UNEMPLOYMENT INSURANCE TAXES AND LABOR DEMAND: QUASI-EXPERIMENTAL EVIDENCE FROM ADMINISTRATIVE DATA

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Andrew C. Johnston[1]

ABSTRACT

States raise tax rates on firms that lay off workers, a practice that pays for unemployment (UI) benefits. Since rates respond to layoffs, taxes are highest for troubled firms after downturns, potentially reducing labor demand during recoveries. I assess the effect of UI tax hikes among distressed firms leveraging a kink in the tax formula with full-population records from Florida. Higher taxes reduce hiring and employment, with larger effects for firms in duress. In contrast, I find little evidence that penalties deter layoffs. These responses are consistent with a model of cash-constrained firms and suggest unanticipated costs of UI provision.

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[1]Johnston: Department of Economics, University of California, Merced; email: acjohnston@ucmerced.edu. I am grateful to many, including Santosh Anagol, Patricia Anderson, Peter Blair, Leah Boustan, Julie Berry Cullen, Gordon Dahl, Mark Duggan, Pascaline Dupas, Fernando Ferreira, Alexander Gelber, Todd Gormley, Jessie Handbury, Joseph Harrington, Ann Harrison, Robert Jensen, Carla Johnston, Judd Kessler, Camille Landais, Corinne Low, Alexandre Mas, Bruce Meyer, Olivia S. Mitchell, Alex Rees-Jones, Michael Sinkinson, Kent Smetters, Jeremy Tobacman, Arthur van Benthem, Christopher Walters, Stephan Woodbury, and Gregory Wright, with particular debt owed to Alexandre Mas, Mark Duggan, Olivia S. Mitchell, Alex Rees-Jones, and Robert Jensen. Data were provided by the Florida Departments of Labor. Financial support from the U.S. Department of Labor's Employment and Training Administration and the National Bureau of Economic Research is gratefully acknowledged. Views expressed are those of the author and should not be attributed to The University of California.

"If a company wants to fire their workers…we will apply a thirty-five percent tax."

~Donald Trump, *campaign rally*

"I might consider adding a new salesperson, [but if] I need to lay off this person, I will likely end up paying out $5,000, $10,000, or even $20,000 in unemployment taxes."

~Jay Goltz, *New York Times*

I. Introduction

Employers pay a dynamic payroll tax to finance unemployment insurance. Under this unique tax regime, each firm has its own tax rate that rises in response to layoffs and falls when firms avoid them. Because tax rates are linked to layoffs, firms mechanically face higher payroll taxes when unemployment is high and troubled firms bear the largest tax increases. To protect already distressed firms from crushing tax rates, each state sets a limit on how high it allows a firm's taxes to rise, limits that range by almost an order of magnitude from state to state.[2] These large differences likely reflect disagreement among policymakers regarding the impact of mechanical tax hikes on distressed firms, an unintended consequence of experience rating I call the *overhang effect*.[3]

Measuring this effect empirically is challenging. First, large micro data combining UI tax rates and firm behavior are not publicly available. Second, even when available, tax rates are explicitly endogenous, making it difficult to separate the causal effect of taxes from unobserved, confounding factors. In this paper, I employ new, full-population micro data on all workers and firms in Florida, linking firm behavior (hiring, layoffs, employment, wages, and exit) to tax information, and I exploit quasi-experimental variation to disentangle the effect of tax rates from other factors.

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[2] The maximum per-employee tax ranges from $440 in Arizona, California, and Florida, to $3,100 (seven times larger) in North Dakota, Minnesota, and Virginia.

[3] In this setting, tax increases represent a consequence of layoffs from a previous period. Thus, the tax hike is an "overhang" from the layoffs associated with a negative shock.

Specifically, I leverage a kink in Florida's UI tax formula to isolate variation in the tax rate that is independent of firm behavior, conditional on a few controls (Card et al. 2015). I find that UI tax increases reduce hiring and employment, with no effect on layoffs, separation, wages, or exit. A one percentage point increase in UI tax rates reduces yearly firm hiring by 0.7 hires (2.8 percent), lowering yearly employment by 0.8 employees (1.5 percent). Importantly, the estimates remain when controlling for firm fixed-effects, which suggests the estimates are not generated by spurious firm differences around the kink. Moreover, predetermined covariates evolve smoothly across the kink and no bunching exists, recommending the natural experiment. Precise manipulation is essentially impossible since firms cannot control the costs of former employees, charges increase at discrete weekly increments, and the tax schedule changes slightly from year to year, all complicating a firm's ability to fine tune their tax rate. Without spillovers, the estimates suggest UI tax increases during the recent recovery accounted for twelve percent of unemployment in 2010 and 2011. Various specification checks, non-parametric evaluations, and permutation exercises confirm the results.

The firm response is large, consistent with a labor-demand elasticity of 4.1 with CIs ruling out elasticities smaller than 2.1, similar to the elasticities reported in Anderson and Meyer (2000).[4] Labor demand may be more responsive for firms in duress. I evaluate this hypothesis by comparing the sensitivity of firms in more and less distressed periods and industries, and I find firms are significantly more responsive to tax rates during the recessionary period and in industries that had low cash-on-hand. Because distressed firms systematically face higher tax increases under UI, the tax regime likely has larger employment effects than a broader-based payroll tax.

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[4] Estimated elasticities span a wide range but cluster around 0.5 (Katz 1995; Lichter, Peichl and Siegloch 2015).

It is natural to wonder whether there is a counterbalancing benefit of a tax system which penalizes layoffs: Do penalties encourage firms to retain their workers, possibly increasing employment on net? I measure how layoffs respond to the penalties they face depending on their placement on the tax schedule. Using several natural experiments, I find little evidence that tax penalties discourage firms from layoffs whatsoever.

This presents a puzzle. If firms are very responsive to payroll tax increases, why do they not simply avoid them by cancelling layoffs? A model of cash-constrained firms reconciles these seemingly disjointed responses. That is, if firms need non-negative profits to cover their costs in each period, a future penalty cannot prevent layoffs needed to survive. Similarly, if a firm is cash-constrained when it receives a tax bill, the firm is forced to reduce costs.

Though an enormous literature surrounds UI benefits,[5] little has been done to assess the impacts of UI taxes, an equal and opposite arm of the UI program. UI tax research has focused on the consequences of experience-rating for temporary layoffs, a relatively small piece of the labor market and only five percent of layoffs in my data (Feldstein 1976; Topel 1983; Wolcowitz 1984; Card and Levine 1995). Anderson and Meyer (1997, 2000) demonstrate the influence of UI taxes are far broader. The authors use panels covering a sample of firms in the early 1980s to relate endogenous tax adjustments to wages and employment, finding large correlations; in 2000, they use the introduction of experience rating in 1985 in Washington state to estimate the effect of rates on wages and employment, again suggesting large consequence. The study I present offers complementary evidence using a design-based approach, contemporary records, large data and long panels, visibility of hiring and layoffs, and variation that is

[5] See, for instance, Meyer (1989), Landais, Michaillat, and Saez (2010), Schmieder and von Wachter (2012), Kroft et al. (2012), Kroft, Lange, and Notowidigdo (2013), Hagerdorn et al. (2015), Farber, Rothstein, and Valletta (2015), Card et al. (2015), and Johnston and Mas (2017).

more likely to reflect the effect of UI taxation.[6] Moreover, the conceptual framework I provide offers a rationale for the large firm responses that Anderson, Meyer, and I document.

The results speak to policy. First, UI taxes track the business cycle, which may diminish the automatic stabilizing influence of UI. Second, the results contribute to the optimal UI literature which assumes taxes reduce wages and impose no economic loss (Baily 1978; Chetty 2006). The evidence here suggests that taxes reduce employment and diminish welfare (Gruber 1994), altering the calculus of optimal UI.

II. The Background of Unemployment Insurance Taxation

The U.S. federal government mandates that each state administer a UI program, under which laid-off workers receive benefits. The unemployed receive a weekly payment replacing approximately half of their earnings for up to six months in normal times and up to two years when unemployment is high. To finance benefits, firms pay a dynamic payroll tax. That is, a firm's tax rate increases with layoffs, and the tax rate falls when a firm generates fewer costs, with tax rates updating each year.

To estimate the overhang effect, I leverage a kink in Florida's tax formula, which assigns each firm a tax rate based on its *benefit ratio.* The benefit ratio is a statistic that reflects the cost of benefits drawn (B) by a firm's former employees over the past three years divided by the firm's total taxable payroll (W) during the same period:

$$BR_t = \frac{\sum_{j=1}^{3} B_{t-j}}{\sum_{j=1}^{3} W_{t-j}},$$

The state uses each firm's benefit ratio to calculate its tax rate, $\tau_t = \min(\alpha + \lambda \mathrm{BR}_t, 5.4)$. Parameters α and λ are chosen by the state and vary slightly from year

[6] Most of the literature on payroll taxes studies the influence of stable, constant-rate taxes. In contrast, UI tax rates change from year to year and are more likely to increase for firms in dire straits.

to year. Tax rates rise with the benefit ratio until the rate reaches the maximum, 5.4 percent, which generates a kink in the tax rate as a function of the benefit ratio as seen in Online Appendix Figure I. In the empirical section, I measure corresponding kinks in firm behavior to elucidate the impact of this tax variation. Many firms reside around Florida's kink due to the state's low maximum rate and large population, which provide statistical power. To evaluate whether firms are discouraged from layoffs by tax penalties, I exploit the fact that the penalties firms face differ depending on the extent to which the maximum rate shields some firms more than others from tax increases in a narrow region around the point where the maximum rate begins.

More broadly, UI taxes are unique in important ways. Unlike other tax instruments, taxes for UI may exaggerate the business cycle, since rates rise in response to downturns.[7] Figure I demonstrates how unemployment and UI tax payments vary over the business cycle. The tax bill follows the pattern of the unemployment rate, lagging it by eight quarters. After the 2001 recession, the average per-worker tax bill increased by 40 percent. After the 2007 recession, the per-worker tax bill increased by 70 percent. Because these increases represent averages, they obscure enormous heterogeneity in which distressed firms confronted much larger increases while stable firms faced no change. In states with the highest experience rating, distressed firms faced taxes up to seven times the peak tax rate seen in Figure I.

III. CONCEPTUAL FRAMEWORK

A stable payroll tax rate reduces wages with little impact on employment in equilibrium (Britain 1971; Hamermesh 1996; Gruber 1997; Chetty et al. 2011). Unlike traditional payroll taxes, UI tax rates change each year. Therefore, UI tax hikes may reduce employment rather than wages since, in the short run, wages are

[7] A progressive income tax, by contrast, automatically reduces average marginal tax rates during recessions and increases rates during periods of growth.

rigid (Kaur 2014). I analyze a labor-demand model featuring negative demand shocks and UI experience rating to explore the effects of tax penalties in potentially deterring layoffs during a negative shock and hampering hiring during the subsequent recovery. For simplicity, workers are homogeneous, prices are exogenous, and I assume that firms do not foresee or insure negative demand shocks.[8] Consider a discrete-time model in which firms seek to maximize profits over several periods. Thus, the firm's problem is to maximize the sum of a sequence of Π_t up to time T:

$$\Pi_t = p_t f(N_t) - N_t - c - \tau \frac{L_{t-1}}{N_{t-2}} N_t + \psi H_t$$

Capital is fixed in the short-run so output is a function of the variable input, labor. Element c represents periodic operating costs, and $\tau \frac{L_{t-1}}{N_{t-2}}$ represents the per-employee cost of layoffs from the previous period (L_{t-1}), mirroring experience-rating. The production function $f(N_t)$ reflects a decreasing marginal product of labor. The cost of hiring workers is ψ. Firms adjust their employment by choosing non-negative hires and layoffs in each period, and workers attrit at rate $1 - \delta$:

$$\mathrm{N}_t = N_{t-1}\delta - L_t + H_t; L, H \geq 0.$$

If a firm experiences a negative shock, represented by depressed output prices ($p' < p$), the model predicts that the tax restrains layoffs during the initial shock and reduces hiring when the tax penalty is applied. Firms optimally choose hiring that satisfies the first-order condition:

$$H_{t+1}^{\star} = g^{-1}\left(\frac{1}{p} + \frac{\tau L_{t-1}}{p N_{t-2}}\right) + L_{t-1} - N_{t-2}$$

[8] In related work, I find that while labor demand is responsive to current Bartik shocks, they are not responsive to future shocks, suggesting that firms do not foresee the coming downturn or at least do not act accordingly.

In which $g = f'()$, the inverse of which is a decreasing, convex function. These imply that as the tax rates increase, hiring falls at a decreasing rate.[9] Similarly, the tax penalty discourages layoffs at a descending rate:

$$L_t^{\star} = \frac{N_{t-1}\big(p'f'(N_t) + pf'(N_{t+1})\big)}{2\tau} - \frac{N_{t-1}}{\tau} + \frac{N_{t-1} + H_{t+1}}{2}$$

Firms, however, may not be able to follow the optimal employment path if they are cash-constrained, requiring profits to be positive in each period ($\Pi_t \geq 0\ \forall t$) just to cover the costs of operation, similar to the cash-in-advance constraint in Clower (1967) and Lucas and Stokey (1987).

