(0.012)
Observations	718	718	781	772	782	773	773	773
R-squared	0.105	0.118	0.022	0.063	0.196	0.180	0.182	0.193
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
State Contr.	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
Firm Contr.	YES	YES	YES	YES	YES	YES	YES	YES

Table 8
Heterogeneity in Industry Volatility

Table 8 reports regression of changes in dividend payout on a UI dummy and lagged changes in Q, ROA, debt to assets, Log(assets), the logarithm of state income per capita and unemployment rate. Each regression includes year and industry dummies (defined according to the two digits SIC classification). Firms are sorted according to whether their earnings volatility (columns 1 and 2) and amployment volatility (columns 5 and 6) are above or below the yearly median. Each measure is computed by calculating the standard deviation of the rate of change of each measure for each firm over the sample period and then averaging firm-level standard deviations at the industry level. The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1) High Earnings Volatility	(2) Low Earnings Volatility	(3) High Employment Volatility	(4) Low Employment Volatility
UI	9.360***	3.956	8.708***	3.866
	(1.873)	(2.957)	(2.912)	(3.133)
Observations	321	461	366	416
R-squared	0.063	0.097	0.087	0.103
State Controls	YES	YES	YES	YES
Firm Controls	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
χ^2-Stat (p-value)	3.32* (0.068)		1.22 (0.269)	

Table 9
Heterogeneity in Growth Prospects

Table 9 reports regressions of changes in dividend payout on a UI dummy and lagged changes in Q, ROA, debt to assets, Log(assets), the logarithm of state income per capita and unemployment rate. Each regression includes year and industry dummies (defined according to the two digits SIC classification). Firms are sorted according to whether their Q (columns 1 and 2), ROA (columns 3 and 4) and lagged change in ROA (columns 5 and 6) are above or below the yearly median. The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1) High Q	(2) Low Q	(3) High Profitability	(4) Low Profitability	(5) High Profitability Growth	(6) Low Profitability Growth
UI	3.149**	10.080**	2.096	10.740***	-0.807	12.539***
	(1.358)	(3.763)	(2.109)	(3.638)	(2.208)	(3.554)
Observations	389	393	389	393	389	393
R-squared	0.091	0.117	0.104	0.156	0.220	0.114
State Controls	YES	YES	YES	YES	YES	YES
Firm Controls	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
χ^2-Stat (p-value)	3.50* (0.0614)		5.18** (0.023)		14.64*** (0.000)	

Table 10
Heterogeneity in Labor Intensity

Table 10 reports regression of dividend payout on a UI dummy and lagged changes in Q, ROA, debt to assets, Log(assets), the logarithm of state income per capita and unemployment rate. Each regression includes year and industry dummies (defined according to the two digits SIC classification). Firms are sorted according to whether their labor intensity is above or below the yearly median. Labor intensity is measured in columns 1 and 2 as number of employees divided by property, plant and equipment; in columns 3 and 4 it is defined as number of employees divided by total assets. The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1) High Employees/Assets	(2) Low Employees/Assets	(3) High Employees/PP&E	(4) Low Employees/PP&E
UI	9.788***	3.362	10.464***	3.845
	(2.987)	(4.206)	(2.129)	(3.626)
Observations	392	390	393	389
R-squared	0.088	0.160	0.212	0.101
State Controls	YES	YES	YES	YES
Firm Controls	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
χ^2-Stat (p-value)	1.64 (0.200)		2.73* (0.098)	

Table 11

Heterogeneity in Labor Market Conditions

Table 11 reports regression of changes in dividend payout on a UI dummy and lagged changes in Q, ROA, debt to assets, Log(assets), the logarithm of state income per capita and unemployment rate. Each regression includes year and industry dummies (defined according to the two digits SIC classification). Firms are sorted according to whether they belong to industries whose employment growth (columns 1 and 2) or wage growth (columns 3 and 4) are above or below the yearly median. The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1) Low Employment Growth	(2) High Employment Growth	(3) Low Wage Growth	(4) High Wage Growth
UI	12.062***	-1.249	10.819***	0.658
	(2.328)	(3.606)	(2.575)	(2.691)
Observations	393	374	392	375
R-squared	0.102	0.148	0.165	0.094
State Controls	YES	YES	YES	YES
Firm Controls	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
χ^2-Stat (p-value)	12.44*** (0.000)		8.58*** (0.003)	

