

Payroll Tax Incidence: Evidence from Unemployment Insurance

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Abstract

Economic models assume that payroll tax burdens fall fully on workers, but where does tax incidence fall when taxes are firm-specific and time-varying? Unemployment insurance in the United States has the key feature of varying both across employers and over time, creating the potential for labor demand responses if tax costs cannot be fully passed on to worker wages. Using state policy changes and matched employer-employee job spells from the LEHD, I study how employment and earnings respond to payroll tax increases for highly exposed employers. I find significant drops in employment growth driven by lower hiring, and minimal evidence of pass-through to earnings. The negative employment effects are strongest for young workers and single-establishment firms.

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1 Introduction

Employer taxes on labor earnings are used to fund a variety of social insurance programs, and policymakers frequently view payroll tax cuts as a way to stimulate employment. In the theory of tax incidence, the resulting employment and wage change depends on the elasticities of labor demand and labor supply; because labor demand is assumed to be much more elastic than labor supply, canonical models assume payroll tax costs are born entirely by workers, resulting in little to no effect on employment.¹ However, there may be imperfect pass-through in the presence of imperfect competition or downward wage rigidities, leading to a decline in employment rather than wages.² Hamermesh (1979) points out that if the entire burden of a payroll tax could be shifted onto wages, employers would not so vehemently oppose tax increases. Furthermore, payroll tax changes that only affect a subset of firms may prevent employers from fully passing costs through to wages.³

In this paper, I exploit state Unemployment Insurance (UI) tax increases to estimate payroll tax incidence in the United States. The third largest payroll tax in the country, UI is an employer-specific and time-varying tax that is administered at the state level. Employer-specific tax rates are an increasing function of the amount of UI benefits paid out to previous employees. In 2010 and 2011, state-legislated UI tax increases following the Great Recession raised the payroll tax costs for highly exposed employers in multiple states. These tax increases ranged from \$200 to \$600 per worker, and I use administrative data from the US Census Bureau’s Longitudinal Employer-Household Dynamics (LEHD) to study the impact of these increases on worker earnings and employment growth.

My identification strategy exploits two dimensions of tax variation: cross-state variation due to state policy changes, and within-state variation based on employer exposure. Highly

¹As evidenced by early estimates from Brittain (1971) and Gruber (1997).

²Using data from a large payroll processing firm, Grigsby et al. (2021) find very low rates of wage cuts, providing strong evidence for downward nominal wage rigidity. Using Census Bureau data, Murray (2021) estimates that downward nominal wage rigidity accounted for at least 23% of excess job destruction during the financial crisis of 2008.

³Likewise, Saez et al. (2012) and Saez et al. (2019) found that payroll tax changes only affecting a subset of workers within a firm also failed to result in full pass-through.

exposed employers, defined as those with a history of significant layoffs, are charged UI taxes close to the state maximum. Thus, when a state policy change increases the maximum UI taxes on employers, highly exposed employers face binding tax increases, while minimally exposed employers face little to no tax increase because they are already far away from the maximum. First restricting my analysis to these highly exposed employers, I use a difference-in-differences research design to compare the outcomes of employers located in states that experienced tax increases versus their counterparts located in states that did not. I then augment my analysis with a triple-difference research design that adds in low-exposure employers, that nets out the difference between high-exposure and low-exposure employers within the same state. This allows me to control for potential differences in state economic conditions or other state policies in the wake of the Great Recession.

I estimate negative impacts of state tax increases on employment growth, and little evidence of pass-through to earnings except for the lowest-earning workers. For each \$100 increase in payroll taxes per worker, employment growth declined by 0.74 percentage points over the course of the year. Event study estimates found the largest effects in the 2nd and 3rd year after the initial policy changes, which suggest that these UI tax increases hindered the economic recovery after the Great Recession. I test whether this drop in employment growth is driven by increased separations or reduced hiring, and find evidence of the latter. The quarterly share of new hires fell by up to 0.2 percentage points for each \$100 tax increase, while the effect on the separation share was small and statistically insignificant. These findings align with firm tax incentives: Each new hire incurs an additional UI tax cost, while additional separations provide no further UI tax savings. This is because low taxable earnings thresholds cause most workers to exceed their payroll tax base early in the year. Heterogeneity analysis shows that the negative employment effects are stronger for young (age under 25) workers. Low-earning workers also experienced larger drops to earnings growth than the average worker, but still below full pass-through. Additionally, single-establishment firms were more likely to reduce hiring than multi-establishment firms,

suggesting that cash constraints in the face of an unexpected tax increase may also play an important role.

The recent literature on payroll tax incidence has combined administrative microdata with exogenous policy changes to provide strong evidence of imperfect pass-through. These studies largely focus on European and South American countries, as the lack of variation in federal payroll taxes has caused U.S.-based studies to be absent from the recent literature. The main contribution of this paper is to provide a U.S. context to study payroll tax incidence, because differences in labor market institutions may prevent the estimates of tax incidence of one country from generalizing to another. The state-level UI tax increases I study provide a setting similar to Ku et al. (2020), which found that regional payroll tax increases in Norway lowered employment, because firms were only partially able to pass the costs on to workers. Saez et al. (2021) finds that the positive employment effects from a young workers payroll tax cut persisted even after workers aged out of eligibility. Likewise, I estimate a gradual and sustained decrease in employment growth due to payroll tax increases. Benzarti and Harju (2021*a*) found that a payroll tax cut in Finland helped treated firms weather the Great Recession despite having minimal impact on employment beforehand, suggesting a cyclical component to firm responses. My findings of a negative employment response to tax increases following the Great Recession suggests that firms respond symmetrically to tax changes.

Another contribution is the estimation of heterogeneous effects across worker and employer characteristics. Benzarti and Harju (2021*b*) found that firms with greater payroll taxes substituted away from low-skilled and manual workers. I find greater drops in labor demand for low-earning and young workers, but not for those with less education. My finding that single-establishment firms were especially likely to decrease hiring in response to higher UI taxes, is consistent with the finding in Lobel (2021) that small firms were more responsive to payroll tax cuts. Biró et al. (2022) estimate heterogeneity in payroll tax incidence by firm productivity, finding that lower productivity firms experienced employment responses

while higher productivity firms passed tax cuts on to workers. While I do not observe firm productivity, my sample of high exposure employers likely overlaps with lower productivity firms, as they suffered layoffs prior to the tax increases.

Finally, this paper relates to the small literature on UI tax incidence in the United States. Anderson and Meyer (1997) studies cross-sectional variation in state UI taxes, and Anderson and Meyer (2000) studies the introduction of experience rating in Washington. Both studies found evidence of pass-through for long-run industry UI taxes but not employer-specific rates. Using administrative data from Florida, Johnston (2021) found little effect of annual changes in employer-specific rates on worker earnings, but large impacts on hiring. This paper contributes to the literature by using exogenous policy changes from multiple states, and by estimating additional heterogeneity by worker and employer characteristics, as described above.

The next section provides institutional details about U.S. Unemployment Insurance. Section 3 describes the research design, and Section 4 discusses the data and construction of my analysis sample. Section 5 presents the main employer-level results, and Section 6 presents a worker-level analysis focusing on low-earning workers. Section 7 concludes.

2 Institutional Background

Unemployment insurance in the U.S. is funded solely through payroll taxes on employers.⁴ Unlike Social Security or Medicare taxes, which are uniform across all employers, UI is experience-rated and therefore employer-specific. UI tax revenues reached a height of 1 percent of total wages in 2012, but this masks considerable heterogeneity across employers. Employers that lay off workers who then claim UI benefits are assigned a higher future tax rate - up to a statutory maximum.⁵ These employer-specific rates are calculated annually,

⁴With the exception of New Jersey, which also charges workers a uniform payroll tax of 0.5%.

⁵Guo and Johnston (2021) provides a brief history of U.S. unemployment insurance, which was modeled after the experience rating in workers' compensation. Since its inception, policymakers have recognized the tradeoff between disincentivizing layoffs, and the cyclical tax burdens that dynamic experience rating would

and Appendix Figure A.1 illustrates a sample tax schedule from the state of Florida, where the maximum tax rate is capped at 5.4%. In 2008, only 5 percent of eligible employers were assigned the maximum tax rate, while 77 percent were at the minimum. However, by 2011 these shares changed to 22 percent and 63 percent, respectively, due to layoffs during the Great Recession. Appendix Figure A.2 plots total UI contributions over time as a percentage of total wages, and reveals the cyclical pattern of UI tax collections, where tax rates are highest in the years following recessions. Due to this time-varying nature of UI tax rates, businesses may have more difficulty passing the costs on to their workers, and changes in UI tax liabilities have the potential to influence employment as well as earnings.

Another unique aspect of UI is the low tax base in most states. The federally mandated level of taxable earnings is only \$7,000, and because most states fail to index UI taxes to wage growth, they have experienced a gradual erosion of the UI tax base over the last half-century. In 2009, the year before the first UI tax increases, the median UI tax base was only \$10,000 – with the median state’s taxable wages covering only 28% of total payroll. When workers’ annual earnings exceed the UI tax base, UI becomes a lump-sum tax, independent of earnings. For example, an employer facing a UI tax rate of 5% and a tax base of \$10,000 would contribute payroll taxes of $.05 * 10,000 = \$500$ per worker for any worker earning more than \$10,000 annually. Thus UI taxation is regressive in the sense that seasonal, part-time, and low-wage workers are taxed at a higher effective tax rate (as a share of earnings), and these types of workers become relatively more costly for a business facing high UI tax rates. Huang (2022) finds that higher state taxable wage bases increases teenage employment rates, and Duggan et al. (2022) finds that higher tax bases increase labor demand for low-wage part-time workers.

Because unemployment insurance is administered at the state level, there is substantial variation in UI tax schedules across states. A tax increase in one state raises payroll costs for incumbent employers, without affecting tax regimes in any other. Many state UI programs

create.

are underfunded, leading them to deplete their UI trust funds during recessions. During the Great Recession, 36 states depleted their UI trust funds and were forced to take out loans from the federal government; this occurred again for 18 states during the height of the COVID-19 pandemic. Although states recognized the need to replenish their trust funds following the Great Recession, policy responses varied greatly, with some states legislating policy changes while others chose inaction. During this period the Obama administration had attempted to increase the federally mandated UI tax base (of \$7,000), which would have given states a way to obtain more tax revenue without needing to legislate their own UI tax policy. California, for example, did not legislate any UI tax increases despite needing to borrow from the federal government to pay out UI benefits.⁶ The possibility for states to choose inaction produced exogenous variation in state policy changes, despite similar financing needs.

3 Research Design

In 2010 and 2011, a total of sixteen states implemented UI tax increases in order to replenish their trust funds. Coupled with the mechanical increase in employer-specific tax rates due to experience rating, these tax hikes could amount to a 2-5% increase in effective tax rates. Within the LEHD research sample of 23 states and District of Columbia, Figure 1 graphs the distribution of maximum tax increases (tax base * maximum rate) from 2009 to 2011. Among this subsample, I exclude 7 states from my analysis due to baseline differences in UI tax regimes. Six of these excluded states automatically index UI tax bases to wage growth, resulting in incremental and predictable tax increases every year throughout my sample period (and thus greater tax costs and trust fund solvency at baseline than the treatment states). I also exclude Pennsylvania, which experienced a UI tax *decrease* in

⁶When state trust funds borrow from the federal government to pay out UI benefits, they are given two years to repay that debt. After this window, every single employer is charged \$21 per worker per year until the loans are paid back. These consequences are akin to a tax increase for employers, but the amount is small and spread out over the entire state (whereas state-legislated tax increases would charge high-exposure employers significantly more).

2011-12.

From the remaining states, I identify 9 treatment states whose statutory maximum UI taxes rose by more than \$100 from 2009 to 2011, leaving a remainder of 8 control states with minimal tax changes during this window. From 2009 to 2011, treatment states experienced per-capita maximum UI tax increases ranging from just under \$200 to over \$600, and the average increase was \$378. In percentage terms, maximums increased by 67% on average, and the largest tax increase was experienced by South Carolina (in both level and percentage terms).⁷ Figure 2 plots the locations of these 17 LEHD states, and the change in maximum UI taxes from 2009 to 2011.

In order to focus on employers for whom these state UI tax increases would be binding, I create a sample of high exposure employers that are likely to be close to the maximum tax rate following the Great Recession. This is based on imputed layoffs, and details are described in subsection 4.1. I first estimate event study regressions to test for differential pre-trends in treatment states relative to control states. I then estimate difference-in-differences regressions to estimate a continuous treatment effect from UI tax increases. To establish a consistent sample for both regression specifications, I restrict to the period 2008 to 2013, to allow for at least two years of pre-change data and three years of post-change data.