Illustrating this point, consider an example in which firms receive \$10 per unit and respond to an unexpected shock in which prices fall to \$5 for two periods. I set $\delta = 0.1$, $c = 2$, $\tau = ¼$, ψ=¼, ψ'=⅛, and $N_0 = 25$, the long-run optimal employment:

$$\Pi_{\mathrm{t}} = p_t\sqrt{N_t} - N_t - 2 - \tau\frac{L_{t-1}}{N_{t-2}}N_t + \psi H_t$$

The penalty τ naturally discourages layoffs. But in this example, the shock induces the firm to contemplate negative profits. If the firm is without cash or credit, the firm must reduce employment by at least five to satisfy the constraint, maintaining non-negative profits to cover its costs. If the firm does not reduce its employment, it will fail to cover its costs and be forced to exit ($\Pi_t = 5\times\sqrt{25} - 25 - 2 = -1.5$). Since $\delta = 0.1$, the firm reduces employment by two and a half workers by attrition and must layoff an additional two and a half workers to completely cover its costs ($\Pi_t = 5\times\sqrt{20} - 20 - 2 = 0.4$). Notice that the firm prefers to layoff no one, since hiring is costly, but no employment reduction fewer than five will yield positive profits. Importantly, the future penalty—no matter

[9] Notice, the derivative $\partial L^{\star}/\partial\tau$ is negative only if the average marginal revenue product is greater than one; this is a necessary condition of profitable firms since the wage rate is unity.

how large—cannot deter the firm from these layoffs because the layoffs are necessary to survive the period.

In the following period, the firm's tax rate increases, again inducing negative profits ($\Pi_t = 5\times\sqrt{20} - 20\left(1 + \tau\frac{2.5}{25}\right) - 2 = -0.1$). Without the tax, the firm could have maintained positive profits while hiring workers to replace those that attrit ($\Pi_t = 5\times\sqrt{20} - 20 - 2 - \frac{1}{4} = 0.1$). With the tax, the firm prefers to maintain its employment but hires one fewer with the tax than without in order to satisfy the binding cash constraint ($\Pi_t = 5\times\sqrt{20} - 20\left(1 + \tau\frac{2.5}{25}\right) - 2 - \frac{1}{4} = -0.4$). In short, cash constraints can theoretically make firm hiring more responsive to tax increases while making layoffs less responsive to promised penalties.

The important elements for determining whether the tax promotes employment, empirically, are the overhang effect ($\partial H_{t+1}/\partial\tau$) and a deterrent effect ($\partial L_t/\partial\tau$), which I turn to estimate in the empirical sections. For cash-constrained firms, $\partial L_t/\partial\tau$ will be closer to zero and $\partial H_{t+1}/\partial\tau$ will be larger (more negative) than for firms without such constraints, a prediction I test in the empirical sections.

IV. DATA

I obtained full-population administrative data for the universe of workers and firms in Florida 2003–2012, with each worker or firm identified by a scrambled SSN or EIN.[10] These data include three main files: a wage file covering 17,722,328 workers, a firm file covering 890,734 unique firms, and a claims file covering 2,771,418 workers who claimed UI benefits during the period. The wage

[10] Nationally, an employer is automatically enrolled in the UI program if it has a payroll of $1,500 or more in a calendar year or has at least one employee working at least a portion of one day during any 20 weeks of a calendar. The coverage includes businesses, nonprofit organizations, state or local government employers, and Indian tribal units (Florida 2012). In practice, all lawful employers are in the data.

file indicates the earnings each worker received from each employer in each quarter; the firm file includes each firm's tax rate, benefit ratio, benefit cost, wage base, NAICS code, and county; the claims file indicates which employees claimed benefits, the value of benefits they received and which employer they charged.

I use these data to calculate the employment of each firm, identify new hires from the wage data, layoffs from the claims data, and identify temporary layoffs as those layoffs recalled to his most recent employer within one year of a claim. I code a firm as exiting in the last quarter it has positive employment. I exclude firms that are too new to be experience-rated and firms that have no benefits charged since these firms face no kink-related tax variation.[11] I present the summary statistics of the remaining firms in Table 1. The average firm employs 58.8 workers with significant turn over. For instance, a representative firm hires 27.5 employees each year while separating from 27.0 employees annually. Separations appear to primarily be voluntary quits. The average firm lays off 0.8 workers each year who claim benefits, five percent of which are temporary layoffs.

About 7.4 percent of firms exit each year, with smaller firms exiting at a slightly higher rate. The median worker receives yearly earnings of $21,600 and the wage distribution is highly skewed.[12] The average firm faces a UI payroll tax rate of 3.1 percent and has a benefit ratio of 0.066, ranging from 0.0 to 99.999. Firms around the kink have a similar profile and I compare the summary statistics of the full sample of cleaned data to the summary statistics of the firms within the IK optimal bandwidth around the kink in Table 1. To limit the influence of

[11] In Florida, firms do not receive a firm-specific tax rate for their first three years when they are new. Instead, they pay a fixed rate of 2.7 percent but the state keeps record of the firm's benefit charges and wage base to calculate an individualized tax rate after three years. Firms with no charges tend to be smaller, stable firms like insurance offices and family restaurants.
[12] For comparison, Florida has the population of a medium-large European country, having about double that of Austria, Belgium, Denmark, Greece, Portugal, Sweden, or Switzerland.

outliers, I winsorize hiring, employment, and wages at the 0.1 percent tails (Edmans 2012).

V. The Overhang Effect

Because of experience rating, UI tax increases target distressed firms just after layoffs. Whereas other tax instruments reduce the tax burden when firms are in stress, the UI program increases tax rates which may reduce hiring in times, industries, or states where unemployment is already prevalent. The tax rate firms bear in Florida is a kinked function of the benefit ratio because the tax rate increases with the benefit ratio but cannot exceed the maximum allowable rate.

Because the maximum in Florida is relatively low by modern standards, it is well-populated and firms at the kink do not appear strikingly different from other firms (see Table I). Roughly 11 percent of firms have benefit ratios that exceed the kink point, suggesting the kink is at the 89th percentile of in terms of firm layoff rates; among firms with positive benefit ratios (those with some UI charge in the past three years), over a third of firms have benefit ratios higher than the kink point.[13]

Importantly, the kink identifies the effect of tax variation among firms who have recently had layoffs, meaning it precisely recovers the impact discussed previously as the overhang effect.

Using a regression kink design (RKD), I compare similar firms whose tax rates vary independent of underlying firm factors, conditional on a few controls (Card et al. 2015). I model a firm's behavior y (principally in terms of hiring, but also in terms of employment, exit, wages, and layoffs) as a continuously differentiable function of the running variable (the firm's benefit ratio, v_{it}, minus the kink's location, k_{it}) to estimate the effect of kink-induced variation in the tax

[13] About 41 percent of observations have a benefit ratio of 0, having had no benefit charges in the past three years.

rate. The tax formula changes slightly from year to year so I implement a fuzzy RKD, instrumenting rates with the kink in two-stage estimation:

$$R_{it} = \gamma_1(v_{it} - k_{it}) + \gamma_2 D_{it} \times (v_{it} - k_{it}) + u_{it},$$
$$y_{it} = \beta_1 \widehat{R_{it}} + \beta_2(v_{it} - k_{it}) + \varepsilon_{it}.$$

Here, R_{it} is the UI tax rate of firm i in year t, and D_{it} is an indicator for being above the kink point. I leverage the panel structure of the data and show the estimates are highly robust to firm and year fixed-effects. This robustness demonstrates that the estimates are not the result of spurious firm differences around the kink point.[14] The parameter γ_2 reflects the average slope change at the kink point which provides the identifying variation for β_1, the effect of a one-point tax increase. The first stage is strong (Figure II). The instrument has a t-statistic of 1,428 and the specification explains 97 percent of the variation in tax rates around the kink.[15] The analyst has two degrees of freedom, bandwidth and flexibility, when implementing these models, and I vary both to demonstrate the results are not the product of multiple testing. I use Imbens-Kalyanaraman optimal bandwidths but show that the main effects are remarkably robust to a wide variety of bandwidth choices (Card et al. 2015; Imbens and Kalyanaraman 2012).

The IK bandwidths essentially restrict the analysis to firms within a narrow region around the kink point, implicitly comparing the behavior of similar firms with similar layoff histories who confront different tax rates. Comparisons among these similar firms address concerns that observations at the maximum are

[14] This estimation strategy imposes an assumption that the firm dummies and RKD controls are additively separable. Within firm variation identifies the effect of the tax kink and cross-firm comparisons of firms that do not change their placement relative to the kink point aid in estimating the relationship between the outcome and running variable and the year coefficients, in effect assuming that the relationship is similar for different types of firms. In practice, the fixed-effects can also exacerbate finite sample problems since in practice the design aggregates many single-firm experiments.

[15] The model does not explain 100 percent of the tax-rate variation because the tax schedule changes slightly from year to year.

radically different than other firms in the analytic sample. For example, all the firms within the usual IK bandwidth of the kink differ by less than 0.02 standard deviations of the benefit-ratio statistic, suggesting even the highest rated and lowest rated firms in the analytic sample are quite similar. Throughout the analysis, standard errors are clustered at the firm level.

Diagnostics

Unbiased estimation relies on two assumptions. First, the assignment variable must have a smooth marginal effect on the outcome of interest, meaning the benefit ratio must not have a kinked effect on firm behavior. Second, unobserved determinants of the outcome variable must evolve smoothly around the kink point. Put simply, other factors of firm behavior must not be kinked at the same point at which the tax kinks.

A principal concern arises if firms can precisely manipulate their placement on the tax schedule, since this can generate a break in unobserved factors arising from selection. Although firms can know the placement of the kink, it is essentially impossible for firms to precisely manipulate their placement on the schedule for three reasons. First, tax rates depend on the value of benefits drawn by laid-off workers, a variable that firms would find difficult to control; second, benefit costs increase in discrete weekly increments, making precise manipulation still more challenging; finally, the schedule changes from year to year, making fine tuning especially impractical.

More to the point, there is no evidence of strategic manipulation as firm density does not bunch or break around the kink point (Online Appendix Figure III); the vertical line denotes the location of the kink point. Firm density thins as the benefit ratio increases, but there is no increased density on the favorable side of the kink, as would be the case if firms could manipulate their benefit ratio precisely. I find no significant discontinuity or kink in firm density when estimating a local quadratic model to fit the curvature of the distribution

(McCrary 2008). Together, this suggests little reason for concern based on selection (Lee and Lemieux 2013).

As another probe of validity, I test for kinks in pre-determined covariates around the threshold. In addition to verifying that there are no kinks in industrial makeup, I construct covariate indices using all the predetermined covariates available in the data, following Card, Chetty, and Weber (2007). To construct each index, I regress each outcome variable (hiring, employment, layoffs, average wages, and exit) on all the pre-determined firm characteristics including entity type, detailed industry code, proxies for firm age, county location, and observation year.

The predicted values of these regressions amount to covariate indices which I use to assess the smoothness of firm covariates around the kink which I present in Figure III. Visually, each index evolves smoothly across the kink point, suggesting covariate balance. To test for covariate balance more formally, I estimate placebo models that deploy RKD using the covariate indices as outcome variables to evaluate whether firm characteristics evolve smoothly at the kink point, seen in Online Appendix Table I. The estimated kinks in the covariate indices are quite small—miniscule—and statistically insignificant, suggesting that any estimated kinks in firm behavior are not artifacts of observed firm characteristics around the kink.[16]

A final concern is that the state implements other policies that differ at the threshold. I corresponded with Florida's UI program administrators who indicated that no other policies, at the local, state, or federal level are based on the benefit ratio, implying other policies are not triggered by the reserve ratio. In summary,

[16] I investigate whether quasi-experimental variation in tax rates predicts tax differences in other years which is natural because new benefit charges affect a firm's tax rate for three years. A kink-induced 1.0-point higher tax rate in year *t* is associated with a 0.3-point higher tax rate in year *t–1* and a 0.6-point higher tax rate in year *t+1*. To generate the placebos in Figure IV, I use years farther out when constructing placebo figures. I use three years before the tax to generate placebos and three years after to produce a placebo for exit.

there is no bunching, covariates trend smoothly, the estimated placebo effects from the covariate indices are small and insignificant, and other government policies do not change at the kink, suggesting in total that the setting represents a useful quasi-experiment for unbiased estimation.

Results

Results are presented in Table II. The estimates reflect the effect of a one-point UI tax increase on firm hiring, employment, layoffs, exit, and log wages. A one-point tax increase raises the tax fee by $70 per employee, which is 0.3 percent of median yearly wages and 24 percent increase in average UI tax liability among firms around the kink. Most of the tax bill must be paid in the first quarter of the year (in practice, two-thirds), since firms pay the tax rate on each employee's wages until the worker has been paid excess of the individual's taxable wage base.

The estimates suggest that a one-point tax increase reduces yearly firm hiring by 0.7 employees (2.8 percent reduction), and firm size falls by a corresponding 0.8 employees (1.5 percent reduction). It is reassuring that hiring and employment estimates closely align which is a useful check on the results. These estimated effects are in line with a labor demand elasticity of 4.1 with CIs ruling out elasticities smaller than 2.1.[17] Consistent with short-run wage rigidity, tax changes do not reduce worker wages. Nor do tax increases affect firm layoffs, separations, or exit. The estimates are robust to varied bandwidths and polynomial specifications (see Table II). Importantly, the results are robust to including firm-specific fixed-effects, arguing against concerns that the kink is an artifact of spurious firm heterogeneity along the reserve ratio around the kink point.