Table 12
Alternative Interpretations:
Unionization and Demand Seasonality

Table 12 reports regressions of changes in dividend payout on a UI dummy and lagged changes in Q, ROA, debt to assets, Log(assets), the logarithm of state income per capita and unemployment rate. Each regression includes year and industry dummies (defined according to the two digits SIC classification). Firms are sorted according to whether they belong to states whose average unionization level (columns 1 and 2) is above or below the yearly median, or whether their sales seasonality (measures as the standard deviation of the Log change in quarterly revenues in the previous year (columns 3 and 4) is above or below the yearly median. The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1) High Unionization	(2) Low Unionization	(3) High Seasonality	(4) Low Seasonality
UI	5.358*	6.905***	5.263	5.980**
	(2.901)	(2.374)	(3.866)	(2.415)
Observations	350	432	389	393
R-squared	0.100	0.075	0.165	0.082
State Controls	YES	YES	YES	YES
Firm Controls	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
χ^2-Stat (p-value)	0.23 (0.633)		0.03 (0.860)	

A Appendix

This Appendix has additional results and details omitted from the main text for the sake of brevity. Appendix A.1 gives definitions and sources of the main variables used in the empirical analysis. Appendix A.2 pprovides a brief synopsis of the 12 UI benefit increases considered in the text, and of the 6 excluded. Appendix A.3 includes additional robustness checks and evidence on the relationship between UI generosity and leverage.

A.1 Data Definitions

Table A1
Variables Definitions and Sources

This table has definitions and data sources of the main variables used in the paper. Compustat items are in italic.

Variable	**Definition**	**Source**
Dividend Payout	Common Dividend (*dvc*) divided by Net Income (*ni*)	Compustat
Total Payout	Common Dividend (*dvc*) plus Repurchases (*prstkc*), all divided by Net Income (*ni*)	Compustat
Debt to Assets	Long-Term Debt (*dltt*) plus Debt in Current Liabilities (*dlc*), all divided by Total Assets	Compustat
Investment	Capital Expenditures (*capx*) divided by lagged Total Assets (*at*)	Compustat
ROA	Earnings Before Interest, Taxes, Depreciation and Amortization (*ebitda*) divided by lagged Total Assets (*at*)	Compustat
Cash	Cash and Short-Term Investments (*che*) divided by Total Assets (*at*)	Compustat
Repurchase	Repurchases (*prstkc*) divided by Net Income (*ni*)	Compustat
Z-Cash	Cash minus the average cash within the industry divided by its standard deviation	Compustat

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Table A1 – *Continued from previous page*

Variable	**Definition**	**Source**
Cash Flow	Net Income (*ni*) divided by lagged Total Assets (*at*)	Compustat
Leverage	Book Debt divided by Total Assets (*at*). Book Debt is defined as Total Assets minus Book Equity. Book Equity is Total Assets Total Liabilities (*lt*) minus Preferred Stock (*pstk*) + Deferred Taxes (*txdb*) plus Convertible Debt (*dcvt*). If Preferred Stock Variable is missing, Redemption Value of Preferred Stock (*pstkrv*) is used instead.	Compustat
Leverage	Book Debt (defined as in the variable "Leverage") minus cash (*che*) divided by Total Assets (*at*).	Compustat
Employment Volatility	Average of the firm standard deviation of change in Log(Employment) (*emp*) computed across all firms in each industry (defined at the 2-digits SIC level) between 1991 and 2007. Only firms with at least 5 non-missing observations are kept.	Compustat
Earnings Volatility	Average of the firm standard deviation of change in ROA computed across all firms in each industry (defined at the 2-digits SIC level) between 1991 and 2007. Only firms with at least 5 non-missing observations are kept.	Compustat
Q	Total Assets plus Market Value of Equity ($csho \times prcc\_f$) minus Common Value of Equity (*ceq*), all divided by Total Assets	Compustat
Labor Intensity	Number of employees (*emp*) divided by either Total Assets (*at*) or Property, Plant and Equipment (*ppent*)	Compustat