Estimating an event study requires defining a single starting date for each state tax increase. While some state increases occurred all at once, other states legislated a more gradual increase from 2010 to 2012. The pooled difference-in-differences estimation will incorporate this gradual increase in treatment magnitude, but for the event study I simply define event time relative to the quarter before the calendar year in which the initial tax increase occurs ($event = 0$ in either 2009:Q4 or 2010:Q4 for treatment states, and event time

⁷Appendix Figure A.3 plots maximum and average UI taxes over time for each control state, and appendix Figure A.4 and Figure A.5 plots them for treatment and excluded states, respectively.

always equals zero for control states). I then estimate the following regression specification:

$$Y_{fst} = \alpha_f + \sum_{k=-8, k \neq -1}^{13} \beta_k(event = k) + NAICS * \delta_t + \gamma minwage_{st} + \epsilon_{fst} \quad (1)$$

Here f indexes employer (SEIN), s indexes state, t indexes year-quarter, and k indexes the quarter relative to the policy change. Quarters more than two years before the policy change are assigned $event = -8$, and quarters more than three years after the policy change are assigned $event = 13$. To allow for potential anticipation effects, the treatment effects are estimated relative to the outcome two quarters prior to the tax change (ie: if a state increased taxes in 2010, the baseline would be Q3 of 2009, and if a state increased taxes in 2011, the baseline would be Q3 of 2010). Thus the excluded quarter is $k = -1$, and the first quarter of the tax change is labeled $k = 1$.⁸ Coefficients β_{-8} to β_{-2} test for differential pre-trends prior to the policy changes. Fixed effects are included for each SEIN, and fixed effects for year-quarter interacted with 2-digit NAICS sector control for industries such as construction being disproportionately impacted by the Great Recession. The primary outcomes of interest are year-over-year employment growth, and mean log quarterly earnings for stable employees (which excludes the earnings of new hires and separations).

I also estimate a pooled difference-in-differences regression with a continuous treatment variable, $Tax\Delta_{st}$, defined as the dollar change in maximum UI tax relative to 2009 (and defined to equal zero in years 2008 and 2009). The baseline regression specification is as follows:

$$Y_{fst} = \alpha_f + \sum_{q=1}^4 \beta_q * Tax\Delta_{st} + NAICS * \delta_t + \gamma minwage_{st} + \epsilon_{fst} \quad (2)$$

Here f indexes employer, s indexes state, and t indexes year-quarter. I estimate four coefficients of interest β_1 to β_4 to allow for differential responses by calendar quarter, because UI tax burdens are largest in Q1. The identifying assumption is that in the absence of UI tax policy changes, employers in the same 2-digit NAICS sector in treatment states and control

⁸States with 2010 increases are Arkansas, Maine, Maryland, Tennessee, and West Virginia; States with 2011 increases are Illinois, Indiana, Oklahoma, and South Carolina.

states would have evolved similarly in the years following the Great Recession.

It is worth noting that after the Great Recession, there was also a significant federal extension of UI benefit duration beyond the typical 26 weeks, through both the Emergency Unemployment Compensation (EUC) program and the Extended Benefits (EB) program. Benefit duration reached a potential maximum of 99 weeks in 2010-2011, before dropping in 2012 and coming abruptly to an end at the close of 2013. Farber and Valletta (2015) and Farber et al. (2015) found little impact of these benefit extensions on job finding, as they primarily caused fewer workers to leave the labor force and led to long-term unemployment. Thus, I assume the extension of UI benefits does not differentially impact worker labor supply during my analysis period.

4 Data

This project uses the Census Bureau’s Longitudinal Employer-Household Dynamics (LEHD), an administrative employer-employee matched dataset of quarterly earnings, directly sourced from state UI records. Each employer in the LEHD is assigned a state EIN number (SEIN), and this will be my firm definition. A SEIN is also the level at which UI tax rates are assigned to employers. It can encompass multiple establishments within a state, but no employment outside the state. And just as large firms may have multiple EINs associated with their business, these firms may also own multiple SEINs within a given state.

To construct my sample, I include employers from 9 treatment states and 8 control states, covering the time period from 2008 to 2013 (allowing for an unbalanced panel). I also drop the Retail Trade and Accommodation & Food Services sectors (NAICS 44-45 and NAICS 72). These two sectors experience high turnover and low UI take-up rates, such that it is very difficult to classify employers into high versus low experience rating. The exclusion of these sectors should not affect the generalizability of my results, as they rank among the sectors with the lowest average UI tax costs, just above Healthcare and Education (Guo and

Johnston (2021)).

I calculate each employers’s mean quarterly earnings, either for all workers, or for stable workers, which excludes workers who were just hired or separated. Since these are quarterly measures and no information on hours is available, the estimated tax incidence on earnings may also reflect changes in hours. In additional analyses in section 6, I identify low-earning workers in the LEHD to separately estimate the impacts on these workers with the highest relative UI tax burdens. To study employment outcomes, I count hires and separations based on the beginning and end of employment spells, and divide by employment to define quarterly shares of new hires and separations. I also measure year-over-year employment growth using DHS growth rates, defined as $\frac{Emp_t - Emp_{t-1}}{\frac{1}{2}(Emp_t + Emp_{t-1})}$.

4.1 Calculating UI Tax Exposure

State UI tax increases affected either the maximum tax rate or tax base, both of which require a sufficiently high experience rating to be binding. Because I do not directly observe employers’ payroll tax rates in the LEHD, I impute exposure to UI tax increases based on separation history. Employers who never separate workers into non-employment are inferred to have a low UI tax rate (low exposure), and employers who separate a significant share of workers into non-employment will be inferred to have a tax rate close to the state maximum (high exposure).

It is not layoffs themselves that impact employer-specific tax rates, but rather the amount of UI benefits claimed by laid off workers. In order to identify separations that could be eligible for UI benefits, I first limit the scope of consideration to workers with at least 2 consecutive quarters of earnings, and high quarter earnings of at least \$1500 (minimum requirement to be eligible for UI benefits in most states). I also ignore workers younger than age 18 as those job separations could be driven by schooling, and ignore workers older than age 60 as those separations could be driven by retirement. I then impute a layoff as a job separation in quarter t that results in at least one quarter of zero earnings from

quarters $t + 1$ to $t + 3$. Separations that were the result of moves to a different state will not be captured in the LEHD data, so these could also be miscoded as layoffs. I also drop employers with less than 20 or greater than 500 workers at baseline (2009:Q3), to minimize measurement error in this imputation.⁹

For each SEIN, I aggregate the number of imputed layoffs over two separate pre-periods, and use these two cumulative shares to define inclusion in my analysis sample as a high-exposure firm. The first pre-period runs from 2006:Q1 to 2007:Q4, and the second runs from 2008:Q1 to 2009:Q2 (the Great Recession began in December 2007 and ended in June 2009).¹⁰ High-exposure employers are defined as SEINs that experienced total layoffs of 33-100% of their 2009:Q3 baseline employment in each of the two pre-periods. The purpose of using two separate pre-periods rather than the cumulative total is to identify firms with persistent UI risk, rather than firms that experienced a single mass layoff event during the Great Recession.

I also identify a subset of low-exposure firms, defined as SEINs with imputed layoffs of less than 15% from 2006:Q1-2007:Q4, and less than 10% from 2008:Q1-2009:Q2. Given the likelihood of overestimating layoffs, an SEIN with an imputed layoff rate less than 10% very likely experienced zero or very little UI benefit claims. This set of employers, which are expected to be the least affected by UI tax increases due to their distance from the maximum tax rate, will act as a “placebo” low-exposure sample in triple-difference analyses.

Separation into non-employment is still an imperfect predictor of eventual experience rating, because of imperfect take-up rates and substantial variation in UI duration. In new work using administrative data from Washington, Lachowska et al. (2023) document significant employer-level heterogeneity in the take-up of UI benefits. Thus I view this exercise as accepting there will be false positives (separations that do not result in UI charges), in

⁹The 2009 County Business Patterns estimates that nationally, firms between 20-500 workers made up 10% of firms, and 31% of employment.

¹⁰I use the three-year period before the first state tax increases in order to best capture layoffs that could lead to potential UI tax increases. Layoffs that occurred prior to 2006 are unlikely to impact tax rates in 2010 and later, because most firms will have already finished experiencing tax increases. For example, layoffs during the 2008-09 Great Recession caused tax rates to peak in 2011-12.

favor of minimizing false negatives (missing potential UI charges). Thus the measurement error introduced by inadvertently including some employers with low UI tax exposure in the high-exposure group may attenuate my estimates. Nevertheless, my results are robust to using alternative layoff thresholds to define high-exposure employers.

4.2 Summary Statistics

Table 1 reports summary statistics for my sample of high exposure firms in treatment and control states at baseline in the third quarter of 2009. They are very similar in size, with average employment of roughly 67 workers. Average layoff histories also match extremely well, with the average employer having experienced cumulative layoffs in 2006-07 that amounted to 57% of their 2009:Q3 employment, and 50% for the period of 2008:Q1 to 2009:Q2. One area of mismatch, however, is annual earnings, with control states paying higher wages than treatment states. This is likely due to state-level differences in cost-of-living and minimum wages. Federal minimum wage increases occurred in July of 2007, 2008, and 2009, and treatment states were more likely to have faced binding minimum wage increases. Thus I also include a control for the state minimum wage in all of my regression specifications. This table also shows that the construction sector is overrepresented among high-exposure firms; nationally the industry only accounts for 10% of establishments, while in my analysis sample construction employers make up over 20%.

5 Results

I first establish that increases to the statutory maximum UI tax translated into increases in actual taxes paid. In the absence of employer-specific tax rates, I estimate an event study regression using aggregate data from the public-use Quarterly Census of Employment and Wages (QCEW). The observation level is a 4-digit industry, state, and quarter, and effective UI tax rates are calculated by dividing quarterly UI contributions by taxable quarterly

payroll.

Using construction industries as a proxy for high exposure employers (as they have the highest average tax costs), Figure 3 shows that the policy changes in treatment states led to an increase in UI tax costs, predominantly in the first two quarters of the calendar year when workers are still earning below the taxable earnings threshold. On average, Q1 tax rates in treatment states increased by 0.83, 1.1, and 1.0 percentage points over the three years following a policy change, relative to control states. Estimates from a difference-in-differences specification in Appendix Table A.1 find that each \$100 increase in maximum UI taxes increased effective UI tax rates by 0.16 percentage points in the first quarter of the year. However, this average effect at the industry level produces a lower bound on the size of tax increases for high-exposure employers, since not all firms within construction would be facing the maximum tax rate.

5.1 Event Study

Figure 4 plots estimates from Equation 1 using year-over-year employment growth as the outcome. I define year-over-year employment growth as the DHS growth rate of quarterly employment relative to the same quarter of the previous year, $\frac{Emp_t - Emp_{t-1}}{\frac{1}{2}(Emp_t + Emp_{t-1})}$, which creates a symmetric growth measure that allows for entry and exit. Employment growth shows a sustained decline in treatment states after the tax increase, with a 2 percentage point drop by the end of the first year, and a 4 percentage point drop after the second. This growing treatment effect over time could be driven by two factors. Looking at a breakdown of taxes by treatment state in Figure A.4, we see that some states experienced a gradual tax increase that peaked in 2012 (either a year or two after the initial increase). Secondly, average tax rates were also highest in 2011-2012, as layoffs from the Great Recession took a few years to translate into higher tax rates. This pattern is also robust to the inclusion of state-specific calendar quarter fixed effects to account for differences in geographic seasonality.

To study the impact on earnings, Figure 5 plots event study estimates using log quarterly

earnings as the outcome. Log earnings are calculated by taking the firm-level average of log quarterly earnings for all stable employees continuously employed from quarter $t-1$ to $t+1$. By excluding new hires and separations, this ignores any potential drops in earnings due to incomplete job spells. I also include state-by-calendar quarter fixed effects to account for the presence of state-specific seasonality in earnings.¹¹ Unlike employment growth, we observe a pre-trend in earnings prior to the policy change, likely due to the series of federal minimum wage increases imposed between 2007 and 2009. However, we also observe a drop in average earnings in the first quarter after the tax increase, although the estimate is not statistically significant. Taken at face value, this implies a potential 1% drop in Q1 earnings. This is a significantly smaller drop than the 5% benchmark of full pass-through, which is estimated by dividing the average first quarter tax change by average first quarter earnings. To further investigate the possibility of UI tax increases impacting workers' quarterly earnings, I estimate earnings impacts at the worker level in section 6.