To evaluate how the bandwidth choice affects the estimates, I vary the bandwidth while implementing the preferred specification on hiring (linear

[17] The tax reduces employment by 1.5 percent and increases the wage bill by 0.3 percent.

controls, optimal bandwidth, firm and year fixed-effects), the results of which are shown in Figure V. At bandwidths smaller than optimal, the effect tends to be larger, but the estimates are remarkably stable, at around –0.65, from 60 percent of the optimal bandwidth to 500 percent of the optimal bandwidth. An important concern when implementing RKD is that the analyst is recovering effects only by misspecifying a curve with linear approximations. Importantly, if the kinks I estimated were the product of misspecifying a curve, smaller bandwidths would produce smaller (less negative) coefficient estimates and larger bandwidths would generate larger estimates (more negative). Instead, the largest estimates exist at small bandwidths, suggesting the results are not the product of misspecification. I also implement a score of placebo tests in which I reproduce the main table using outcomes from placebo years in Online Appendix Table II. Whereas the main effects are significant and robust, placebo estimates tend to be small, insignificant, and unstable.

I present residualized plots which account for firm and year fixed-effects in Figure IV. The figures show a distinct kink in firm hiring and employment, but not in exit. Parallel placebo figures using the firm's outcome from a placebo year show no kink, suggesting the kinked effect is not the product of coincidental firm differences. To evaluate the likelihood of estimating significant hiring effects by chance, I estimate placebos at regular intervals along the benefit ratio while keeping the bandwidth fixed.[18] By comparing the empirical distribution of these placebos to the main estimate one can evaluate the empirical likelihood of estimating the effects I do by chance (Online Appendix Figure IV). This process generates 450 placebo estimates, which cluster near zero, averaging –0.03. Only

[18] I estimate a placebo from the minimum benefit ratio to 0.50 at intervals of 0.001. The bandwidth is held constant at 0.04, the optimal at the kink, and I exclude the estimates from the area around the kink point.

one of the placebo estimates is smaller than the main effect, suggesting the probability of randomly estimating an effect this size is quite unlikely.

The estimates on hiring and employment are large. To evaluate the extent to which cash-constraints magnify the overhang effect, I investigate whether the effect varies by measures of cash-constraints. First, I estimate the effect separately in "expansionary" years (2003–2007) and "recessionary" years (2008–2012), implementing the preferred model on the two distinct subsamples (linear controls, optimal bandwidth, firm and year fixed-effects). Consistent with financial duress exacerbating the overhang effect, taxes reduce hiring significantly more during the recessionary period when firms were in greater distress ($p < 0.01$). Differences in employment effects in the two periods are large economically but not statistically different.

I use Compustat to compute a measure of cash-per-employee and cash-flow-per-employee of each industry and estimate the model separately for firms in industries that are above and below the median of each cash measure. Again, the effect of tax variation is significantly larger for firms in industries with less cash on hand (significant differences with $p < 0.01$ in hiring, $p < 0.05$ in employment) (Online Appendix Table III), consistent with the hypothesis that cash-constraints magnify the overhang implications of UI taxes. The industry-average cash measure is a noisy signal of a firm's cash constraint, which may understate the role of cash constraints in the firm response (Farre-Mensa and Ljungqvist 2015). Significantly, since the baseline firm at the kink is somewhat inherently distressed by virtue of being near the kink, this may explain why the effects are so large. Moreover, I estimate the effect by industry group. The hiring and employment effects are concentrated in manufacturing, transportation/ communication, and retail and the effects are smaller and statistically insignificant in industries that tend to have access to credit, like finance, insurance, and real estate.

In a final test of heterogeneity, I exploit a parallel dataset from Missouri. Missouri employs a kink in the tax rate, but the kink resides at the minimum rate rather than its maximum. This allows for an estimation of the tax effect among a group of firms with few layoffs. The estimates suggest a 1-point tax increase reduce hiring by 0.25 ($p < 0.01$) and employment by 0.26 (insignificant) (Online Appendix Table IV), consistent with an own-wage elasticity of 1.0 where the CIs on hiring rule out own-wage elasticities smaller than 0.2. Consistent with the cash-constraints explanation of firm responsiveness, firms are more responsive at Florida's maximum than Missouri's minimum.[19]

VI. The Deterrence Effect

Given that tax increases reduce employment, it is natural to wonder whether there is a countervailing benefit. Specifically, do penalties deter layoffs as intended (Myers 1940)? If penalties succeed in deterring layoffs, the tax program could increase employment on net.[20] To measure the deterrence effect, I leverage variation in the penalties firms face because of the maximum rate. The statutory maximum differentially shields firms from rate increases based on where they reside on the tax schedule around the onset of the maximum rate. For instance, firms with reserve ratios that put the firm just under the maximum rate will bear a modest penalty for layoffs while those with reserve ratios barely reaching the maximum rate confront no penalty though they are very similar firms within 0.01 standard deviations in terms of layoff histories (the benefit ratio). I estimate the

[19] To examine the timing of the tax effect over the year, I estimate models in which firms impact the hiring behavior in each quarter of the year. The effects are concentrated in the second and third quarters in about equal share ($p < 0.01$). Interestingly, after three quarters of below-average hiring, the firm hires at slightly above average in the last quarter of the year, perhaps having stabilized ($p < 0.05$).

[20] Wisconsin was the first to experiment with what it called "merit rating" in 1938, intended to apportion costs of layoffs to originating firm. Most states adopted the practice within the following decade, though Washington State was not experience-rated as recently as 1984 (Anderson and Meyer 2000; Ratner 2013).

relationship between a firm's layoff rate (layoffs as a percentage of last year's employment) and the tax penalty the firm faces:

$$y_{it} = \beta P_{it} + \psi(BR_{it}) + \alpha_t + \delta_i + \varepsilon_{it}.$$

Here, P reflects the implicit tax penalty a given firm faces for layoffs given its placement on the tax schedule. I calculate this variable by simulating how the firm's tax rate would change in response to a typical five-percent layoff.[21] Firms with a benefit ratio 0.02 below the onset of the maximum incur tax increases of 1.8 percentage points for a layoff, which penalty declines to zero as the firm approaches the maximum rate. That is, within 0.01 SDs of the benefit ratio, the penalty ranges from 0 to 1.8 percentage points, holding layoff size fixed. Negative estimates of β would reflect that penalties succeed in deterring layoffs. Firm fixed-effects (δ_i) account for stable firm differences in their propensity to layoff workers, time fixed-effects (α_t) control for broad macroeconomic trends, and a quadratic polynomial of the firm's reserve ratio (RR_{it}) accounts for time-varying differences related to the firm's evolving layoff history. In practice, P is not randomly assigned and the penalties firms face depend in part on the slope of the tax schedule each year and the minimum rate applied to firms. To train the regression on plausibly exogenous variation arising from the differential shielding of the maximum rate, I enrich the panel model above with an instrumental variables strategy (Angrist 2009).

I operationalize this approach by instrumenting the penalty a firm confronts (P) with the rate-distance between the firm's rate and the maximum allowable rate which is plausibly exogenous to the firm. The maximum generates this variation by shielding firms from penalties based on their distance to the maximum when below the maximum rate. By comparing firms within a narrow bandwidth around the maximum rate, I implicitly compare like firms that faced

[21] To calculate the cost, I assume claimants received the average weekly benefit for the average duration.

different tax penalties for layoffs. The first stage is strong with the instrument predicting 12 percent of the variation in the endogenous regressor and a t-statistic of 1,005. The resulting estimates represent the effect of a one-point higher UI tax increase in response to a layoff. I implement the specification within the optimal-IK bandwidth of the onset of the maximum rate and cluster the standard errors at the firm level, as before.

Firms facing larger penalties, surprisingly, do not have fewer layoffs (Table III). The estimated deterrence effects from the IV model are all small, insignificant, and wrong-signed ranging from 0.0004 to 0.0009 (recall negative coefficients reflect deterrence). The estimates from the panel and the instrumental variable rule out layoff deterrence effects of magnitudes greater (in absolute value) than –0.001 (layoffs per employee), –0.0004 (temporary layoffs per employee), and –0.0007 (separations per employee).

What effect size should we expect? In theory, a layoff penalty has the same effect on labor demand as a wage reduction of the penalty size since the employer will pay the dismissal cost regardless. Average estimates of labor demand elasticities cluster around 0.5 (Katz 1996; Lichter, Peichl and Siegloch 2015) which suggests that firms should reduce their layoffs by 6.6 percent, equivalent to an estimated –0.0028 (6.6 percent times 0.043 layoff rate). The confidence intervals on the deterrence estimates rule out magnitudes of this size, suggesting that firms are less responsive to penalties than we would expect under normal conditions, possibly due to cash-constraints.

To evaluate the robustness of this finding, I turn to two additional natural experiments. In Missouri, the tiered tax schedule sanctions firms with larger penalties if they are nearer the next tax step or the next step is larger. In particular, there is one large step which allows me to compare very similar at the threshold that face different penalties. Estimates from this setting also suggest precise null effects. Perhaps firms do not respond to local penalties on a tax schedule but are deterred by major differences, like those that exist across states over time. To probe this explanation, I exploit variation in experience rating across states and over time 1986–2013, to estimate whether industries in states that allow for larger penalties are less likely to downsize with data from County Business Patterns. Again, I find that firms more exposed to tax penalties are no less likely to downsize. To conserve space, I discuss these two natural experiments in detail in the Online Appendix.[22,23]

VII. Discussion

A one-point increase in the UI tax rate reduces yearly firm hiring by 0.7 hires (2.8 percent), reducing employment by 0.8 employees (1.5 percent). Taxes have no effect on layoffs, exit, or wages. The implied own-wage labor demand elasticity is 4.1, with the confidence interval ruling out elasticities smaller than 2.1. The responsiveness of firms to tax increases suggest that labor demand may be more elastic for firms in distress. In contrast, promised penalties do not reliably reduce layoffs. As discussed in the conceptual framework, these responses are consistent with a model in which cash-constraints magnify the overhang response while mitigating deterrence.

The overhang analysis of this paper extends and complements the work of Anderson and Meyer (1997 2000), henceforth AM. Using data from the late 1970s and early 1980s, they estimate the effect of UI rate changes which imply large elasticities. AM (1997) find own-wage labor demand elasticities of 0.7–0.9

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[22] The results are robust to using other measures of tax penalties including that of Topel (1983).

[23] One feature of experience-rating that has been sometimes ignored in previous research due to data limitations is how experience-rating partially depends on the distance a firm is from the maximum rate. A firm ε below the maximum rate does not suffer a significant penalty from marginal layoffs, so the slope of the tax rate locally is not a good measure of experience rating as the firm approaches the maximum rate. At the other end, a firm with a minimum rating faces a large potential tax penalty. To capture this variation in experience-rating, I also experiment by converting Topel's measure of experience-rating into the per-employee penalty of a five percent layoff, a measure developed by the Bureau of Labor Statistics (BLS) to describe the tax penalty. I test to see if my results come from this computation of the marginal tax cost. The results are consistent with and without the adjustment.

for firm-level tax variation but inconsistent estimates from industry-level variation. In this work, they identify elasticities using endogenous changes in tax rates which may be confounded with firm behavior.[24] AM (2000) provide cleaner identification using the introduction of experience-rating in Washington State in 1985 and report earnings elasticities of 2.8–3.9 using industry-level variation in the tax rate. Firm-level variation in the tax rate produce smaller, but still large, point estimates in the 0.7–1.8 range. Though that variation is exogenous to the firm, it's unclear whether the variation reflects the impact of UI tax increases. Not only were two changes occurring simultaneously (the introduction of firm-specific rates and the onset of experience rating), but the tax change was long-anticipated. The 1985 introduction of experience rating was initiated three years prior when the federal government passed TEFRA in 1982. With a long lead time, managers can adjust employment and wages well in advance of the tax change. In contrast, typical UI tax rates are announced a month before they are implemented, allowing little scope for firms to adjust employment and wages in preparation.

Analysts tend to find employment effects when wages are fixed either by a short time horizon or under a binding minimum wage. For instance, Kramarz and Philippon (2001) found that a sudden 1 percent decrease in labor cost increases employment by 1.5 percent among minimum-wage workers. Using Belgian data, Goos and Konings (2007) found similar large employment effects. In contrast, analysts exploiting reforms well-known before implementation report smaller or insignificant employment effects (Hamermesh 1979; Kugler and Kugler 2008; Bennmarker, Mallander, and Ockert 2009; Saez, Matsaganis, and Tsakloglou 2012; Egebark and Kaunitz 2014).[25]

[24] A changes-in-changes design in this context is likely to be biased since depressed demand for a firm's production affects both firm behavior and tax rates.
[25] Kugler and Kugler (2008) find a 1-point decrease in taxes increasing employment by 0.5 percent. Egebark and Kaunitz (2014) use a payroll tax cut targeted to young workers and report labor demand elasticity of 0.3; the authors find larger elasticities for young workers with skills

In addition to the role of duress and a short time horizon in explaining the size of the elasticities, labor demand elasticities may be growing over time as the substitutes for domestic labor become more capable and less expensive. Estimated labor demand elasticities have increased (grown more negative) steadily by 0.l each decade over the past several years (Lichter, Peichl and Siegloch 2015).

The deterrence piece of this paper relates to the work of Topel (1983), Anderson (1993) and Card & Levine (1994). These papers use data from the 1970s and early 1980s to compare the unemployment transitions of industries facing different penalties for layoffs and find substantial evidence of deterrence, usually in terms of discouraging temporary layoffs. Topel (1983) uses a sample of individuals from the 1975 *Current Population Survey* (CPS) and relates temporary layoffs to the marginal tax rates in the cross section. Card and Levine (1994) develop this strategy by constructing a panel, from the CPS and find deterrence after accounting for state and industry differences.