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Table A1 – *Continued from previous page*

Variable	**Definition**	**Source**
Seasonality	Standard Deviation of change in the logarithm of quarterly revenues (*saleq*) over the year	Compustat Quarterly
EPS	Last analysts' consensus (average) forecast of earning per share preceding the end of the firm fiscal year divided by share price at the end of the fiscal year	I/B/E/S and CRSP
Recommendation	Last analysts' consensus (average) recommendation preceding the end of the firm fiscal year on a 1 (strong sell) to 5 (strong buy) scale	I/B/E/S
LT Growth	Last analysts' consensus (average) forecast of long term earnings growth preceding the end of the firm fiscal year	I/B/E/S
Income	State Income per Capita	US Bureau of Economic Analysis
UI	A dummy equal to 1 if the firm's headquarter is in a state that experiences an increase in the maximum unemployment benefit of at least $100 (in 2010 dollars)	Elaborations from "Changes in Unemployment Insurance Legislation" in several editions of the "Monthly Labor Reviews" (Bureau of Labor Statistics)
Unionization	State Union Coverage Density	Hirsch and Macpherson UnionStats Database
Unemployment Rate	State Unemployment Rate	US Bureau of Labor Statistics
Industry Wage Growth	Yearly Percentage Average Wage Growth at the industry level (defined at the 4-digits NAICS level)	US Bureau of Labor Statistics
Industry Employment Growth	Yearly Percentage Employment Growth at the industry level (defined at the 4-digits NAICS level)	US Bureau of Labor Statistics

A.2 Case Studies

In this Appendix I reconstruct briefly the political environment surrounding the twelve UI changes for which I was able to obtain sufficient information through newswire and articles searches in Factiva, Lexis Nexis and Google. I also add some details regarding concurrent policies adopted. I first describe the 12 events which I judged unconnected to macroeconomic or unrelated to the adoption of significant additional policies, and then the 6 excluded events. More details about the screening criteria are in Section 4.3.

Selected Events

(1) Florida 1992

Maximum Unemployment benefits was raised from \$225 to \$250 a week. For the previous 11 years, lawmakers had increased the benefit cap every few years in \$25 increments. The measure passed the Senate with a close vote (18-16) because some senators had originally proposed a percentage increase anchored to the average statewide weekly wage. The proposal was rejected as it would have implied an automatic increase each year. The increase was estimated to cost \$37 million out of the \$104 million of Unemployment Benefits Trust Fund. *(Source: St. Petersburg Times, 06/06/1992)*

(2) Missouri 1997

Missouri increased maximum payment from \$175 to \$205, to increase \$15 a year up to \$250 in 2001. The most recent increase was in 1992, by \$5. The bill was a compromise between business lobbies and unions motivated by the strikes occurred the same year at the McDonnell Douglas plant, where many business groups were outraged when the state granted benefits to strikers. Under the new bill, workers' representatives accepted that no benefits would be paid to strikers, but obtained the raise in the maximum weekly cap. *(Source: St. Louis Post-Dispatch, 05/21/1997)*

(3) New York 1998

On August 14th, Governor Pataki, at that time running for re-election, signed into law raising maximum unemployment benefits from $300 to $365 per week, with an additional increase in 2000 up to one-half the state's average weekly wage. It was the first increase in six years. The law was the outcome of negotiations with the AFL-CIO and the Business Council of New York state, a lobbying group representing corporate interests. The new law also put in place a wage reporting system, in which computerized information from the state tax department will be used to verify wages, a system less prone to fraud. New York had been the only state in the nation not using a wage reporting system at that time. *(Source: Buffalo News, 08/14/1998)*

(4) Virginia 2000

Governor Gilmor signed the so-called "Tultex bill," a reference to the textile company that laid off 2,000 workers. The bill initially was meant to boost unemployment benefits and provide health insurance for unemployed textile workers. Both Gilmor and the Republicans congressmen felt it would be unfair to raise jobless benefits for Virginians in some parts of the state and not others. After a lengthy political battle, a bill backed by Gilmore that raised the maximum weekly unemployment benefits by $36, to $268, passed the legislature. *(Sources: KRTBN Knight Ridder Tribune Business News, 04/20/2000; Associated Press Writer, 03/11/2000)*