5.2 Difference-in-Differences

I next use a difference-in-differences estimation strategy to estimate average treatment magnitudes. In these specifications, I now use a continuous treatment measure equal to the change in the maximum UI tax schedule between time t and 2009, and equal to zero prior to 2009. I estimate specifications described by Equation 2, which include employer (SEIN) and industry-by-time fixed effects, as well as a control for the state minimum wage.

Table 2 reports estimates for three different employment outcomes, with unweighted estimates reported in the odd columns, and estimates weighted by firm employment in the even columns. The baseline specifications are unweighted, since the analysis sample is already restricted to SEINs between 20 and 500 workers in 2009, although weighting generally has

¹¹Figure A.6 plots event study estimates without state-by-calendar quarter fixed effects, showing that treatment states had a pattern of lower first quarter earnings relative to the rest of the calendar year. This persistent seasonality in earnings is likely driven by regional differences in climate. Two of the largest control states, California and Florida, are known to have very mild winters and thus less seasonal employment in high exposure industries such as construction and recreation.

little impact on the estimated magnitudes. Column 1 estimates that UI tax increases lower year-over-year employment growth, and the effect compounds throughout the year. For the average firm, a \$100 increase in per-capita UI taxes lowers employment growth by 0.43 percentage points by the end of the year. This implies that in a state like Indiana, which experienced a \$500 jump in maximum UI taxes in 2011, employment growth fell by 2.2 percentage points in high exposure firms, relative to similar counterparts in control states.

To decompose this employment change into hires and separations, I define the quarterly new-hire share as the number of new hires divided by employment, and the quarterly separation share as the number of separations divided by employment. With the exception of one quarter in Column 6, the separation estimates do not suggest any firm response to tax changes. The new hire estimates, on the other hand, suggest a negative impact on hiring, with each \$100 increase in per-capita maximum UI taxes lowering both the first quarter and last quarter hiring rate by 0.2 percentage points. This pattern of negative employment growth and hiring is consistent with the mechanism of greater UI tax burdens. At the start of the calendar year all earnings up to the tax base are eligible for UI taxes, and thus create a large payroll tax burden for the employer. Each additional worker hired incurs an additional UI tax burden, whereas additional separations do not incur a tax saving and could even push future costs higher if they claim UI benefits. Towards the end of the calendar year, most if not all existing workers have exceeded the UI tax base and no longer cost their employer any additional payroll tax. However, the earnings of new hires still incur taxes up to the same tax base, resulting in a much higher effective tax rate for new hires; this could explain the observed reluctance to hire in the last quarter of the year. Although I am unable to observe this, another possible explanation for the drop in employment growth is substitution to contract workers, who would not be subject to UI taxation.¹²

Table 3 reports estimates for three earnings outcomes. Columns 1 and 2 calculate the employer's average quarterly earnings across stable workers who were employed for the full

¹²For example, Kugler et al. (2017) found that payroll tax cuts in Colombia increased rates of formal employment among affected workers.

quarter, while Columns 3 and 4 include all workers when calculating the average. Both outcomes show a small increase in log quarterly earnings after the tax change in all quarters except for Q1, consistent with the observed pre-trend in Figure 5. The absence of an increase in the Q1 DD coefficient implies a maximum tax incidence of 40% for every additional dollar of UI tax (given that \$100 is roughly 1% of mean quarterly earnings), although this effect is likely driven by seasonal employment patterns and is not robust to a triple-difference specification.

In Columns 5 and 6 I estimate an alternative measure, which is how likely workers are to experience an earnings increase in a given quarter. Because quarterly earnings vary throughout the year (especially when inclusive of hours), I define a simple indicator for whether the worker earned more in the current quarter than in the previous two quarters (ignoring any workers with less than 3 quarters of tenure). The firm-level outcome is then calculated as the share of workers satisfying the previous definition. This measure is expected to be correlated with the share actually receiving raises, but estimates are likely to be attenuated by measurement error due to the quarterly frequency. These estimates provide further evidence for a minor degree of tax incidence in the first quarter of the year; for every \$100 increase in per-capita maximum UI taxes, the share of workers receiving a raise in earnings in Q1 falls by 0.4 percentage points.

5.3 Triple-Difference Design and Robustness

In order to control for state-specific factors that could be confounding our estimates, I also conduct a difference-in-difference-in-differences estimation comparing the main sample of high-exposure employers to a placebo sample of employers that experienced minimal layoffs. Because UI tax increases are more binding for employers near the maximum tax rate, we can use employers with minimal layoffs – and therefore low exposure to UI tax increases – to control for confounding state-level variation that may influence both tax policy and employment outcomes.

Figure 6 plots event study estimates for year-over-year employment growth using this triple differences design on the expanded sample of both high- and low-exposure employers. It estimates Equation 1 with not only the event-time treatment dummies, but also a set of treatment dummies interacted with whether the employer is classified as high-exposure. These interactions become our new coefficients of interest, while the baseline event-time estimates control for confounding conditions in treatment states. The estimates show no differential pre-trend prior to the tax increases, and a sustained drop in growth after, reaching negative 5 percentage points by the end of the second year. This provides further evidence of a causal relationship between state UI tax increases and drops in labor demand.

Table 4 reports estimates of a pooled triple-difference regression specification studying employment growth, hiring, separations, earnings, and raises. The $Tax\Delta*High$ coefficients for the employment measures in columns 1 through 3 are consistent with those from the baseline difference-in-differences specification, and imply an even larger employment decline. Within just the first quarter of the year, employment growth drops by 0.5 percentage points for every \$100 UI tax increase, and this negative effect grows to -0.74pp by the end of the year. On the other hand, we no longer observe an impact on log earnings or the share receiving raises. This suggests that the previously observed negative impact on Q1 earnings and raises was due to underlying differences in treatment and control states unrelated to UI tax increases, and the wage incidence of UI tax increases are minimal. These results are also robust to the inclusion of state-by-year-quarter fixed effects, reported in Table A.2.

My results are also robust to a number of sensitivity checks of sample composition.¹³ First, while I opted to exclude the retail trade and food/hospitality sectors due to higher likelihood for measurement error, both the difference-in-differences and triple-differences estimates are robust to the inclusion of these employers. Second, because I do not observe actual employer-specific tax rates, I measure expected UI tax costs based on imputed layoff histories. Small changes to the cutoff of layoff shares that warrant inclusion in my main

¹³Estimates from robustness checks have not been disclosed from the Census RDC, but would be available for additional disclosure should they be required.

analysis sample had no qualitative effect on my estimates. Additionally, the results are not driven by the inclusion of any one state in the analysis sample, nor does the restriction to a balanced panel impact the estimates.

Finally, to address potential concerns with the differential timing of state UI tax increases, we can estimate separate event study regressions including only 2010 treatment states or only 2011 treatment states, respectively. This ensures that each group of treatment states are only compared to the control states, and there is no longer an issue of staggered treatment, since all tax increases occur at the same time. Treatment effects from this exercise are qualitatively similar to those using the pooled sample.

5.4 Heterogeneity by Worker and Employer Characteristics

I next test for heterogeneous responses for three specific groups of workers. First, I study the impact on the hiring rate of low-earning workers, who are defined to be those who earned less than \$3000 in the quarter after being hired (inflation adjusted to 2015 dollars).¹⁴ Next I calculate year-over-year employment growth rates for two subgroups of interest: workers younger than 25 years of age, and those with only a high school education or less (conditional on being 25 or older).¹⁵ For firms that did not employ these types of workers in either the current or previous calendar quarter, growth rates are defined to equal zero.

Table 5 reports estimates for these three outcome measures, with baseline difference-in-difference estimates in the odd columns and triple-difference estimates in the even columns. Column 1 shows that a \$100 increase in the maximum UI tax lowered the Q1 share of low-earning new hires by 0.1pp, and Column 2 shows that this effect is robust to the inclusion of the third difference, although less statistically significant. When benchmarked to the sample mean of 5.3%, the magnitude implies a similar effect as that of the overall share of new hires from the previous table (0.16pp relative to a 10.6% mean).

¹⁴The \$3000 threshold was chosen to coincide with the federal poverty level for a single person household (\$11,770 in 2015).

¹⁵Outcomes for workers older than age 55 were also estimated, but the results did not warrant submission for disclosure.

Recall the previous triple-difference estimate of a \$100 increase in maximum UI taxes lowering employment growth in Q4 by 0.74 percentage points (Column 1 of Table 4). For young workers, Column 4 estimates that the impact is an even larger decline of 0.90 percentage points for each \$100 increase. Growth rates for low-educated workers, on the other hand, are more similar to the overall effect, with estimated coefficients in Column 6 that are similar in magnitude to the estimates for overall employment growth (-0.75pp by Q4 for each \$100 increase). This suggests that young workers who are just entering the workforce are the most impacted by falls in labor demand due to UI tax increases.

To test for heterogeneous responses by different types of employers, I classify employers based on their characteristics prior to the policy changes (as of 2009:Q3). I focus on three observable characteristics: median worker earnings, multi-establishment status, and firm age. The latter two are also potential proxies for whether the employer would face cash or financing constraints, as a surprise increase in UI taxes could restrict available cash flow. Recall that all firms in the analysis sample are already age 5 or older by 2010, as they needed a long enough layoff history to be identified as high exposure, so I use an age cutoff of 20.

The main outcome of interest is the new hire share, and the estimation augments the differences-in-differences specification described by Equation 2. In each specification, I add four additional interaction terms interacting UI tax changes in each quarter with one of the following indicators defined at baseline: Median quarterly earnings below \$6000, single-establishment firm (nationally), or firm age less than 20 years old. Figure 7 plots estimates for each of the three characteristics. The estimates show little difference in hiring whether an employer has a majority of low-earning workers or not. However, the single establishment interactions show a large negative response for firms that only have a single location (seen by the interactions for Q1, Q3, and Q4), while multi-establishment firms are essentially unaffected. This indicates that single-establishment firms with less access to internal resources are the most likely to decrease hiring in response to UI tax increases. Among these employers, a \$100 maximum tax hike lowered hiring by 0.23pp, 0.22pp, and 0.41pp in quarters

1, 3, and 4 respectively. Finally, the last set of markers shows that younger firms are also more likely than older firms to reduce hiring in response to a UI tax increase, although the magnitudes are smaller than those for single-establishment firms.

Appendix Figure A.7 plots estimates for the separations share as well. While I estimated no overall effect on separations, the addition of interactions with firm characteristics suggest that single establishment firms exposed to a tax increase were actually less likely to experience separations in the first two quarters of the year, relative to multi-establishment firms. A potential mechanism could be a greater effort towards worker retention in the absence of hiring.

6 Incidence for Low-Earning Workers

To estimate UI tax incidence on earnings with greater precision, this section estimates regressions at the worker rather than firm level. Due to low UI tax bases (the median tax base in 2009 was only \$10,000), low-wage workers face higher effective tax rates and thus bear a larger relative tax burden than their higher-earning counterparts. They may also have less bargaining power and are more likely to work part-time. I identify existing workers of highly exposed firms, and estimate the impact of UI tax increases on their subsequent earnings trajectories. The added precision of a worker-level analysis allows for the identification of earnings impacts within this subgroup of interest.

Focusing on the main analysis sample of high-exposure firms, I construct a worker-level sample consisting of workers employed by the firm for at least a year, with annual earnings between \$5000 and \$24,000 in 2009.¹⁶ To ensure these workers make up a non-negligible share of the firms' employment, I also drop any firms that did not employ 10 or more of these low-wage workers as of 2009:Q3. This subsample includes approximately 549,000 low-

¹⁶Because the LEHD reports total earnings and not wage rates, this definition may pick up high-wage workers who work very few hours, although this should be fairly uncommon. Additionally, since UI taxes are based on earnings and not wages, part-time high-wage workers are equally costly to an employer from a payroll tax standpoint.

wage workers, employed by 13,000 employers (44% of the highly exposed employers in the main analysis sample). I estimate worker-level event study regressions, where event time is defined in the same way as Equation 1.

$$\begin{aligned}
 EarnGrowth_{i\,f\,s\,t} = & \alpha_f + \sum_{k=-8, k \neq -1}^8 \beta_k Tax\Delta_s(event = k) \\
 & + \delta_t + \rho \log(earn_{i\,f\,s, t-4}) + \gamma minwage_{st} + \epsilon_{i\,f\,s\,t} \quad (3)
 \end{aligned}$$

Here i indexes worker, f indexes employer, s indexes state, t indexes year-quarter, and k indexes the quarter relative to the policy change. Quarters more than two years before the policy change are assigned $event = -8$, and I follow workers for up to three years after the policy change, through the end of 2012. $Tax\Delta_s$ is defined for treatment states to equal the average maximum UI tax increase between 2009 and 2010-11 (in hundreds), and equal to zero for control states. The main outcome of interest, $EarnGrowth_{i\,f\,s\,t}$ is defined as the year-over-year earnings growth relative to the same quarter in the previous year. Thus inclusion in the sample requires the worker to have been employed at the firm a year earlier, limiting the analysis to a more stable subset of workers.