There may be empirical reasons that the results reported in this study differ. My analysis uses firm-level data whereas Topel (1983) and Card and Levine (1994) use industry-level data. I show in Table 3 that without firm fixed effect, the estimates would suggest deterrence (column 1). When I account for firm heterogeneity, the sign flips (column 2), suggesting that firm-specific factors may be important when measuring responses tax penalties. Anderson (1993) constructs a panel of 8,000 retail firms from the 1970s and 1980s and finds evidence of deterrence even after accounting for firm differences with firm fixed effects. I use the three natural experiments I've reported on to see if I can detect deterrence effects among retail firms specifically. As before, the estimated effects are small, insignificant, and often wrong-signed in each of the three experiments. I replicate

(0.8). Bennmarker, Mellander, and Ockert (2009) find that a 10-point reduction in payroll taxes had no immediate effect on employment, increased wages by 3 percent, and reduced firm exit. Saez, Matsaganis, and Tsakloglou (2012) use an anticipated cohort reform and report no effects on employment and wages.

the measure of marginal tax cost that Anderson used, but can detect no effect despite significant statistical power.

Three possibilities seem likely. One is that the labor market may have changed significantly. Automation and a rapidly evolving market has induced churn and reduced "attachment" of workers to firms, represented by the fact that temporary layoffs, which were 22 percent of layoffs in 1980, are now just 5 percent in my data. Two, it may be that within state variation does not deter layoffs but across-state variation does. If so, it would be surprising that the macro exercise I present which incorporates over 25 years of national data finds no correlation either within or across states. Third, it may be that in the 1970s and 1980s, UI tax penalties were correlated with other adjustment costs that no longer differ systematically.

Of particular concern is that the tax burden targets firms that are already in distress. Over the past three decades this targeting has increased markedly. Prior to the 1980s, states largely shielded distressed firms by limiting their tax exposure with low maximum rates. The 1982 Tax Equity and Fiscal Responsibility Act (TEFRA) required states to raise their maximum tax rates to 5.4 percent or higher by 1985. Online Appendix Figure X shows that in the mid-1980s the average maximum rate rose from 4.2 percent to 6.9 percent.[26] This rise dramatically increased the potential tax increases that offending firms faced.

Analysts in the 1970s and 1980s estimated that firms paid 50 percent of the benefit costs they originated due to low maximum rates (U.S. Department of Labor 1985; Hamermesh 1996). Anderson (1993) reports the average firm only paid 40 percent of the cost of benefit charges in her sample (1978–1984). Since then, TEFRA increased experience rating substantially. Using administrative records, I find that firms today pay a larger share of benefits charged than they did

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[26] Hamermesh (1993) estimates that TEFRA boosted experience rating by 15 percentage points or 30 percent.

four decades ago. On average, a firm in Florida pays 87 percent of benefits originated by the firm within two years. In Missouri, firms pay an estimated 96 percent of benefits within two years. Florida employs the federal minimum for maximum rates and taxable wages, suggesting that Florida's pay-back rate is likely near the lower bound for other states.

Since rates reflect the cost of benefits, more generous benefits or longer unemployment spells increase the penalties firms face. From 1990 to 2015, weekly benefit payments and average duration of UI receipt increased, resulting in the average claim costing 65 percent more, adjusted for inflation.[27] The confluence of increased experience rating and increased costs mean that a typical firm pays 187 percent more for an average layoff than it did in 1980.

With all caveats about external validity, I calculate what the estimates would imply about the national tax hikes that occurred in 2010 and 2011 assuming no spillovers.[28] Under strong assumptions, the results suggest that the tax increases in 2010 and 2011 reduced employment by 1.6 percent per year accounting for 12 percent of unemployment. Had experience-rating stayed constant since the early 1980s, tax increases during the Great Recession would have reduced employment by 1.0 percent. The increase in taxes during labor market recovery may explain part of the emergence of jobless recoveries since the 1980s. Whereas the Post-WWII average recession experienced 7.5 percent employment growth over the 24 months after the recession ended, the recent three recessions have average 0.5 percent growth over the same period. My estimates suggest that 29 percent of the gap between contemporary recoveries and past recoveries can be explained by

[27] Additionally, the 1991 Emergency Unemployment Compensation Act increased firms' tax liability during recessions by increasing the duration of extended benefits from 13 to 20 weeks, half financed by state UI programs.

[28] The $300 billion received by the unemployed from state UI programs during the aftermath of the Great Recession triggered $300 billion in UI tax increases.

increases in experience rating and benefit costs, with 55 percent of the effect accounted for by experience rating and 45 by cost increases.

The estimates speak to the social costs of financing UI benefits under the current tax regime. The work-horse model of optimal benefits seeks to maximize social welfare by increasing generosity until the marginal costs and benefits equate (Baily 1978; Chetty 2006; Schmieder, von Wachter, and Bender 2012). This model assumes that benefits are financed from workers' wages, a premise that would be accurate if rates were stable. Instead, UI tax increases reduce employment, not wages. Whereas wage reductions may provide welfare enhancing transfers, employment reductions represent deadweight loss, altering the calculus of optimal benefits.

In contrast to the US, European UI programs do not penalize firms for layoffs (Baicker, Goldin, and Katz 1998). A few reforms may mitigate the unintended consequences of the present regime while preserving the level of insurance provided to the unemployed. One possibility would be to use a low flat payroll tax to finance benefits for permanent layoffs, while experience-rating temporary layoffs which would continue to internalize the cost of routine temporary layoffs, while mitigating the deadweight loss associated with financing benefits for systemic shocks. Another proposal that would mitigate the unintended overhang effect, is one in which firms pay into a savings account for each employee's unemployment insurance until the worker has sufficient funding for the full duration of an unemployment spell (Acevedo, Eskenazi, and Pages 2005; Feldstein and Altman 2007; Hartley, van Ours, and Vodopivec 2011; Casselman 2016). If an employee is laid off, the worker receives his traditional benefit from the account, supplemented by the firm if the account is not yet fully funded, fully preserving the same level of insurance for displaced workers. This structure reduces tax payments when firms are most vulnerable and provides for more complete financing since a firm's contribution cannot be avoided by closure.

VIII. CONCLUSION

This paper measures the countervailing influences of the UI tax in order to identify the effect of UI penalties in discouraging layoffs (the *deterrent effect*) and the impact of resulting tax increases on labor demand and exit (the *overhang effect*). Using a variety of quasi-experimental designs, I find that firm employment is quite sensitive to UI tax rate increases but tax penalties do little to deter layoffs. A model of cash-constrained firms rationalizes these firm responses. Future penalties cannot deter a firm from layoffs if downsizing is necessary to survive, and added costs during recovery may necessitate additional cost reductions.

Many economists think of the UI program as an automatic stabilizer because more benefits are paid when unemployment is high; however, UI taxes also track the business cycle with lag, potentially burdening labor market recovery with higher labor costs when unemployment is already high. Thus, the UI tax structure may erode the overall stabilizing influence of the UI program. My results demonstrate that UI tax increases reduce labor demand imposing an efficiency loss not incorporated in models of optimal benefits.

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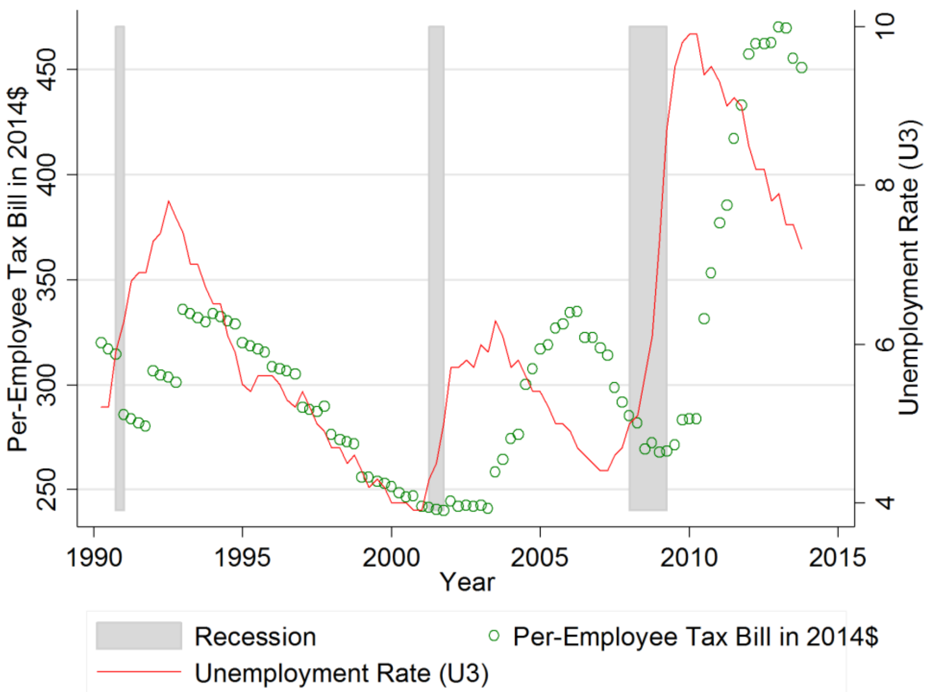
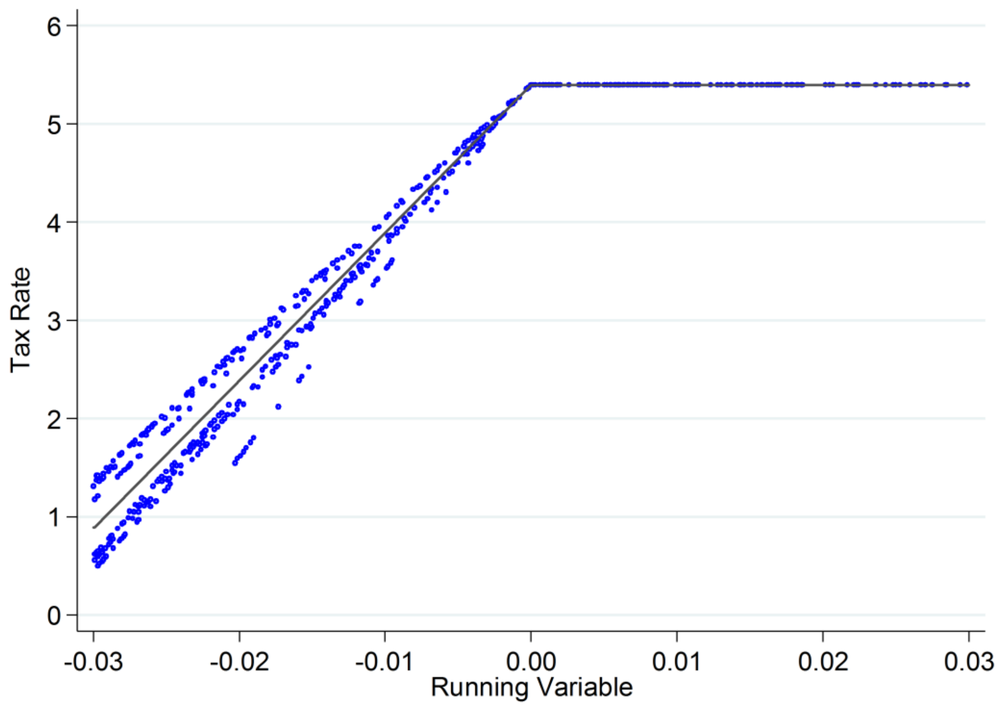
Figure I
Unemployment Insurance Taxation over the Business Cycle



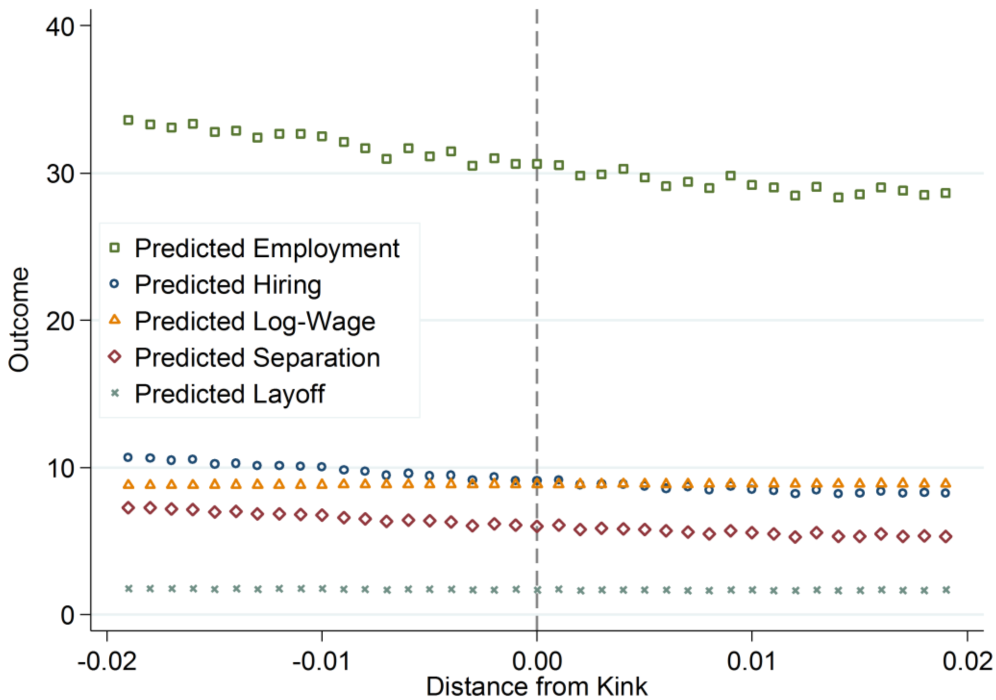
Note: The open dots represent the average per-employee tax paid across states by employers in 2014$ using BLS Unemployment Insurance Data Summary (UIDS). The continuous line represents the average unemployment rate using U3. The shaded periods are NBER-designated recessions. Source: Author's calculation of UIDS data provided by US DOL.