(5) California 2001

On October 1st, Governor Davis signed a legislation boosting unemployment benefits by $100 a week starting from January 1, 2002, the first raise in nine years. Davis vetoed a similar legislation the previous year but, lagging in public opinion polls, was seeking to shore up support from organized labor, which had made an increase in unemployment benefits one of its top priorities. The bill established additional increases amounting in $40 per week each year until 2005, when the maximum unemployment insurance check reached $450 per week. No increase in UI taxes were expected in the near future because, as a result of the booming economy in the late 1990s, California's unemployment insurance trust fund amounted to more than $6 billion. *(Source: Los Angeles Times, 10/01/2001)*

(6) Tennessee 2001

The legislation signed by Governor Sundquist increased the maximum weekly unemployment benefit by $20 to $275. This was part of a compromise with lobbyists for labor and business. Union leaders obtained the increase and accepted that certain companies would continue getting state-funded training for their workers under the state Job Skills program for another five years. *(Source: Associated Press Newswires, 07/06/2001)*

(7) Georgia 2002

Governor Barnes approved and increase in unemployment benefits to be phased in over two calendar years to $300 per week from the current $284. In 2003, benefits went up by $6 a week, followed by a $10 increase during 2004. Because of a healthy state's fund, no increases in unemployment taxes were foreseen. The main additional provision was a sales-tax holiday for clothing, school supplies, and computer equipment, in an attempt to curry favor with Georgia businesses during Barnes' (failed) re-election campaign. *(Source: Associated Press Newswires, 02/06/2002)*

(8) Maryland 2002

Maximum unemployment benefits were increased by $30 (from $280 to $310) as a result of protests by unemployed workers and their families in conjunction with union, community, and religious leaders. All parties petitioned for more generous unemployment insurance following the $143 million the state Unemployment Insurance system received as a result of the economic stimulus bill passed by the US Congress. Had the Assembly not taken action before their April 8th session, the infusion of the new federal funds would have triggered an automatic reduction in the unemployment taxes paid by state employers. *(Source: U.S. Newswire, 03/28/2002)*

(9) Michigan 2002

Maximum unemployment benefits were increased by $62 (from $300 to $362) by Governor Engler, the first time since 1995. The law passed after weeks of debate and failed deals, with one GOP House member even losing his committee chairmanships in the political dispute and more than 1,500 workers protesting the delay in the benefit increase on the Capitol lawn. The most controversial point was the "waiting week" requested by Republican legislator and business lobbies, which was opposed by unions. The legislation in the end did not include such provision but reduced the maximum cap to $362, relative to the initially planned $415. No other provisions regarding taxes were included in the bill. *(Sources: Associated Press, 04/19/2002, 04/26/2002; U-Wire, 08/05/2002)*

(10) New Hampshire 2002

New Hampshire increased the maximum benefits cap from $331 to $372. Governor Jeanne Shaheen, who would soon be involved in the state Senate elections, urged lawmakers to act on the bill. Among the reasons cited were the layoffs of mill workers in the Berlin area that had occurred the previous year. No tax increases were expected to occur. *(Source: Associated Press, 02/13/2002)*

(11) Arizona 2004

Governor Napolitano signed a legislation increasing maximum unemployment benefits from $205 to $240 in April 2004, after she vetoed a similar bill during the previous year. The bill was declared dead by a Republican congresswoman, as unions and business groups lobbies could not find an agreement. A business lobby obtained the inclusion of stricter eligibility restrictions on payment of unemployment benefits while former employees receive severance pay, whereas unions condensed an initially planned two-step increase in an immediate boost of maximum claim. *(Sources: Associated Press, 04/02/2004, 03/17/2004, 04/07/2004, 05/17/2004)*