Figure 8 plots the estimates of the event time interactions with $Tax\Delta_s$; thus the magnitude of each estimate reflects the effect of a \$100 UI tax increase. In the first quarter after the tax increase ($event = 1$), each \$100 UI tax increase lowered earnings growth by 0.63 percentage points for continuing workers in treated firms, relative to their counterparts in control firms. This negative impact is small and precisely estimated, and does not last beyond the first year. This allows us to rule out negative earnings impacts beyond 1% in Q1 and 0.9% in Q3, for each \$100 increase. Since 1% of quarterly earnings equates to \$48 on average among this subsample, together these estimates imply a maximum pass-through rate of 90% (\$90 out of \$100) for existing low-wage workers.¹⁷ Therefore, even amongst a

¹⁷Meanwhile, estimating this regression on the subgroup of high-earning workers results in no perceivable treatment effect.

subgroup that bears the largest effective tax burden, we find evidence that short-run tax increases result in less than full tax incidence on the earnings of existing workers.

7 Conclusion

The employer-specific and time-varying aspects of UI payroll taxation provides researchers with extensive policy variation that can be used to understand how firms respond to temporary payroll tax changes. Given frequent proposals to cut employer payroll taxes to stimulate employment during recessions, there is debate over whether such tax cuts would have the intended effect or whether they would simply increase firm profits. I estimate significant negative employment responses to unexpected tax increases, which suggests that temporary payroll tax cuts are likely to have a positive impact on employment as well, especially for young and low-earning workers that face higher relative tax burdens, and for smaller employers with less access to financial resources.

In response to state maximum UI tax increases in the years following the Great Recession, UI tax burdens rose for employers with large layoff histories, with minimal evidence of tax incidence on workers. With little ability to pass tax increases on to workers, employers lowered hiring rates and employment growth, and this effect grew over time. Using the Q1 treatment estimates of hiring rates from Table 2, I calculate an implied labor demand elasticity of -1.1. This measure is obtained by dividing the Q1 DD coefficient estimate of -0.170 by 0.161, the first quarter effective UI tax increase estimated using the QCEW in Appendix Table A.1. An alternative measure can be calculated by dividing the triple-difference drop in Q4 employment growth of -0.74 by 0.31, the share of average annual earnings a \$100 tax increase is equivalent to; this equates to an end-of-year labor demand elasticity of -2.4. These elasticity estimates fall within the range of elasticities from the labor demand literature, although they are on the lower end of those from recent payroll tax studies.¹⁸

¹⁸Ku et al. (2020), estimates a labor demand elasticity of -3.6 using place-based payroll tax increases in Norway, while Benzarti and Harju (2021a) estimates elasticities of -2.9 to -4.16 in response to firm-specific

The tax incidence of unemployment insurance payroll taxes has been an understudied topic in the payroll taxation literature. The experience rating of employer-specific tax rates creates the benefit of reducing avoidable layoffs (particularly temporary layoffs) and the associated externalities imposed upon the UI system. However, the cyclical pattern in which tax rates are highest in years following economic recessions also slows hiring and employment growth during the recovery. More work is needed to understand the unintended consequences and inherent tradeoffs of US unemployment insurance's current design.

increases in Finland. And in the U.S. setting, Johnston (2021) estimates a UI tax labor demand elasticity of -4 while Guo (2021) estimates an elasticity of -1.1.

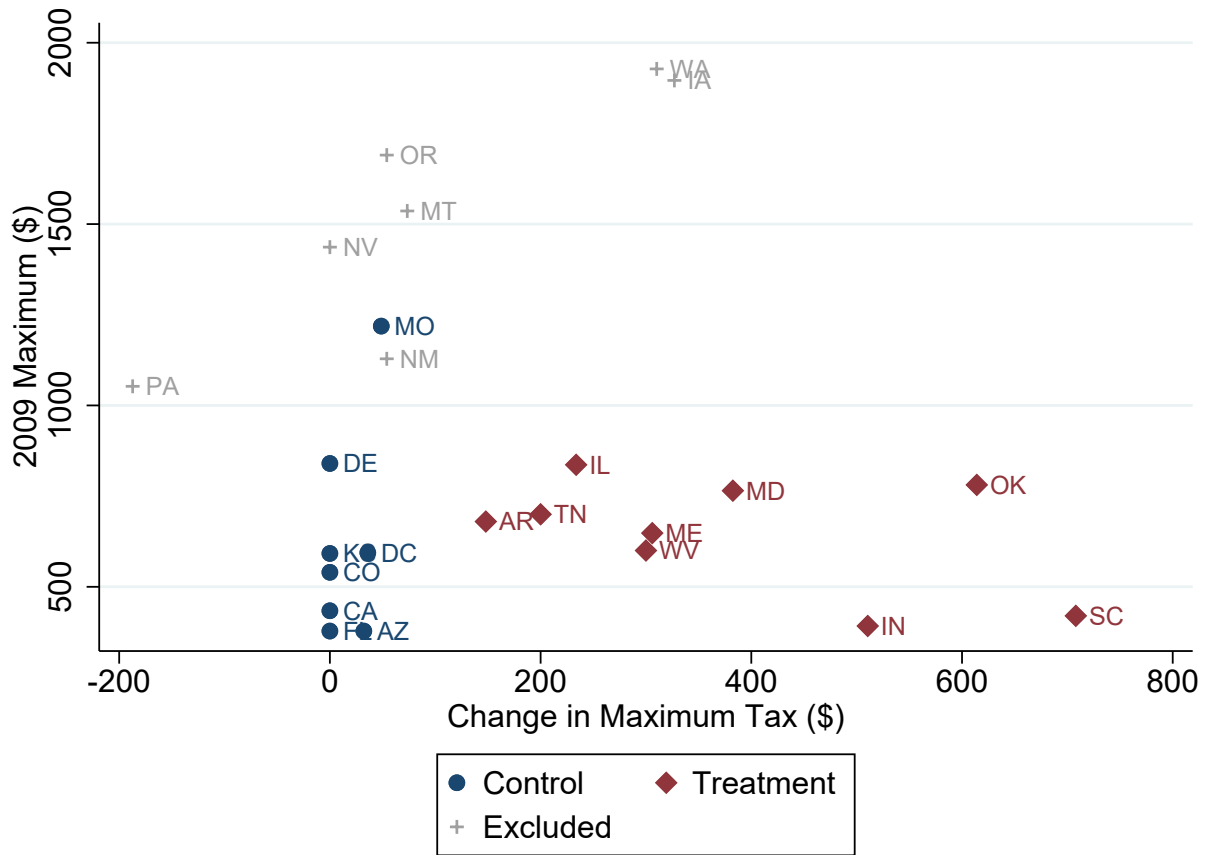
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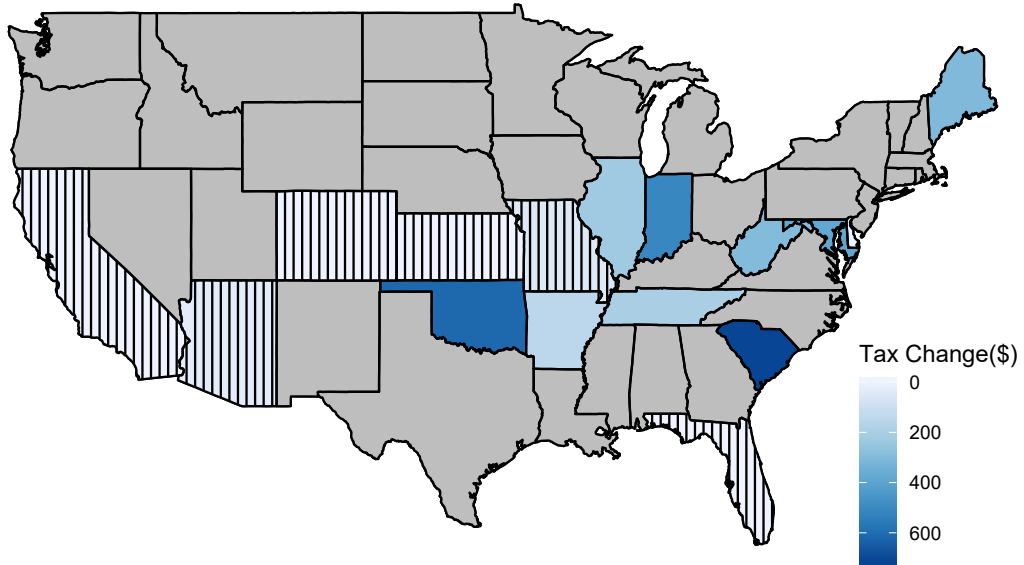
FIGURES

Figure 1: Maximum UI Tax Increase from 2009 to 2011



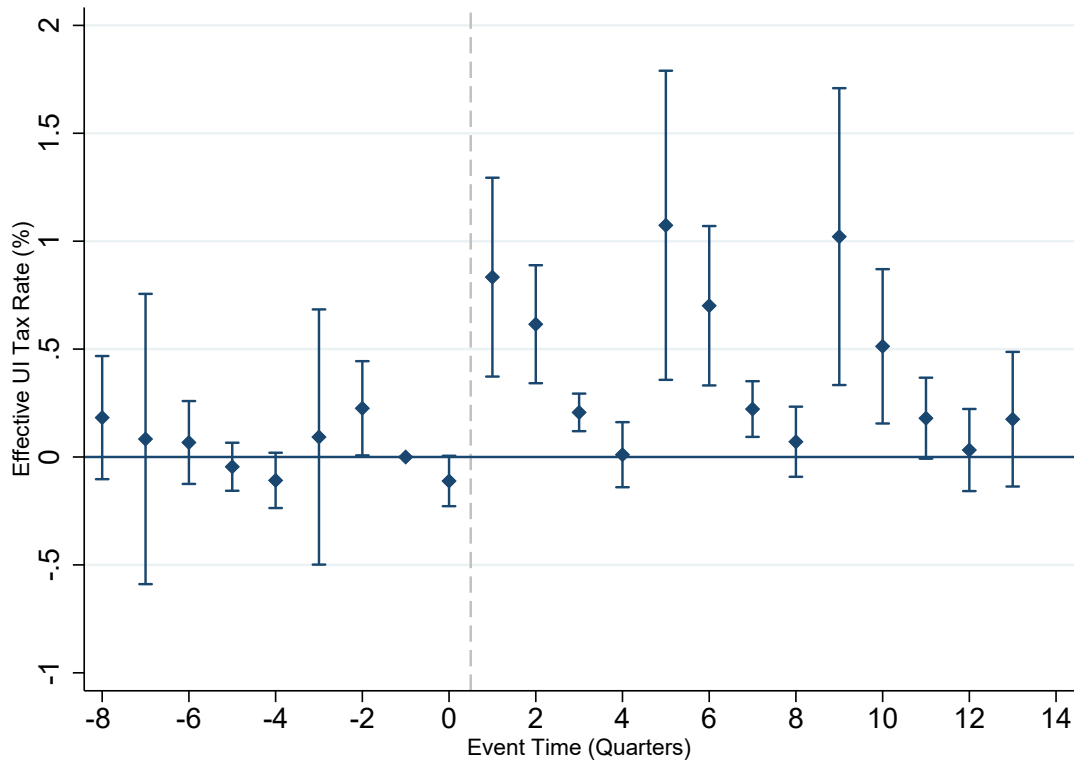
Source: *Significant Measures of State UI Tax Systems*. Maximum tax calculated as taxable wage base multiplied by maximum tax rate.