Figure II
First Stage in Florida Tax Formula



Note: The figure plots a small sample of firms around the tax kink to demonstrate the first stage. Administrative data from Florida DEO.

Figure III
Smoothness of Predicted Values around the Kink

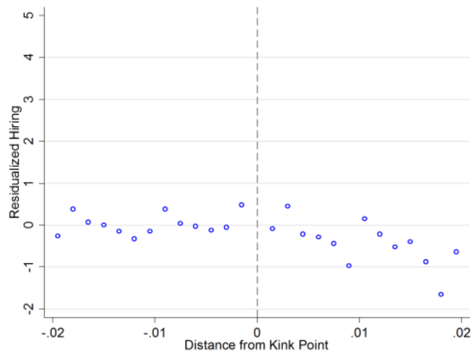


Note: These figures show the predicted values of major outcome variables in bins around the kink point. In each, the outcome is predicted with NAICS code, county, entity type, proxy for firm age, and its square. The indices show that, based on predetermined covariates, the predicted distribution of outcomes is smooth across the kink, suggesting observable factors of the outcome variable do not confound RKD estimation in this setting. Administrative data from Florida DOE.

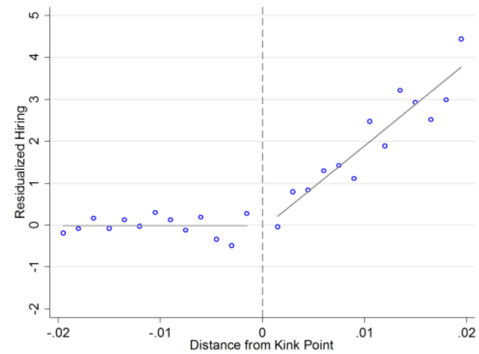
Figure IV
Regression Kink Figures

Placebo Figures *Treatment Figures*

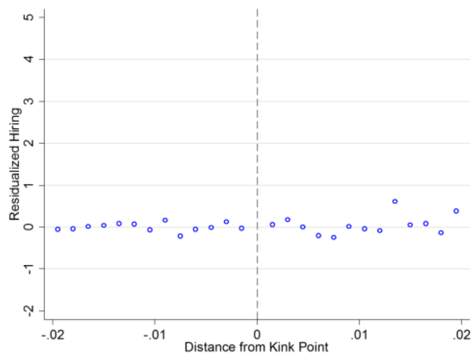
Panel A: Placebo Firm Size



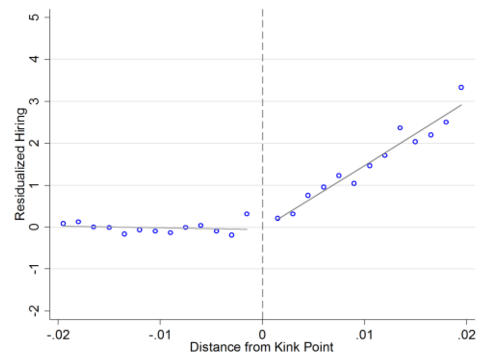
Panel D: Effect on Firm Size



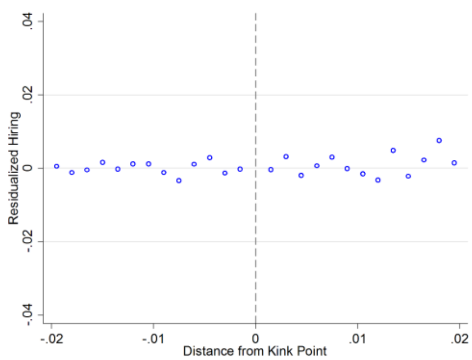
Panel B: Placebo Hires



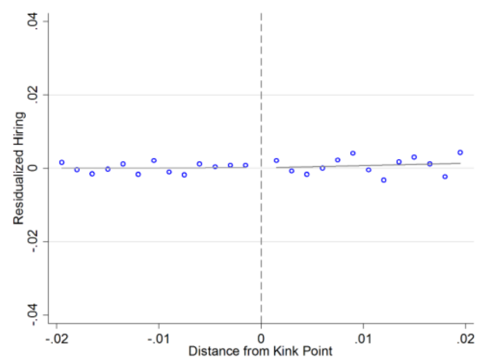
Panel E: Effect on Hires



Panel C: Lag Firm Exit

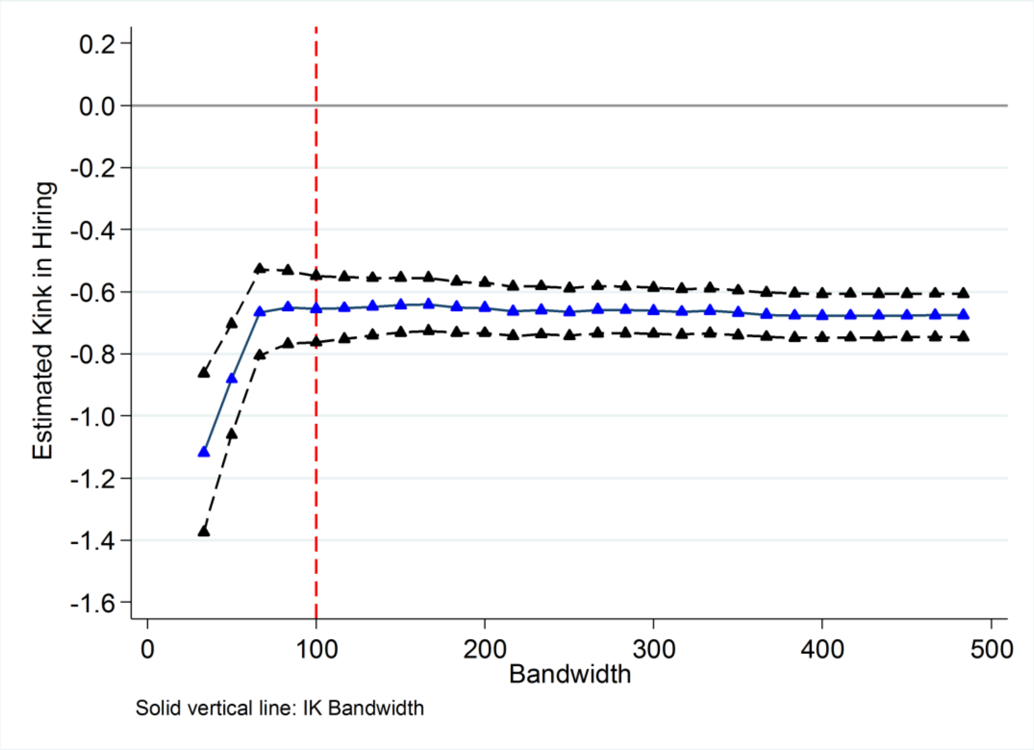


Panel F: Effect on Firm Exit



Note: The figures in the right column show the kink in the residuals accounting for firm and year FE. The left panel show placebo residuals using outcome data from a placebo year.

Figure V
Local Linear RKD Estimates on Hiring with Varying Bandwidths



Note: Each center triangle represents the estimated effect on hiring at a given bandwidth with the accompanying standard errors. The red dashed line represents the optimal IK bandwidth. Administrative data from Florida DEO.

Table I
Summary Statistics

	Cleaned Data	Near the Kink Point
Year	2007.9	2008.4
	(2.88)	(2.99)
Firm size	58.8	51.9
	(659)	(476)
Hires per year	27.5	23.1
	(325)	(211)
Separations per year	27.0	20.9
	(312)	(167)
Layoffs per year	2.9	2.7
	(24.1)	(19.6)
Temp layoffs per year	0.175	0.159
	(2.86)	(2.63)
Average wages	8,736	9,137
	(19,236)	(13,030)
Exit rate	0.074	0.074
	(0.26)	(0.26)
Tax rate	3.08	4.17
	(1.95)	(1.14)
Benefit ratio	0.066	0.026
	(1.74)	(1.14)
N	706,779	269,758

Note: Cleaned data from Florida DOE comparing firms at the kink to the larger sample.

Table II
Regression Kink Estimates of Overhang Effect

Outcome	RKD (1)	RKDFE (2)	RKDFE (3)	RKDFE (4)	RKD (5)
Hiring	-0.707	-0.650	-0.650	-1.012	-1.214
	(.057)	(.060)	(.042)	(.109)	(.117)
	774,273	774,273	832,726	832,726	373,795
Employment	-0.940	-0.788	-0.913	-0.454	-1.735
	(.193)	(.201)	(.114)	(.271)	(.513)
	492,729	492,729	794,414	794,414	213,277
Firm Exit	0.002	0.001	0.003	-0.003	0.003
	(.001)	(.001)	(.001)	(.002)	(.003)
	270,120	270,120	682,280	682,280	121,162
Log Ave. Wage	0.004	0.002	-0.002	0.012	0.009
	(.003)	(.004)	(.001)	(.004)	(.010)
	198,618	198,618	451,208	451,208	95,623
Layoff	0.043	-0.015	-0.025	-0.293	-0.009
	(.027)	(.031)	(.013)	(.034)	(.074)
	337,829	337,829	767,183	767,183	153,477
Controls					
Year Fixed Effects	X	X	X	X	X
Firm Fixed Effects		X	X	X	
Linear Control	X	X	X		X
Quadratic Control				X	
Bandwidth (times optimal)	1	1	2	2	½

Note: Each coefficient represents the causal estimate of a one-point increase in the tax rate at the tax kink on the outcome indicated on the left column. Administrative data from Florida DOE.

Table III
Panel and I.V. estimates of Deterrence in Florida

Outcome	Panel (1)	Panel (2)	Panel (3)	Panel (4)	IV-BW1 (5)	IV-BW2 (6)
All Layoff Rate (×10)	-0.008	0.083	0.007	0.004	0.009	0.004
Cls. St. Err.	(0.002)	(0.002)	(0.004)	(0.004)	(0.012)	(0.006)
(Ave=.043)						
Temp. Layoff Rate (×10)	-0.003	0.002	0.000	-0.001	0.002	0.002
Cls. St. Err.	(0.000)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)
(Ave=.002)						
All Separations Rate	0.016	0.005	0.000	-0.001	-0.001	-0.002
Cls. St. Err.	(0.000)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)
(Ave=.287)						
Year Fixed Effects	X	X	X	X	X	X
Firm Fixed Effects		X	X	X	X	X
Benefit Ratio Polynomial			X	X	X	X
Log Size				X		
Observations	694,541	694,541	694,541	694,541	254,956	413,937

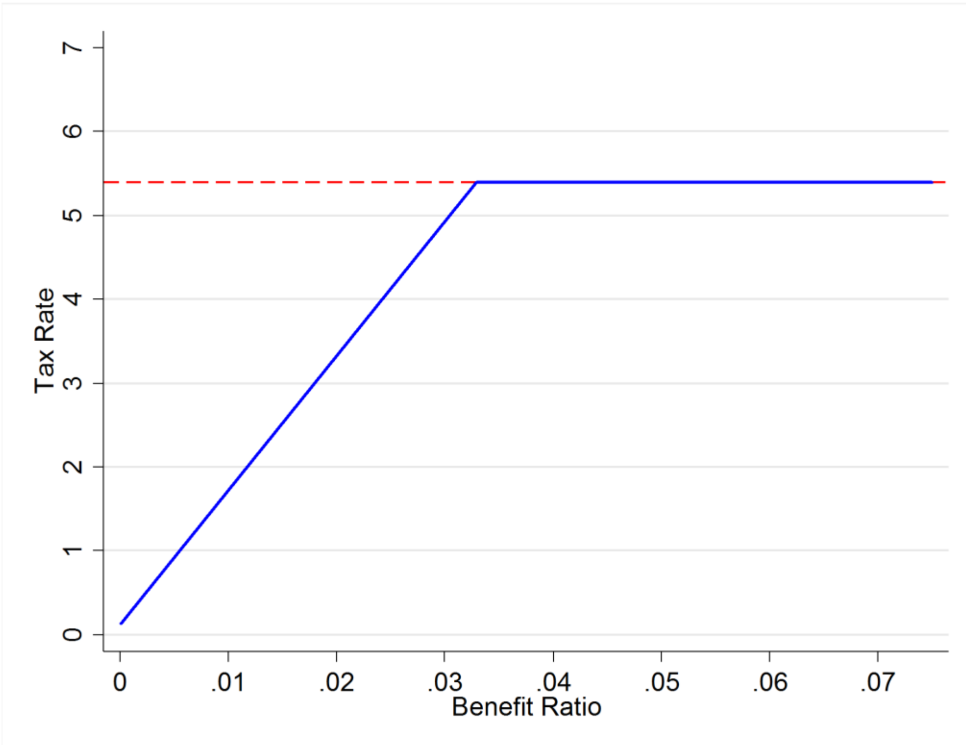
Note: Each coefficient represents the causal estimate of a one-point implicit tax penalty on a separation behavior indicated in the left column. Administrative data from Florida DOE.