(12) Alabama 2006

Governor Riley signed into a low a bill raising the maximum cap from $220 to $230 (and to $235 the following year). No other provision were included in the bill. According to the president of the Alabama AFL-CIO "the legislation was needed because Mississippi had decided to raise its lowest-in-the nation unemployment compensation rate, and Alabama would have become the lowest paying state." *(Source: Associated Press, 04/17/2006)*

Excluded Events

(1) Florida 1997

Governor Chiles approved an increase in the maximum weekly benefit of $25 (from $250 to $275). The idea was suggested first by unofficial gubernatorial candidate Jeb Bush, who, given the large surplus in the state unemployment fund, suggested a one year $158 million tax cut. The bill also had tax cuts for new businesses. In order to appeal to labor advocates, however, the bill also included an increase in the weekly benefit cap. Some labor advocates and a few Democrat House members argued that the bill was disproportionately favoring businesses over workers. *(Sources: Associated Press, 04/18/1997; St. Petersburg Times, 03/07/1997)*

(2) Louisiana 1997

Louisiana increased maximum unemployment benefits from $193 to $215 following a compromise between labor and business unions regarding how to employ the state trust fund, which had reached over $1 billion. After an initial request made by unions of an increase up to $230, closer to the southern average, the measure passed, together with a reduction in employers' unemployment taxes and a worker training fund for businesses. *(Sources: Associated Press, 04/18/1997, 09/19/1997)*

(3) Arizona 1998

Governor Hull signed a legislation increasing weekly benefits from $185 to $195 in 1999 and $205 in 2000. The decision was part of a large-scale plan for business tax cuts, including one in the vehicle license tax, an increase in exemptions for personal income taxes, and cuts in personal income tax as well. As a political compromise, Democrats managed to add a provision to increase state payments to unemployed workers. *(Sources: Associated Press, 03/17/1998, 03/25/1998, 04/16/1998, 04/24/1998)*

(4) Kentucky 1998

Maximum unemployment benefits were increased $32 (from $256 to $288) by Governor Patton. The legislation was made possible by the healthy state of the UI fund (which had risen to $555 million, well above the $350 million required by the Kentucky law), which had a $24 million surplus and was accompanied by reduction in the UI tax rates. *(Sources: Associated Press, 02/04/1998, 03/23/1998; Capital Markets Report, 03/24/1998)*

(5) Georgia 1999

Maximum unemployment benefits were increased by $40 in Georgia, jointly with the approval of a large-scale four-year tax cut put forward by Governor Roy Barnes. This was part of his first year legislative agenda, and was made possible by the fact that state's unemployment reserve held in Washington was at about $2 billion. Similar tax cuts were approved by other Southern states (Florida, Virginia, South Carolina and North Carolina). *(Sources: The Atlanta Constitution, 04/28/1999; Associated Press, 04/27/1999)*

(6) Alabama 2002

In April, Alabama increased the maximum weekly cap by $20 (from $190 to $210) due to pressure from labor unions (Alabama was still at the last place in the country) and thanks also to $111 million from a federal economic stimulus package to help the unemployed. Business lobbies would have accepted an even larger increase, but requested a one-week waiting period before laid-off workers could qualify, which was rejected by unions. Although initially no tax change was foreseen, a later change to slightly increasing UI taxes was approved. *(Source: Associated Press, 04/17/2002)*

A.3 Additional Results and Robustness Checks

This Appendix has additional robustness checks. Table A2 shows that analysts forecasts for the treated and control firms selected according to the procedure outlined in Section 4.3 do not differ prior to the UI changes. Following Fresard and Valta (2003), I consider three measures: average sell/buy recommendation (which varies between 1 and 5, with higher values corresponding to more optimistic forecasts), average of earnings per share forecast standardized by the stock price at the end of the fiscal year, and average Long Term Growth, which is the mean 5-years growth earnings forecast. None of the differences in mean are statistically significant.

In Table A3, I employ three alternative measures of change in dividend payout as dependent variables. In column 1 the dependent variable is the change in common dividend between year $t+1$ and year t, all scaled by net earnings. In column 2 the dependent variable is the change in dividend over total assets. Column 3 is the change in dividend per share. Results are qualitatively similar to those of Table 5. In columns 4 and 5 I split the sample according to whether the firms in the sample have paid any dividends at time t.