Figure 2: UI Tax Policy Variation



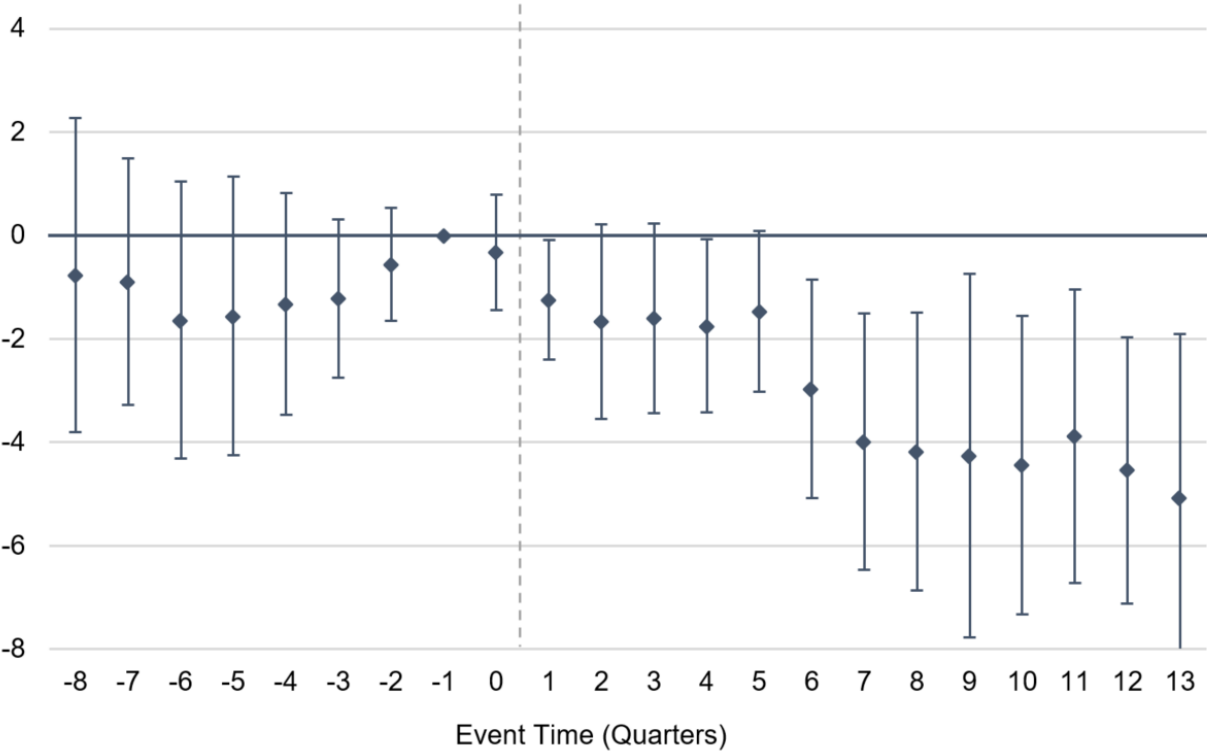
Source: *Significant Measures of State UI Tax Systems*. The 7 treatment states are shaded in blue corresponding to the tax change they experienced, and the 8 control states are shaded with vertical lines (and also include Delaware and District of Columbia). States in grey are excluded.

Figure 3: Effective UI Tax Rates (2008-2013)



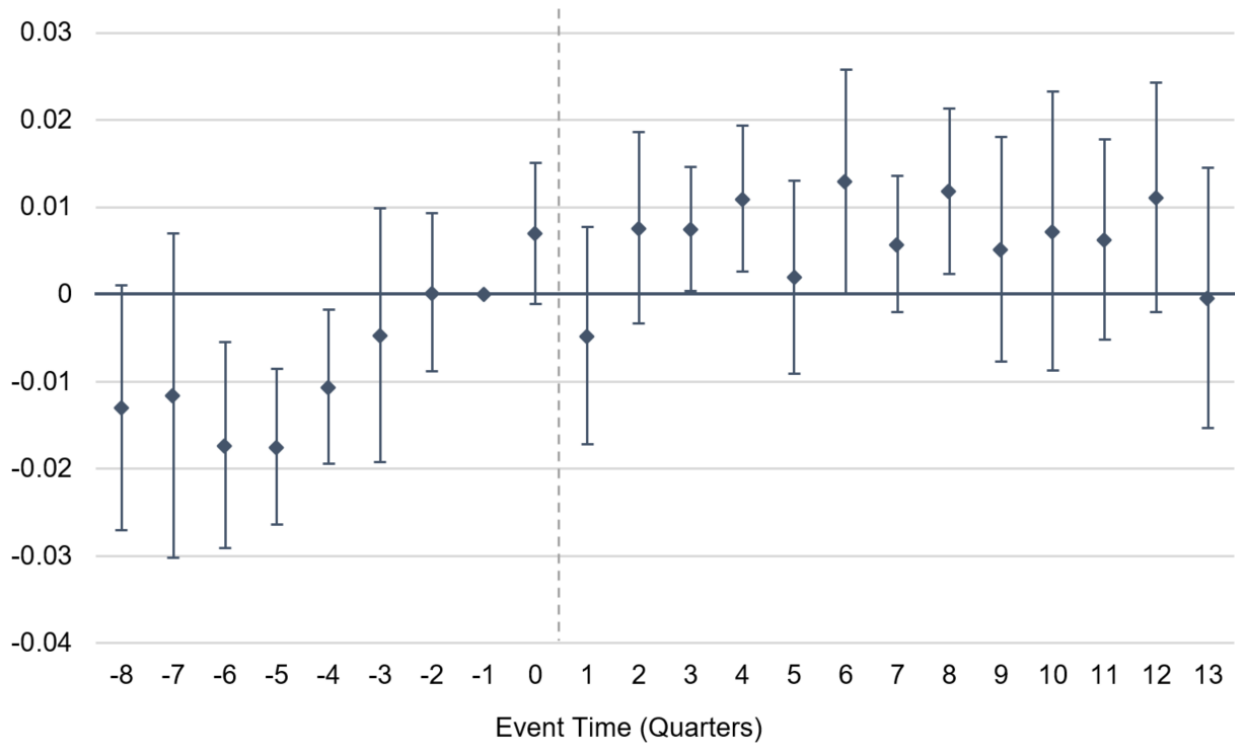
Source: *Quarterly Census of Employment and Wages*. (N = 4,062) Sample limited to ten 4-digit construction industries and years 2008 to 2013 (cells too small to meet disclosure requirements are missing). Effective tax rates calculated by dividing quarterly UI contributions by quarterly payroll. Event study estimated with state-industry and year-quarter fixed effects, and weighted by industry employment. Error bars denote 95% CI for standard errors clustered at state level.

Figure 4: Event Study of Year-Over-Year Employment Growth (2008-2013)



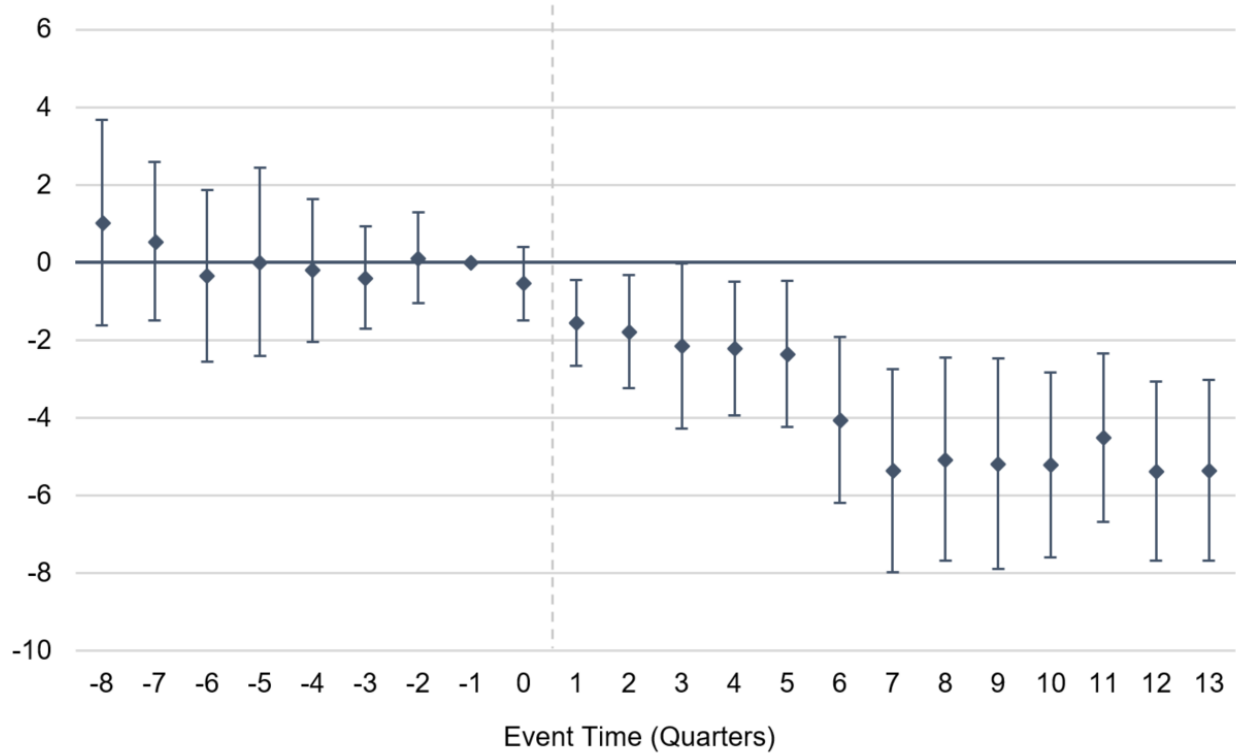
N = 651,000. Outcome variables denoted in percentage points. Also includes a control for state minimum wage, and includes SEIN and year-by-quarter-by N2 fixed effects. Error bars denote 95% CI for robust standard errors clustered at state level.

Figure 5: Event Study of Log Quarterly Earnings (2008-2013)



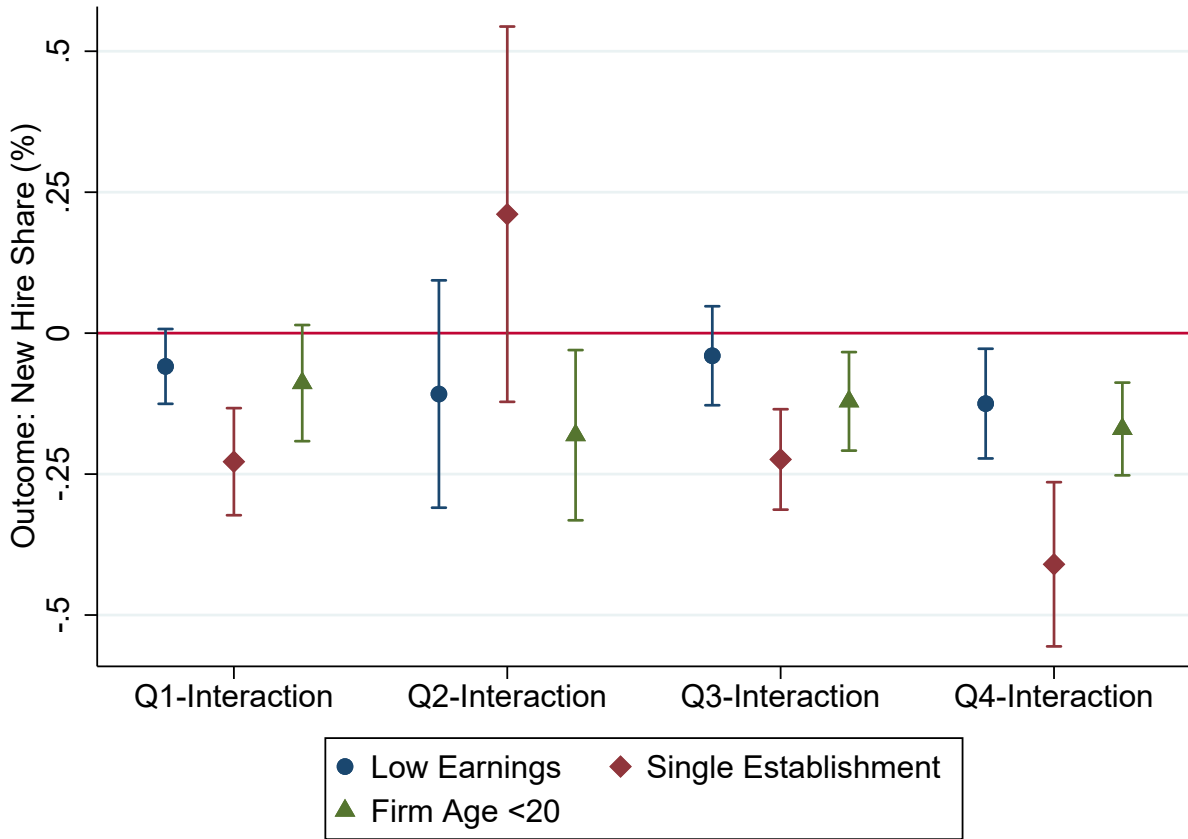
$N = 651,000$. Outcome variable is the mean quarterly earnings of stable workers (employed in both $t-1$ and $t+1$). Also includes a control for state minimum wage, and includes SEIN, state-by-calendar quarter, and year-by-quarter-by N2 fixed effects. Error bars denote 95% CI for robust standard errors clustered at state level.