For Online Publication

Online Appendix

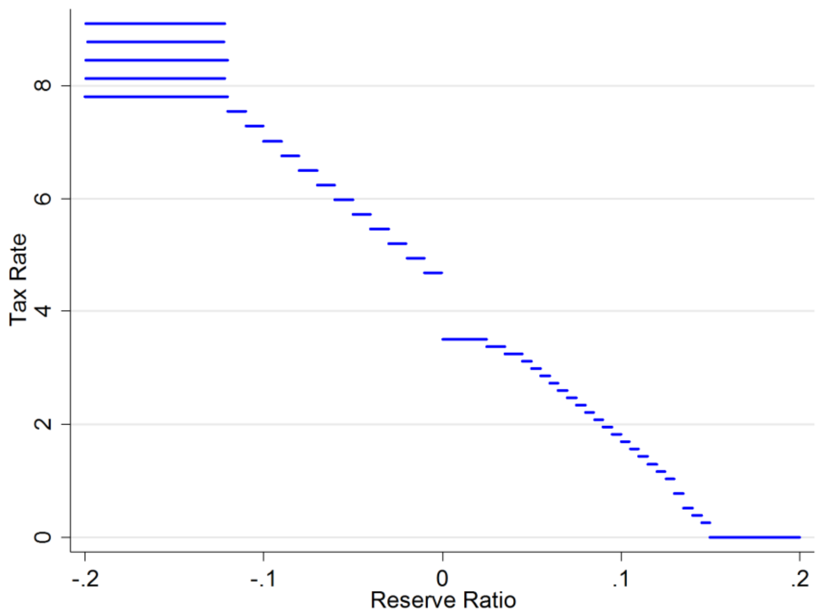
Online Appendix Figures

Online Appendix Figure I
Tax Schedule in Florida Tax Formula



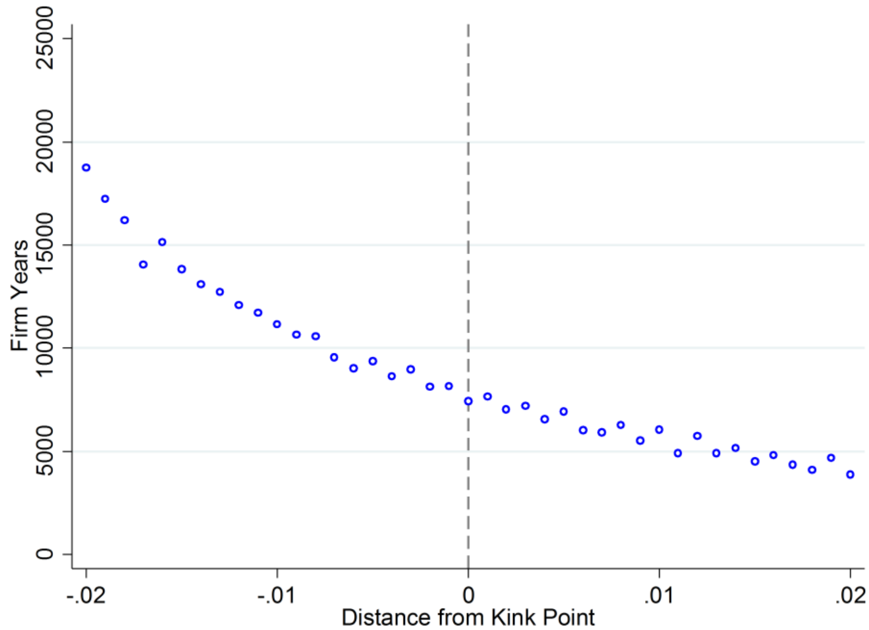
Note: This figure shows the kink in the UI tax formula used in Florida in 2010. The tax is a linear function of the benefit ratio, subject to a maximum. The benefit ratio reflects the cost of a firm's layoffs normalized by firm size (benefits paid to former employees as a percentage of taxable wages over the past three years). The tax rate is capped at 5.4 percent, generating a kink in the tax as a function of the benefit ratio. Source: Author's calculation of data provided by the Florida Department of Labor.

Online Appendix Figure II
Tax Schedule in Missouri



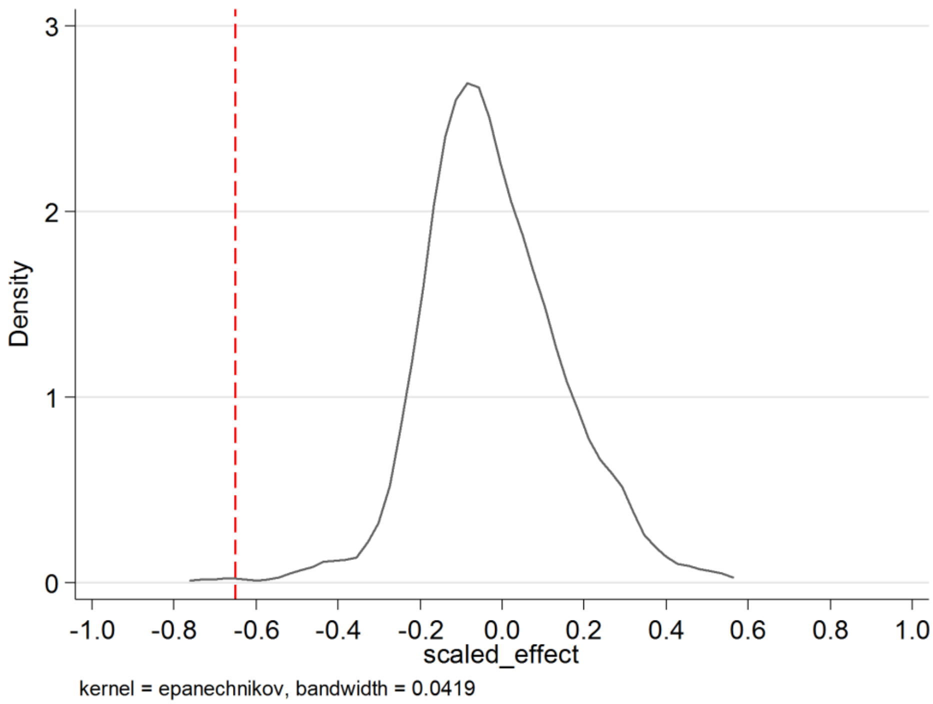
Note: This figure shows the tax schedule applied to firms in Missouri. The state calculates a reserve ratio, which reflects the firm's UI reserves divided by the average wage base over the past three years. Tax rates are generated when the state submits firm reserve ratios to a consistent tax schedule, seen above in part. The schedule is a series of discrete jumps, but one large discontinuity exists in the tax schedule, representing a 1.17 point tax increase for firms at the cutoff.

Online Appendix Figure III
Smoothness of Firm Density around Florida Tax Kink



Note: This figure plots the number of firms (firm-years) in bins around the kink point while excluding unrated firms. There is no bunching in the density and the number of observations appear smooth around the kink with no visible change in the slope. The data are from administrative records from Florida's Department of Labor.

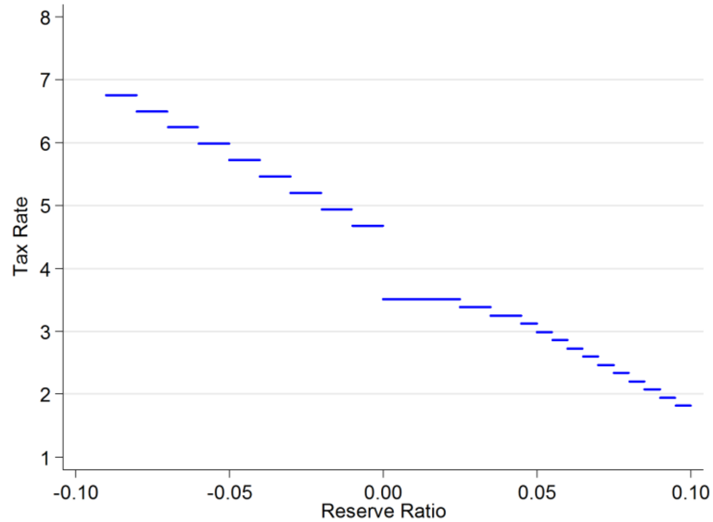
Online Appendix Figure IV
Empirical Distribution of RKD Placebo Estimates



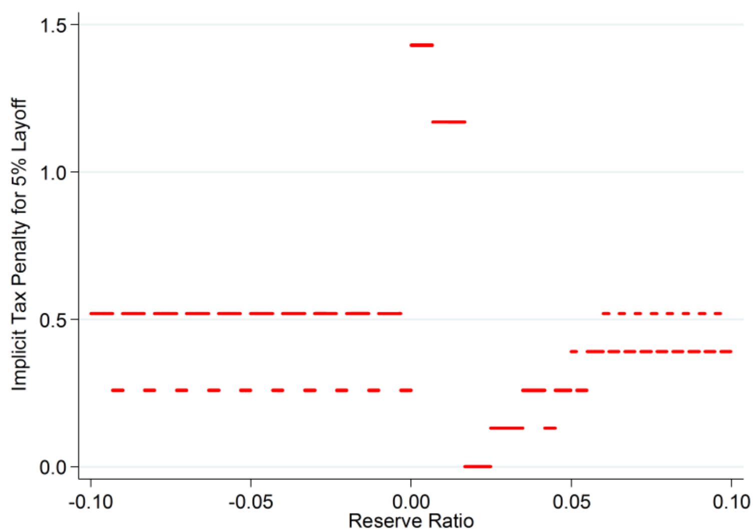
Note: This figure plots the empirical distribution of the RKD placebo estimates on hiring. I estimate the RKD at each point from the minimum running variable to 0.5 (on a scale that goes from 0.0 to 99.99) while keeping the bandwidth constant, and I exclude points in the region of the true kink and points with sparse data at the left boundary. This process produces 450 placebo estimates, only one of which is smaller than estimate from the true kink.

Online Appendix Figure V

A. Discontinuity in Missouri Tax Rate Schedule

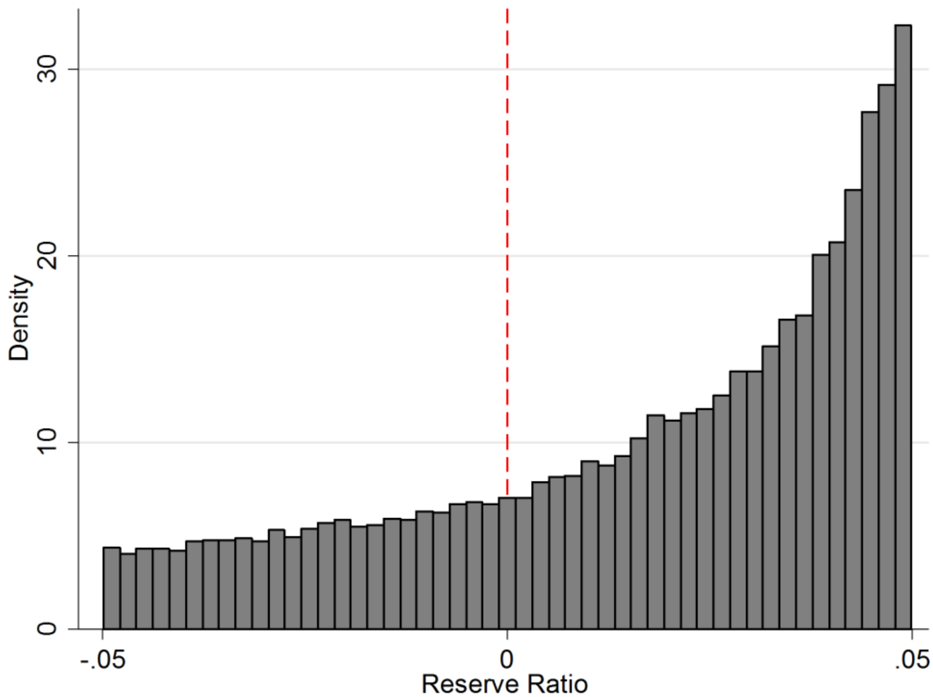


B. Implied Discontinuities in Missouri Tax Penalties



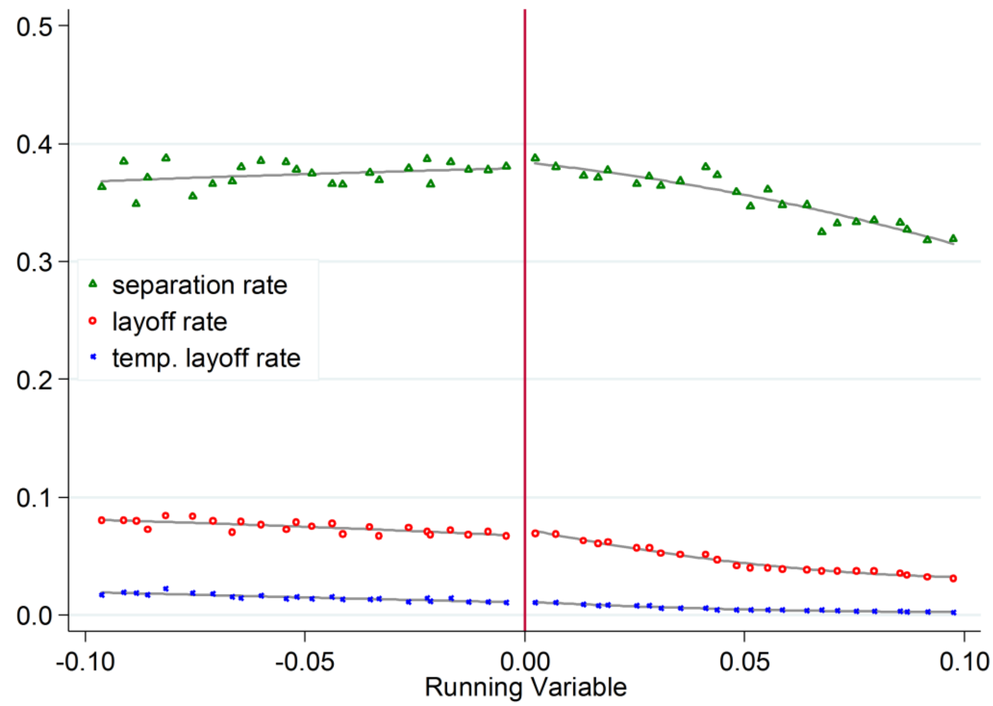
Note: This figure shows the tax schedule applied to firms in Missouri. The state calculates a reserve ratio which reflects the firm's UI reserves divided by the average wage base over the past three years. Tax rates are generated when the state submits firm reserve ratios to a consistent tax schedule, seen above in part. The schedule is a series of discrete jumps, but one large discontinuity exists in the tax schedule, representing a 1.17 point tax increase for firms at the cutoff.