Un Table A4 I use, as dependent variables, the change in book leverage and net leverage. In columns 3 and 4 equation 1 is estimated on a sample of treated and control firms with negative earnings in year t. The matching procedure is identical to that described in Section 4.3.

Table A2

Treated VS Control Firms: Analysts' Forecasts

Table A2 reports means of control variables of treated and control firms, matched according to the procedure outlined in Section 4.3. Variables definitions are in Appendix A.1. Δ is the first difference operator.

	Treated (Std. Error)	Control (Std. Error)	Treated - Control (Std. Error)
Recommendation	1.989	2.038	-0.048
	(0.045)	(0.045)	(0.072)
Growth	18.66	18.15	0.507
	(1.383)	(0.619)	(1.465)
Earnings	0.043	0.041	0.002
	(0.005)	(0.004)	(0.007)
ΔRecommendation	-0.007	-0.000	-0.007
	(0.005)	(0.003)	(0.006)
ΔGrowth	0.001	0.045	-0.044
	(0.025)	(0.032)	(0.037)
ΔEarnings	-0.950	-0.624	-0.325
	(0.276)	(0.296)	(0.400)

Table A3
Alternative Payout Measures and
Intensive VS Extensive Margin

Table A3 reports regressions of different measures of change in dividend payout on a UI dummy and lagged changes in Q, Log(assets), debt to assets, ROA, Log(income), unemployment rate, year and industry dummies (defined according to the two digits SIC classification). In column 1 the dependent variable is the change in common dividend between year *t+1* and year *t* scaled by earnings in year *t*. In column 2 it is the change in common dividend scaled by total assets. In column 3 it is the change in dividend per share. In columns 4 and 5 the dependent variable is the change in dividend payout (defined as common dividend over net income). Column 4 includes only firms with zero dividend payout at time *t*; column 5 includes only firms with positive dividend payout at time *t*. The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	$\frac{\text{Div}_{t+1} - \text{Div}_t}{\text{Earnings}_t}$	$\Delta\frac{\text{Dividends}}{\text{Assets}}$	$\Delta\frac{\text{Dividends}}{\text{Shares}}$	$\Delta\frac{\text{Dividends}}{\text{Earnings}}$	$\Delta\frac{\text{Dividends}}{\text{Earnings}}$
UI	0.814***	0.228**	6.116*	1.753***	12.372**
	(0.298)	(0.106)	(3.146)	(0.563)	(5.787)
Observations	983	782	782	477	305
R-squared	0.079	0.053	0.291	0.033	0.156
Year FE	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES
State Controls	YES	YES	YES	YES	YES
Firm Controls	YES	YES	YES	YES	YES
Firms in Sample	All	All	All	Zero Div.	Positive Div.

Table A4
UI and Leverage

Table A4 reports regressions of changes in leverage (columns 1 and 3) and net leverage (columns 2 and 4 on a UI dummy and lagged changes in Q, ROA, debt to assets, Log(assets), the logarithm of state income per capita and unemployment rate. Each regression includes year and industry dummies (defined according to the two digits SIC classification). The sample comprises matched and treated firms that experience a UI increase between 1991 and 2007. In the year before a UI increase treated firms are matched by Tobin's Q, Log(assets), ROA, debt to total assets and industry. Please refer to the Appendix A.1 for a definition of the variables. The sample of columns 1 and 2 includes only firms with positive earnings in year *t* and *t+1*. In columns 3 and 4 the same matching procedure is adopted, but requiring each firm to have negative earnings in year *t*. Standard errors, clustered at the state level, are reported in parentheses. *, ** and *** denote statistical significance at the 10% 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Dependent Variable:	Leverage	Net Leverage	Leverage	Net Leverage
UI	-0.005	0.005	0.022*	0.031**
	(0.006)	(0.008)	(0.013)	(0.013)
Observations	736	735	582	582
R-squared	0.214	0.111	0.079	0.064
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
State Controls	YES	YES	YES	YES
Firm Controls	YES	YES	YES	YES
Firms in Sample	Negative Earn.	Positive Earn.	Negative Earn.	Positive Earn.