Figure 6: Year-Over-Year Employment Growth - Triple Difference (2008-2013)



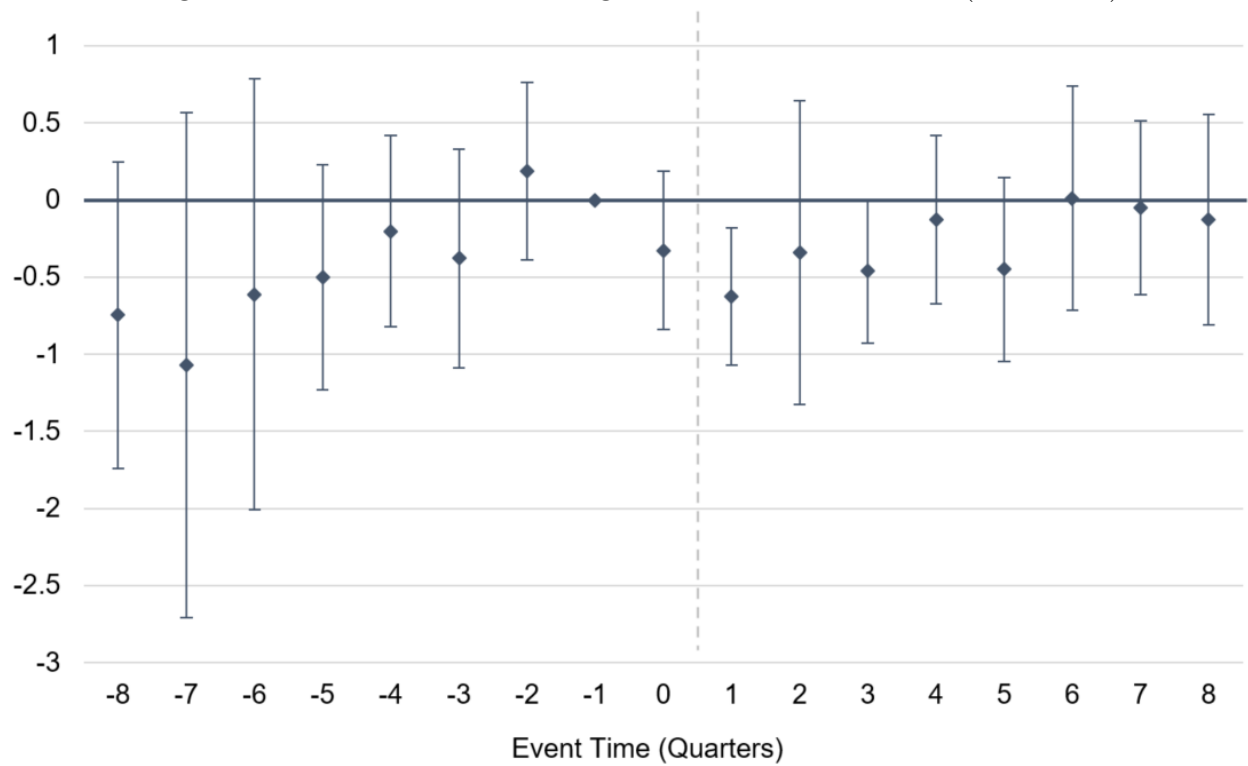
$N = 1,109,000$. Figure plots the estimates from treatment dummies interacted with event time and high-tax status. Regression also includes treatment interacted with event time, a control for state minimum wage, and SEIN and Year-Quarter-N2-High fixed effects. Error bars denote 95% CI for robust standard errors clustered at state level.

Figure 7: Heterogeneity in New Hire Share by Firm Type (2008-2013)



N = 651,000. Also includes a control for state minimum wage, and includes SEIN and Year-Quarter-N2 fixed effects. Error bars denote 95% CI for robust standard errors clustered at state level. The share of employers with median earnings below \$6000 equals 0.29. The share that are single-establishment firms equals 0.69. The share with firm age under 20 equals 0.22.

Figure 8: Year-Over-Year Earnings Growth - Worker Level (2008-2012)



$N = 4,954,000$. Includes 549,000 unique workers across 13,000 unique employers. Figure plots the estimates from taxchange measure interacted with event time. Regression also includes a control for state minimum wage, lagged log earnings, and SEIN, Year-Quarter, and state-by-calendar quarter fixed effects. Error bars denote 95% CI for robust standard errors clustered at state level.

TABLES

Table 1: Summary Statistics (2009:Q3)

	Control		Treatment	
	Mean	SD	Mean	SD
Employment	66.74	72.28	67.29	70
Cumulative Layoff Share 2006-07	0.5668	0.1689	0.5668	0.1679
Cumulative Layoff Share 2008-09	0.5037	0.1473	0.5065	0.1493
Average Annual Earnings	36,900	27,990	32,350	23,340
Share of Workers Earning < \$5000	0.1518	0.1647	0.1572	0.1619
Construction Sector	0.245		0.2282	
<i>N</i>	18000		11500	

Employer-level observations from 3rd quarter of 2009. Restricted to high exposure employers, with employment between 20 and 500 workers in 2009:Q3. Control states include AZ, CA, CO, DE, DC, FL, NE, and MO. Treatment states include AR, IL, IN, ME, MD, OK, SC, TN, and WV.

Table 2: Employment Outcomes (2008–2013)

	Employment Growth		New Hire Share		Separation Share	
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Δ *Q1 (\$100's)	-0.142 (0.129)	-0.469** (0.177)	-0.170** (0.0723)	-0.163** (0.0672)	-0.0968 (0.0832)	-0.0350 (0.0583)
Tax Δ *Q2 (\$100's)	-0.287** (0.120)	-0.571** (0.205)	0.240 (0.193)	0.224 (0.168)	-0.0746 (0.0897)	-0.0494 (0.0757)
Tax Δ *Q3 (\$100's)	-0.318** (0.115)	-0.674*** (0.194)	-0.104 (0.0752)	-0.0962 (0.0756)	0.0514 (0.0652)	0.102** (0.0410)
Tax Δ *Q4 (\$100's)	-0.431*** (0.127)	-0.759*** (0.171)	-0.170* (0.0954)	-0.178* (0.0879)	0.0587 (0.169)	0.110 (0.147)
Minimum Wage	-0.912 (0.696)	-0.792 (0.781)	-0.330* (0.182)	-0.196 (0.201)	0.107 (0.208)	0.190 (0.200)
R^2	0.193	0.254	0.427	0.529	0.405	0.521
Mean of Dep Variable	-4.201	0.349	13.40	14.98	14.74	15.87
Weighting		YES		YES		YES
N	651000	651000	651000	651000	651000	651000

Regressions include SEIN and year-quarter-N2 fixed effects. Outcomes denoted in percentage points, and even columns are weighted by employment. Robust standard errors clustered at state level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Log Quarterly Earnings (2008–2013)

	Stable Workers		All Workers		Share with Raise	
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Δ *Q1 (\$100's)	-0.00113 (0.00190)	-0.000855 (0.00123)	0.000158 (0.00109)	-0.0000151 (0.000735)	-0.419*** (0.113)	-0.398*** (0.0805)
Tax Δ *Q2 (\$100's)	0.00380*** (0.000938)	0.00331*** (0.000754)	0.00209** (0.000869)	0.00214*** (0.000706)	0.111 (0.0929)	0.137 (0.157)
Tax Δ *Q3 (\$100's)	0.00353*** (0.00101)	0.00334*** (0.00101)	0.00309*** (0.000864)	0.00287*** (0.000729)	-0.00947 (0.0985)	-0.0320 (0.0922)
Tax Δ *Q4 (\$100's)	0.00494*** (0.00130)	0.00432*** (0.000902)	0.00484*** (0.00105)	0.00471*** (0.000824)	0.0281 (0.139)	-0.0218 (0.132)
Minimum Wage	0.0152*** (0.00279)	0.0121*** (0.00276)	0.0181*** (0.00363)	0.0126*** (0.00316)	0.170 (0.227)	0.260 (0.229)
R^2	0.869	0.905	0.884	0.921	0.191	0.193
Mean of Dep Var	9.232	9.240	9.067	9.057	31.0	31.7
Weighting		YES		YES		YES
N	651000	651000	651000	651000	651000	651000

Regressions include SEIN and year-quarter-N2 fixed effects. Share with Raise equals the percentage of continuing workers who received a raise relative to the previous two quarters. Even columns are weighted by employment. Robust standard errors clustered at state level in parentheses. ** $p < 0.05$, *** $p < 0.01$

Table 4: Triple Difference (2008–2013)

	Emp Growth (1)	New Hire Share (2)	Sep Share (3)	Log Earnings (4)	Share with Raise (5)
Tax Δ *Q1 (\$100's)	0.330** (0.116)	-0.0286* (0.0143)	-0.0385 (0.0373)	-0.000610 (0.00147)	-0.247** (0.108)
Tax Δ *Q2 (\$100's)	0.303** (0.127)	0.0739 (0.0660)	0.0305 (0.0624)	0.00313 (0.00274)	0.130 (0.489)
Tax Δ *Q3 (\$100's)	0.315** (0.135)	0.0382 (0.0289)	-0.0480 (0.0485)	-0.000553 (0.00283)	0.0416 (0.219)
Tax Δ *Q4 (\$100's)	0.284* (0.140)	0.0380 (0.0231)	-0.0719*** (0.0240)	0.00291** (0.00128)	-0.00031 (0.149)
Tax Δ *Q1 \times High	-0.503** (0.190)	-0.155** (0.0654)	-0.0544 (0.0508)	-0.000246 (0.00172)	-0.171 (0.114)
Tax Δ *Q2 \times High	-0.622*** (0.212)	0.155 (0.156)	-0.0995 (0.0574)	0.00112 (0.00261)	0.0285 (0.506)
Tax Δ *Q3 \times High	-0.664*** (0.222)	-0.151** (0.0596)	0.110 (0.0797)	0.00433 (0.00326)	-0.0328 (0.278)
Tax Δ *Q4 \times High	-0.743*** (0.234)	-0.220** (0.101)	0.135 (0.164)	0.00223 (0.00172)	0.0103 (0.0873)
Minimum Wage	-0.383 (0.336)	-0.170 (0.118)	-0.00830 (0.144)	0.0108*** (0.00271)	0.00649 (0.262)
R^2	0.203	0.476	0.482	0.901	0.178
Mean of Dep Var	-1.373	10.61	11.28	9.323	31.7
N	1109000	1109000	1109000	1109000	1109000

Regressions include SEIN, year-quarter-N2, and year-quarter-High fixed effects. The necessary DDD interactions are either absorbed by the SEIN fixed effects or the year-quarter-High fixed effects. Robust standard errors clustered at state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

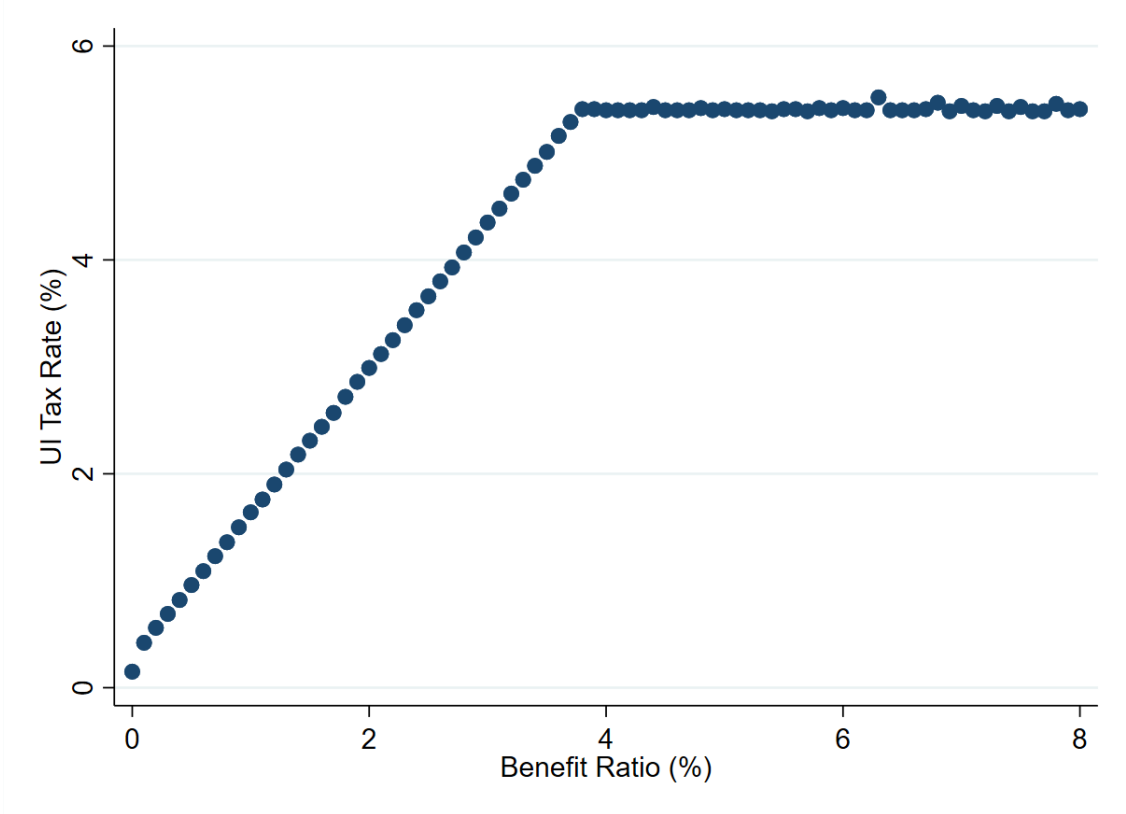
Table 5: Heterogeneity by Worker Characteristics (2008–2013)