Online Appendix Figure VI
Missouri Firm Density: Test for Manipulation



Note: This figure represents the number firms (firm-years) in bins around the discontinuity while excluding new firms. There is no bunching to suggest strategic sorting. The data are from administrative records from Missouri's Department of Labor.

Online Appendix Figure VII
RD Deterrence Test (Missouri)



Note: The data are administrative records from the state of Missouri 2003–2012. Firms just to the right of the vertical line face a higher penalty for layoffs than firms just to the left. A firm on the right will face a 1.22-point higher tax penalty (1.43 on the right, 0.21 on the left) in response to a five-percent layoff since they are near a large step on the tax function. Firms are informed of their placement on the tax schedule and the tax rates they face at each point on the schedule. Consistent with the regression analysis, there is little in terms of a systematic relationship between higher penalties and a lower propensity to layoff or separate from workers.

Online Appendix Figure VIII
Sample Unemployment Tax Statement from Missouri

MODES-527(08-14)
CONT.
DES-CER212A(08-14)

MISSOURI DEPARTMENT OF LABOR AND INDUSTRIAL RELATIONS
DIVISION OF EMPLOYMENT SECURITY
P.O. BOX 59, JEFFERSON CITY, MO. 65104

TELEPHONE
573-751-1995

EMPLOYER'S EXPERIENCE RATING STATEMENT AND NOTICE OF CONTRIBUTION RATE FOR CALENDAR YEAR 2015

PREPARED AS OF CALCULATION DATE JULY 1, 2014

ACCOUNT NUMBER: [redacted]

DATE OF MAILING MONTH DAY YEAR	ELIGIBLE FOR RATE CALCULATION FOR 2015
[redacted]	YES: X NO:

VERIFICATION

THE TAXABLE WAGE BASE FOR 2015 (SEC. 288.036 WILL BE **$13,000** FOR EACH EMPLOYEE.

1/2 YR PERIODS	TAXABLE WAGES	CONTRIBUTIONS	BENEFITS CHARGED	ACCOUNT BALANCE
	SUMMARY OF ACTIVITY PRIOR TO DETAIL --->>> SHOWN BELOW	$33,203.62	$98,298.65	
2011 2	14,483	1,263.65	3,435.14	
2012 1	32,837	2,988.17	1,234.56	
2012 2	35,467	991.80	3,813.06	
2013 1	26,640	2,597.40	9,352.02	
2013 2	15,906	3,421.66	2,486.00	
2014 1	28,346	2,763.81	8,826.00	
AVERAGE ANNUAL PAYROLL	$43,036	$45,359.36	$133,374.42	$88,015.06- DR
YOU ARE ELIGIBLE FOR 2015 RATE CALCULATION AND YOUR AVERAGE ANNUAL PAYROLL IS ONE-THIRD OF THE TOTAL TAXABLE WAGES REPORTED DURING THE THIRTY-SIX (36) MONTH PERIOD JULY 1, 2011 - JUNE 30, 2014.		THIS AMOUNT INCLUDES ALL CONTRIBUTIONS PAID BY YOU (OR YOUR PREDECESSOR, IF APPLICABLE) SINCE THE ACCOUNT BECAME LIABLE UNDER THE MISSOURI EMPLOYMENT SECURITY LAW.	THIS AMOUNT INCLUDES ALL BENEFITS CHARGED AGAINST THE ACCOUNT (YOU OR YOUR PREDECESSOR, IF APPLICABLE) SINCE IT BECAME LIABLE UNDER THE MISSOURI EMPLOYMENT SECURITY LAW.	THIS AMOUNT IS THE TOTAL OF CONTRIBUTIONS CREDITED MINUS TOTAL BENEFITS CHARGED. IT IS NOT AN AMOUNT DUE.

YOUR RATE IS DETERMINED UNDER SECTION 288.120 BASED ON THE PERCENTAGE EXCESS FACTOR.
THE ACCOUNT BALANCE IS DIVIDED BY THE AVERAGE ANNUAL PAYROLL TO ARRIVE AT THE PERCENTAGE EXCESS FACTOR OF -99.99 PERCENT.

A MAXIMUM RATE SURCHARGE OF ONE AND ONE-HALF PERCENT HAS BEEN ADDED TO YOUR 2015 RATE. THIS IS REQUIRED UNDER SECTION 288.120 OF THE MISSOURI EMPLOYMENT SECURITY LAW.

2015 RATES INCLUDE A CONTRIBUTION RATE ADJUSTMENT OF THIRTY PERCENT DUE TO THE AVERAGE FUND BALANCE. THIS IS REQUIRED UNDER SECTION 288.121 OF THE MISSOURI EMPLOYMENT SECURITY LAW.

CONTRIBUTION RATE (288.120)	6.000%
MAXIMUM RATE SURCHARGE (288.120)	+ 1.500%
	7.500%
CONTRIBUTION RATE ADJUSTMENT (288.121) 30% OF 7.500 =	+ 2.250%
YOUR CONTRIBUTION RATE FOR CALENDAR YEAR 2015--->>>	9.750% (.09750)

RIGHT OF APPEAL

THIS DETERMINATION OF YOUR CONTRIBUTION RATE BECOMES FINAL 30 CALENDAR DAYS AFTER THE DATE OF MAILING SHOWN ABOVE UNLESS, WITHIN SUCH 30 CALENDAR DAYS A **SIGNED** WRITTEN REQUEST FOR A HEARING IS FILED WITH THE DIVISION OF EMPLOYMENT SECURITY, P.O. BOX 59, JEFFERSON CITY, MISSOURI, 65104, ATTENTION: EMPLOYER ACCOUNTS UNIT, OR FAXED TO 573-751-7910. THIS REQUEST MUST BE MADE BY: THE EMPLOYING UNIT (INCLUDING ANY OFFICER OR EMPLOYEE OF IT) OR BY A LICENSED ATTORNEY REPRESENTING THE EMPLOYING UNIT.

4E 2

2016 RATE TABLE

30% CRA increase applies

Max Rate may also have a *.25 surcharge (if max rate for 2yrs); **.50 surcharge (if max rate for 3yrs); *.75 surcharge (if max rate for 4yrs); **** 1.00 surcharge (if max rate for 5 yrs); *****1.50 surcharge (if max rate for 6 yrs)**

288.120. Employer's contribution rate, how determined – exception shared work plan, how computed.

-1. On each June thirtieth, or within a reasonable time thereafter as may be fixed by regulation, the balance of an employer's experience rating account, except an employer participating in a shared work plan under section 288.500, shall determine his contribution rate for the following calendar year as determined by the following table:

Percentage the Employer's Experience Rating Account is to that Employer's Average Annual Payroll

Equals or Exceeds	Less Than	Rate Table	Rate (30%)
		*****	9.750
		****	9.100
		***	8.775
		**	8.450
		*	8.125
--	-12.0	6.0%	7.800
-12.0	-11.0	5.8%	7.540
-11.0	-10.0	5.6%	7.280
-10.0	-9.0	5.4%	7.020
-9.0	-8.0	5.2%	6.760
-8.0	-7.0	5.0%	6.500
-7.0	-6.0	4.8%	6.240
-6.0	-5.0	4.6%	5.980
-5.0	-4.0	4.4%	5.720
-4.0	-3.0	4.2%	5.460
-3.0	-2.0	4.0%	5.200
-2.0	-1.0	3.8%	4.940
-1.0	0	3.6%	4.680
0	2.5	2.7%	3.510
2.5	3.5	2.6%	3.380
3.5	4.5	2.5%	3.250
4.5	5.0	2.4%	3.120
5.0	5.5	2.3%	2.990
5.5	6.0	2.2%	2.860
6.0	6.5	2.1%	2.730
6.5	7.0	2.0%	2.600
7.0	7.5	1.9%	2.470
7.5	8.0	1.8%	2.340
8.0	8.5	1.7%	2.210
8.5	9.0	1.6%	2.080
9.0	9.5	1.5%	1.950
9.5	10.0	1.4%	1.820
10.0	10.5	1.3%	1.690
10.5	11.0	1.2%	1.560
11.0	11.5	1.1%	1.430
11.5	12.0	1.0%	1.300
12.0	12.5	0.9%	1.170
12.5	13.0	0.8%	1.040
13.0	13.5	0.6%	0.780
13.5	14.0	0.4%	0.520
14.0	14.5	0.3%	0.390
14.5	15.0	0.2%	0.260
15.0	----	0.0%	0.000

2. Using the same mathematical principles used in constructing the table provided in subsection 1 of this section, the following table has been constructed. The contribution rate for the following calendar year of any employer participating in a shared work plan under section 288.500 during the current calendar year or any calendar year during a prior three-year period shall be determined from the balance in such employer's experience rating account as of the previous June thirtieth, or within a reasonable time thereafter as maybe fixed by regulation from the following table:

SHARED WORK

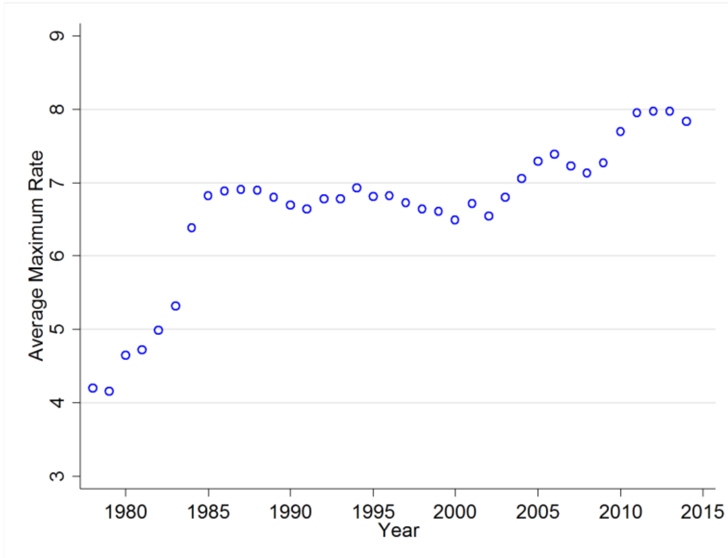
Percentage the Employer's Experience Rating Account is to that Employer's Average Annual Payroll

Equals or Exceeds	Less Than	Rate Table	Rate (30%)
		*****	13.650
		****	13.000
		***	12.675
		**	12.350
		*	12.025
---	-27.0	9.0%	11.700
-27.0	-26.0	8.8%	11.440
-26.0	-25.0	8.6%	11.180
-25.0	-24.0	8.4%	10.920
-24.0	-23.0	8.2%	10.660
-23.0	-22.0	8.0%	10.400
-22.0	-21.0	7.8%	10.140
-21.0	-20.0	7.6%	9.880
-20.0	-19.0	7.4%	9.620
-19.0	-18.0	7.2%	9.360
-18.0	-17.0	7.0%	9.100
-17.0	-16.0	6.8%	8.840
-16.0	-15.0	6.6%	8.580
-15.0	-14.0	6.4%	8.320
-14.0	-13.0	6.2%	8.060
-13.0	-12.0	6.0%	7.800
-12.0	-11.0	5.8%	7.540
-11.0	-10.0	5.6%	7.280
-10.0	-9.0	5.4%	7.020
-9.0	-8.0	5.2%	6.760
-8.0	-7.0	5.0%	6.500
-7.0	-6.0	4.8%	6.240
-6.0	-5.0	4.6%	5.980
-5.0	-4.0	4.4%	5.720
-4.0	-3.0	4.2%	5.460
-3.0	-2.0	4.0%	5.200
-2.0	-1.0	3.8%	4.940
-1.0	0	3.6%	4.680
0	2.5	2.7%	3.510
2.5	3.5	2.6%	3.380
3.5	4.5	2.5%	3.250
4.5	5.0	2.4%	3.120
5.0	5.5	2.3%	2.990
5.5	6.0	2.2%	2.860
6.0	6.5	2.1%	2.730
6.5	7.0	2.0%	2.600
7.0	7.5	1.9%	2.470
7.5	8.0	1.8%	2.340
8.0	8.5	1.7%	2.210
8.5	9.0	1.6%	2.080
9.0	9.5	1.5%	1.950
9.5	10.0	1.4%	1.820
10.0	10.5	1.3%	1.690
10.5	11.0	1.2%	1.560
11.0	11.5	1.1%	1.430
11.5	12.0	1.0%	1.300
12.0	12.5	0.9%	1.170
12.5	13.0	0.8%	1.040
13.0	13.5	0.6%	0.780
13.5	14.0	0.4%	0.520
14.0	14.5	0.3%	0.390
14.5	15.0	0.2%	0.260
15.0	----	0.0%	0.000

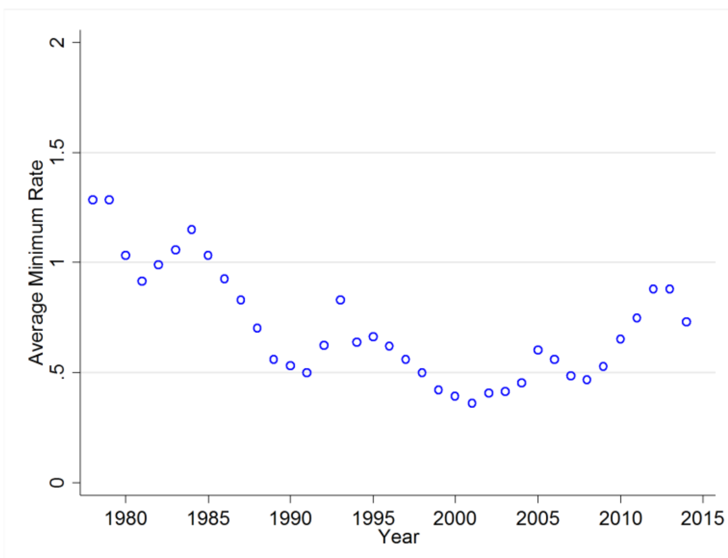
3. Notwithstanding the provisions of subsection 2 of section 288.090, any employer participating in a shared work plan under section 288.500, who has not had at least twelve calendar months immediately preceding the calculation date throughout which his account could have been charged with benefits shall have a contribution rate equal to the highest contribution rate in the table in subsection 2 of this section, until such time as his account has been chargeable with benefits for the period of time sufficient to enable him to qualify for a computed rate on the same basis as other employers participating in shared work plans.