	Employment Growth:					
	Low-Earn Hire Share		Age under 25		High School or Less	
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Δ *Q1 (\$100's)	-0.102** (0.0460)	-0.0295** (0.0126)	-0.218 (0.246)	0.379 (0.228)	-0.0956 (0.133)	0.371** (0.135)
Tax Δ *Q2 (\$100's)	0.0929 (0.0777)	0.0414 (0.0468)	-0.325 (0.254)	0.295 (0.287)	-0.249* (0.122)	0.309* (0.154)
Tax Δ *Q3 (\$100's)	-0.0701 (0.0521)	-0.0524* (0.0287)	-0.510* (0.287)	0.208 (0.242)	-0.288** (0.108)	0.357** (0.149)
Tax Δ *Q4 (\$100's)	-0.0822 (0.0543)	0.00105 (0.0168)	-0.655* (0.328)	0.222 (0.265)	-0.393*** (0.132)	0.299** (0.139)
Tax Δ *Q1 \times High		-0.0796* (0.0397)		-0.618* (0.293)		-0.525** (0.233)
Tax Δ *Q2 \times High		0.0490 (0.0534)		-0.659** (0.290)		-0.617** (0.274)
Tax Δ *Q3 \times High		-0.212 (0.0455)		-0.750** (0.326)		-0.705** (0.253)
Tax Δ *Q4 \times High		-0.0890 (0.0517)		-0.904** (0.387)		-0.748*** (0.252)
Minimum Wage	-0.237* (0.120)	-0.0179* (0.0859)	0.0461 (1.383)	0.598 (0.872)	-1.589* (0.778)	-0.801** (0.369)
R^2	0.515	0.543	0.101	0.086	0.156	0.167
Mean of Dep Variable	6.478	5.270	-11.44	-7.296	-2.712	0.419
N	651000	1109000	651000	1109000	651000	1109000

Dependent variables are multiplied by 100. Odd columns estimated on DD sample, and even columns estimated on DDD sample. Regressions include SEIN, year-quarter-N2, and year-quarter-High fixed effects. The necessary DDD interactions are either absorbed by the SEIN fixed effects or the year-quarter-High fixed effects. Robust standard errors clustered at state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

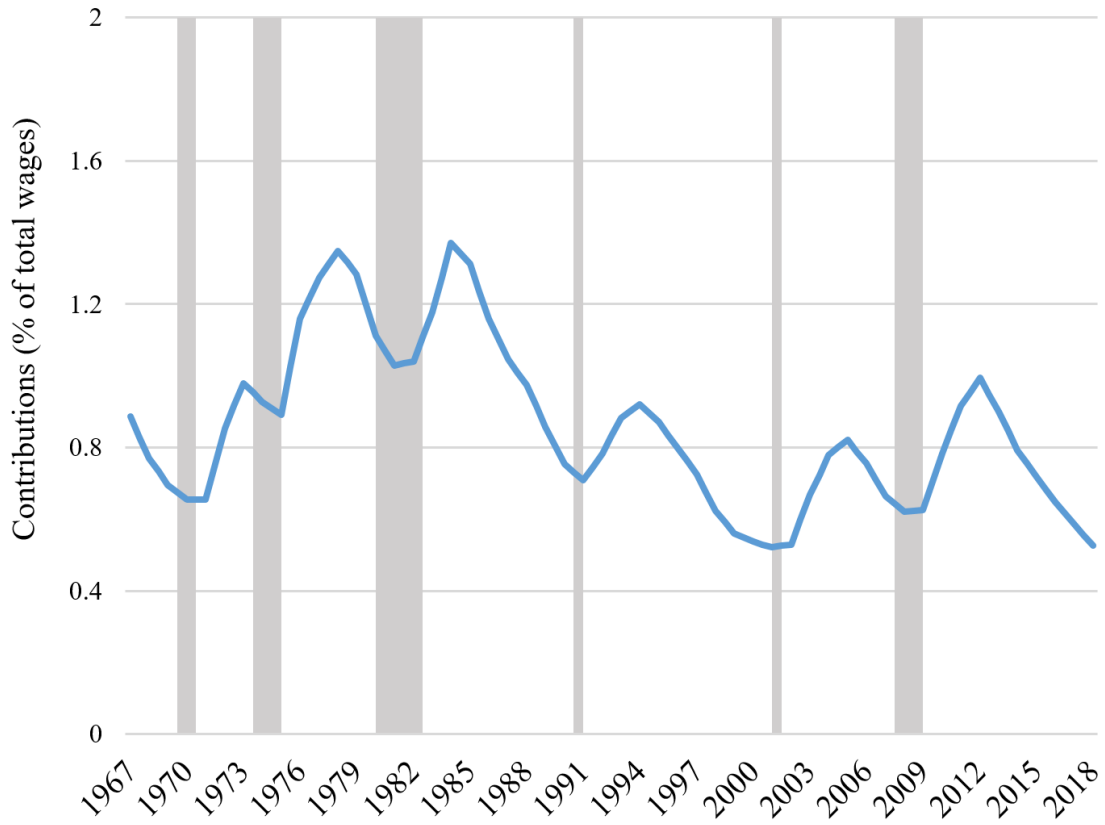
A APPENDIX TABLES AND FIGURES

Figure A.1: Empirical UI Tax Schedule for Florida (2008)



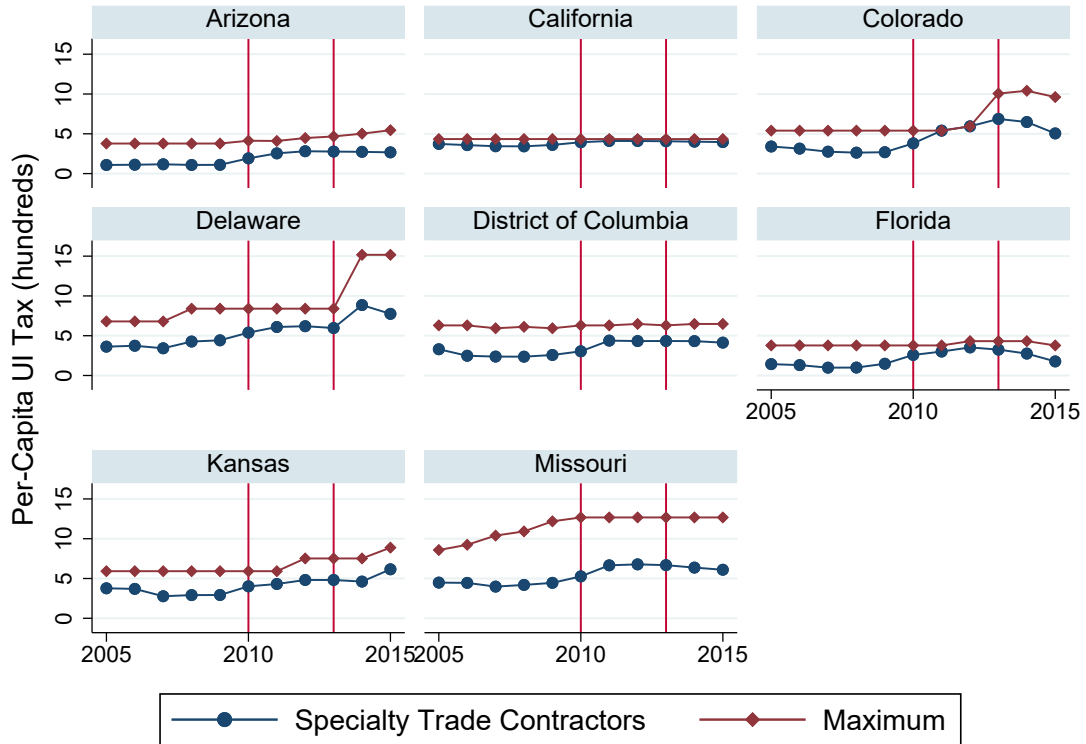
Source: US Dept of Labor ETA 204 Experience Rating Report. The Benefit Ratio is a measure of the employer's layoff experience in the last three years.

Figure A.2: Total UI Contributions, as % of total wages (1967 - 2018)



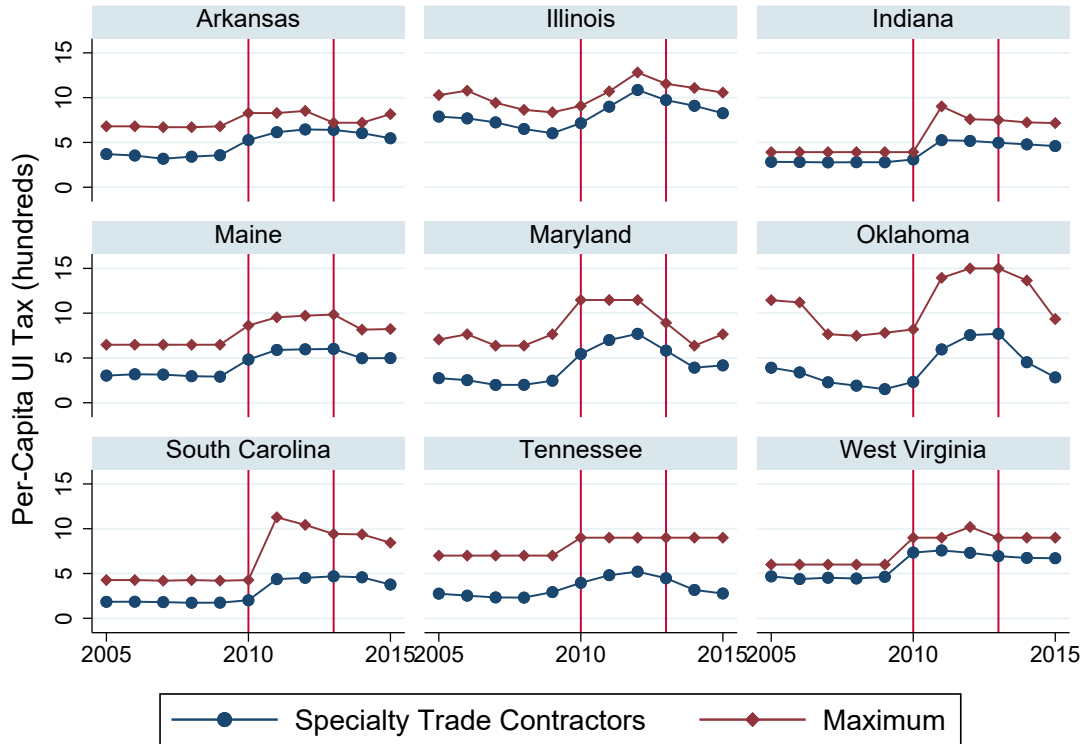
Source: US Dept of Labor Unemployment Insurance Chartbook.
Shaded regions denote US recession years, as defined by NBER.

Figure A.3: State UI Tax Schedules - Control



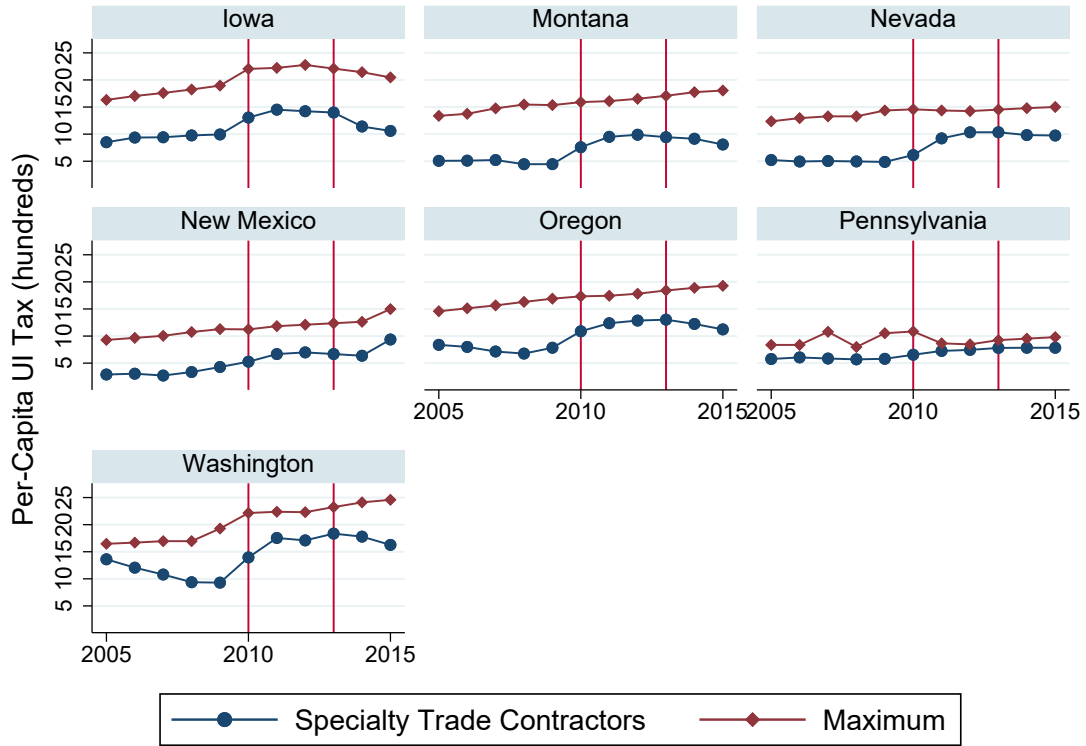
Compares average UI taxes paid and maximum UI taxes. Average taxes for specialty trade contractors obtained from Quarterly Census of Employment and Wages. Specialty trade contractors (NAICS 238) was chosen as an industry whose employers often face rates close to the maximum, to proxy for high exposure firms.

Figure A.4: State UI Tax Schedules - Treatment



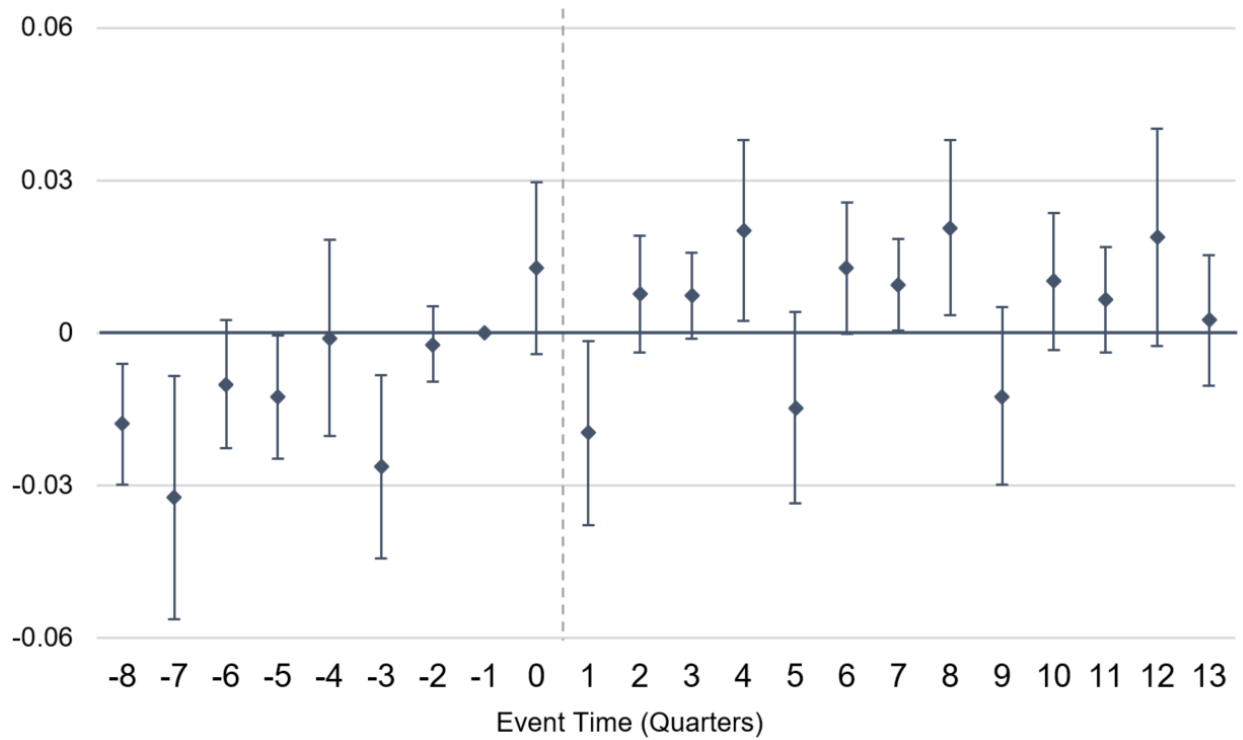
Compares average UI taxes paid and maximum UI taxes. Average taxes for specialty trade contractors obtained from Quarterly Census of Employment and Wages. Specialty trade contractors (NAICS 238) was chosen as an industry whose employers often face rates close to the maximum, to proxy for high exposure firms.

Figure A.5: State UI Tax Schedules - Excluded



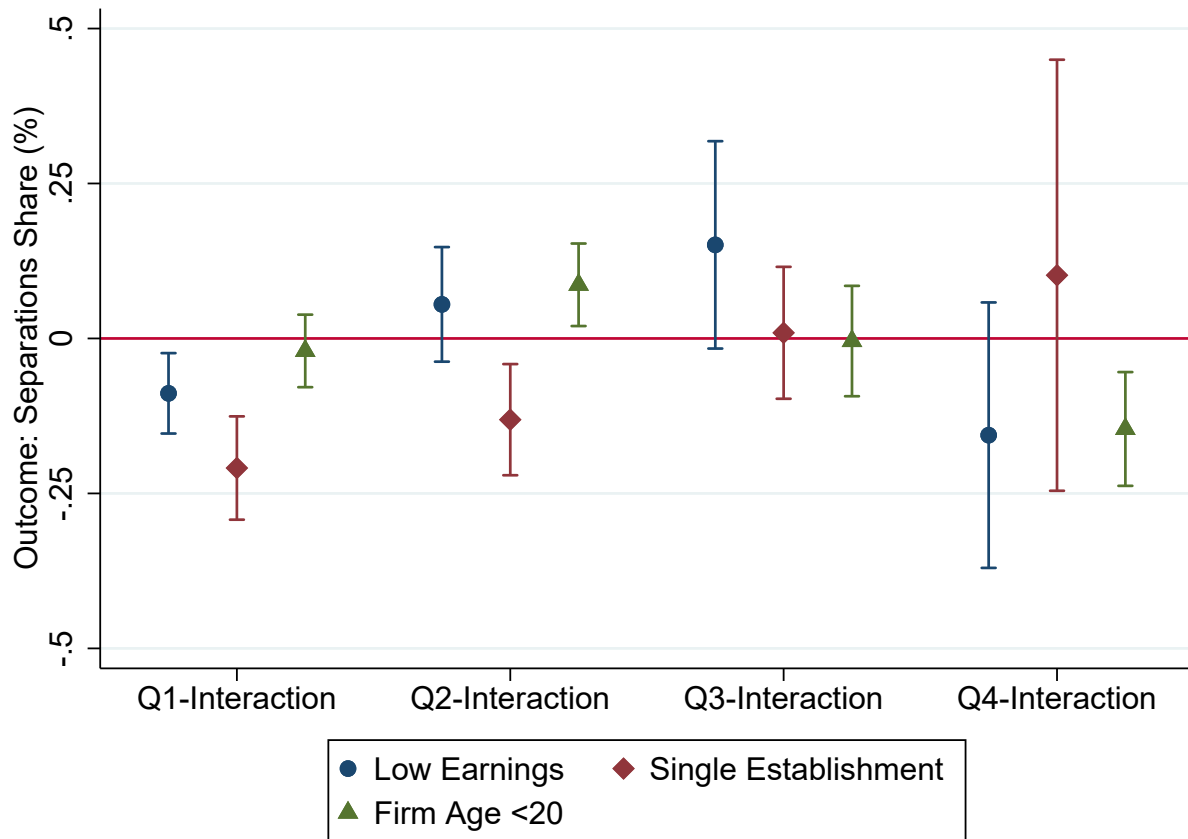
Compares average UI taxes paid and maximum UI taxes. Average taxes for specialty trade contractors obtained from Quarterly Census of Employment and Wages. Specialty trade contractors (NAICS 238) was chosen as an industry whose employers often face rates close to the maximum, to proxy for high exposure firms.

Figure A.6: Event Study of Log Quarterly Earnings (2008-2013)



$N = 651,000$. Outcome variable is the mean quarterly earnings of stable workers (employed in both $t-1$ and $t+1$). Also includes a control for state minimum wage, and includes SEIN and year-by-quarter-by N2 fixed effects. Error bars denote 95% CI for robust standard errors clustered at state level.

Figure A.7: Heterogeneity in Separations Share by Firm Type (2008-2013)



N = 651,000. Also includes a control for state minimum wage, and includes SEIN and Year-Quarter-N2 fixed effects. Error bars denote 95% CI for robust standard errors clustered at state level. The share of employers with median earnings below \$6000 equals 0.29. The share that are single-establishment firms equals 0.69. The share with firm age under 20 equals 0.22.

Table A.1: Average Industry Effective UI Tax Rate (2008–2013)

	Unweighted (1)	Employment-Weighted (2)
Tax Δ^* Q1 (\$100's)	0.0996* (0.0498)	0.161*** (0.0646)
Tax Δ^* Q2 (\$100's)	0.138*** (0.0242)	0.136*** (0.0270)
Tax Δ^* Q3 (\$100's)	0.0752*** (0.0209)	0.0490* (0.0254)
Tax Δ^* Q4 (\$100's)	0.0536* (0.0280)	0.0207 (0.0335)
Minimum Wage	0.0865 (0.104)	0.206 (0.0987)
R^2	0.881	0.914
Mean of Dependent Variable	1.382	1.217
N	4062	4062

Uses average industry data from the public-use QCEW. Sample limited to ten 4-digit construction industries, and observations are at the industry by state by quarter level (cells too small to meet disclosure requirements are missing). Effective tax rates calculated by dividing quarterly UI contributions by quarterly payroll. Regressions include state-industry and year-quarter fixed effects. Robust standard errors clustered at state level in parentheses. ** $p < 0.05$, *** $p < 0.01$

Table A.2: Triple Difference Controlling for State-Time FEs (2008–2013)

	Log Earn (1)	Growth (2)	New Hires (3)	Separations (4)	Gr: Under 25 (5)	Gr: HS or Less (6)
Tax $\Delta^*Q1 \times$ High	-0.00292 (0.00320)	-0.528** (0.190)	-0.154* (0.0739)	-0.0456 (0.0601)	-0.665** (0.284)	-0.551** (0.231)
Tax $\Delta^*Q2 \times$ High	0.00184 (0.00322)	-0.642*** (0.215)	0.196 (0.185)	-0.0838 (0.0528)	-0.717** (0.277)	-0.640** (0.275)
Tax $\Delta^*Q3 \times$ High	0.00511** (0.00239)	-0.672*** (0.218)	-0.131** (0.0504)	0.130** (0.0607)	-0.776** (0.301)	-0.723** (0.249)
Tax $\Delta^*Q4 \times$ High	0.00256 (0.00219)	-0.737*** (0.227)	0.199* (0.102)	0.195 (0.181)	-0.939** (0.370)	-0.754*** (0.244)
R^2	0.914	0.205	0.479	0.485	0.087	0.168
Mean of Dep Var	9.096	-1.373	10.61	11.28	-7.296	0.419
N	1109000	1109000	1109000	1109000	1109000	1109000

Outcomes in percentage points. Regressions include SEIN, year-quarter-state, year-quarter-N2, and year-quarter-High fixed effects. The necessary DDD interactions are either absorbed by the SEIN, year-quarter-state, or year-quarter-High fixed effects. Robust standard errors clustered at state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$