Online Appendix Figure IX
Evolution of UI Tax Systems over Time in the United States

A. The Average Maximum Tax Rate over Time

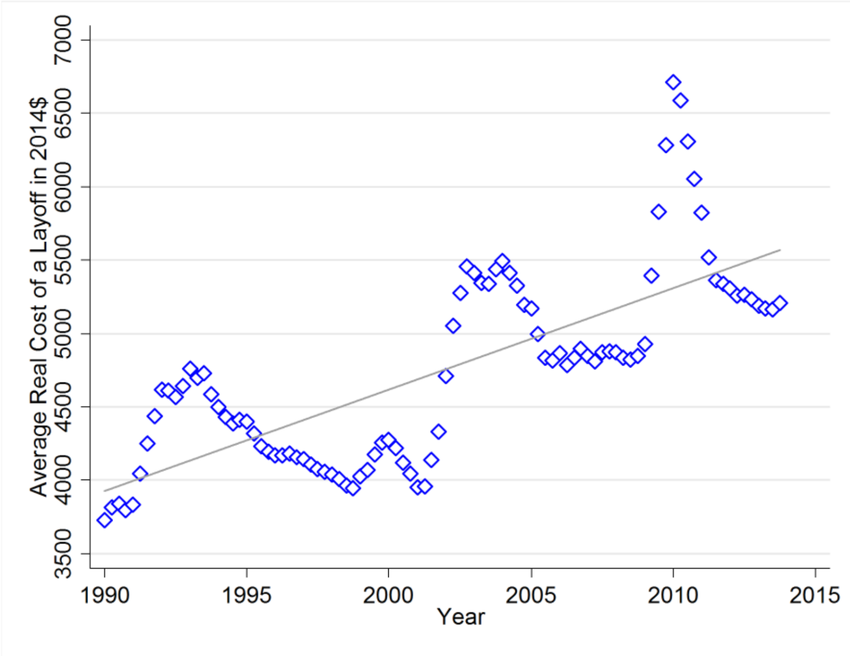


B. Average Minimum Tax Rate over Time



Note: The dots represent the average maximum/minimum UI tax rate across states in the U.S. from 1978 to 2015. There was a significant increase in maximum rates around 1985, when the federal law, TEFRA, induced states to raise their maximum rate substantially. In recent years, the rising maximum rate was instigated by states to help cover the shortfalls in UI trust funds. I calculated these averages from Commerce Clearinghouse UI Data (CCUID), which include the minimum and maximum rate each state had in place in each year, provided by the Employment and Training Administration (ETA) of the Department of Labor (DOL). Source: Author's calculation of CCUID data provided by ETA.

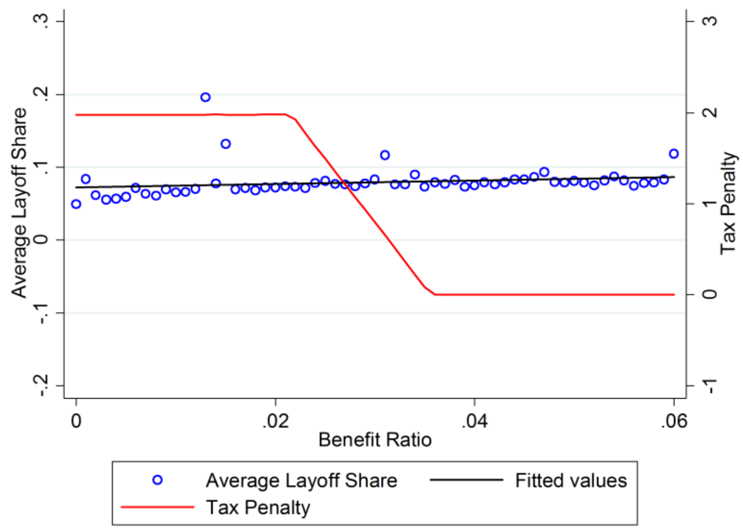
Online Appendix Figure X
Average Cost of Unemployment Spells over Time



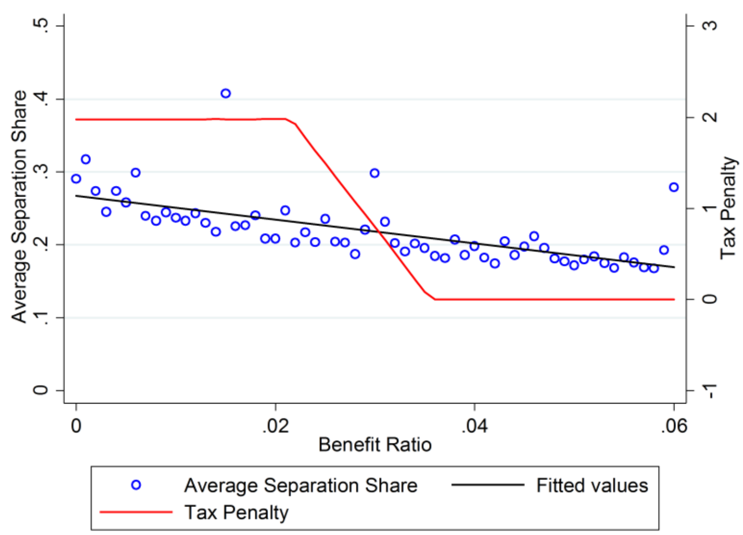
Note: The diamonds represent the average benefit cost of a layoff across states over time from 1990 to 2015 which I calculated from Unemployment Insurance Data Summary (UIDS). Most of the cost growth in this time period is driven by longer unemployment spells. Some growth comes from increases in real benefit generosity around 2001. Source: Author's calculation of UIDS data provided by US Department of Labor.

Online Appendix Figure XI
Non-Parametric Deterrence in Florida

A. Penalties and Layoff Share



B. Penalties and Separation Share



Note: These figures present non-parametric evidence on the deterrence effect. The red line represents the marginal tax a firm faces for a given sized layoff. The potential tax increase declines as the firm approaches the maximum rate when the firm is shielded from additional tax increases. Each circle represents the layoff- or separation-rate within benefit ratio bins. Consistent with the regression analysis, there is little in terms of a systematic relationship between higher penalties and a lower propensity to layoff or separate from workers.

Online Appendix Tables

Online Appendix Table I
Balance Test: Predicted Outcomes across Threshold

Outcome	RKD (1)	RKDFE (2)	RKDFE (3)	RKDFE (4)	RKD (5)
Hiring	0.011	0.011	0.010	-0.006	0.004
	(.001)	(.001)	(.001)	(.003)	(.003)
	749,318	749,318	805,013	805,013	(.003)
Employment	0.004	0.005	0.009	0.01	0.030
	(.008)	(.008)	(.004)	(.011)	(.022)
	476,800	476,800	767,589	767,589	206,414
Firm Exit (×100)	-0.002	0.001	-0.006	0.005	0.000
	(.005)	(.005)	(.002)	(.005)	(.016)
	261,124	261,124	659,101	659,101	117,036
Log Ave. Wage (×100)	0.008	0.002	0.007	0.000	-0.039
	(.014)	(.014)	(.005)	(.016)	(.046)
	256,006	256,006	611,651	611,651	121,030
Layoffs (×10)	0.004	0.003	0.007	-0.002	0.007
	(.005)	(.005)	(.002)	(.005)	(.014)
	327,004	327,004	741,623	741,623	148,557
Controls					
Year Fixed Effects	X	X	X	X	X
Firm Fixed Effects		X	X	X	
Linear Control	X	X	X		X
Quadratic Control				X	
Bandwidth (times optimal)	1	1	2	2	½

Note: This table serves as a covariate balance test. I use all predetermined covariates (firm age, firm age squared, year, entity type, county, and detailed industry code) to predict each outcome. Then I use these predicted outcomes and estimate a placebo kink effect. The estimates are precise, small, and very close to zero, suggesting that in absence of the tax kink, outcomes would likely be smooth across the kink point.

Online Appendix Table II
Main RKD Placebo Estimates

PLACEBO USING OUTCOME FROM OTHER YEARS

Outcome	RKD (1)	RKDFE (2)	RKDFE (3)	RKDFE (4)	RKD (5)
Hiring (t–3)	0.127	0.041	0.365	-0.529	0.080
	(.144)	(.162)	(.079)	(.199)	(.421)
	258,879	258,879	537,492	537,492	119,455
Employment (t–3)	0.822	0.980	2.455	-0.22	0.745
	(.431)	(.446)	(.210)	(.602)	(1.496)
	151,683	151,683	335,633	335,633	73,074
Firm Exit (t+3)	0.003	0.002	0.001	0.001	0.004
	(.002)	(.002)	(.001)	(.002)	(.005)
	121,357	121,357	339,150	339,150	52,160
Log Ave. Wage (t–3)	0.013	0.006	0.004	0.005	-0.008
	(.002)	(.002)	(.001)	(.002)	(.005)
	198,953	198,953	426,658	426,658	92,643
Layoffs (t–3)	0.167	0.128	0.336	0.228	-0.034
	(.035)	(.040)	(.019)	(.047)	(.106)
	219,994	219,994	488,356	488,356	103,333
Controls					
Year Fixed Effects	X	X	X	X	X
Firm Fixed Effects		X	X	X	
Linear Control	X	X	X		X
Quadratic Control				X	
Bandwidth (times optimal)	1	1	2	2	½

Note: This table serves as a placebo test in which each coefficient reflects the causal estimate from a separate regression. I use the same specifications as those in the main results but use outcome variables from a placebo year.

Online Appendix Table III
Kink Heterogeneity by Cash-Constraint

Outcome	RKDFE 2003–2007 (1)	RKDFE 2008–2012 (2)	RKDFE High Cash (3)	RKDFE Low Cash (4)	RKDFE H. Cash Flow (5)	RKDFE L. Cash Flow (6)
Hiring	0.03	–0.72	–0.69	–1.45	–0.98	–1.04
Rob. St. Err.	(.234)	(.172)	(.164)	(.285)	(.166)	(.283)
Ave outcome	*28.2*	*16.5*	*17.4*	*29.2*	*18.2*	*27.9*
Employment	–0.57	–1.165	–0.65	–2.15	–0.74	–2.05
Rob. St. Err.	(1.129)	(.805)	(.324)	(.584)	(.324)	(.586)
Ave outcome	*64.1*	*52.6*	*49.9*	*70.5*	*51.4*	*68.4*
Exit	0.003	–0.001	0.001	0.001	–0.003	0.007
Rob. St. Err.	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Ave outcome	*0.06*	*0.08*	*0.07*	*0.07*	*0.07*	*0.07*
Firm/Year FE	X	X	X	X	X	X
Linear Control	X	X	X	X	X	X

Note: This table serves as a heterogeneity exploration. I estimate the preferred specification on subsamples that focus on firms in more or less cash constrained periods or industries.

Online Appendix Table IV
Missouri Kink Estimates

Outcome	**Hiring** (1)	**Hiring** (2)	**Empl.** (3)	**Empl.** (4)
Main Effect	–0.25	–0.32	0.33	–0.26
	(.094)	(.100)	(.476)	(.448)
	215,837	431,825	215,837	431,825
Placebo Effect	–0.032	0.05	1.36	0.58
	(.086)	(.092)	(.534)	(.597)
	215,837	431,825	215,837	431,825
Controls				
Firm Fixed Effects	X	X	X	X
Linear Control	X		X	
Quadratic Control		X		X
Bandwidth (times .02)	1	2	1	2

Note: This table generates the estimated kink in hiring and employment at Missouri's minimum rate using administrative UI program records.

Online Appendix Table V
Panel Data and RDD to Estimate Deterrence in Missouri

Outcome	Panel (1)	Panel (2)	Panel (3)	Panel (4)	RDD (5)	RDD (6)
All Layoffs	-0.020	0.009	-0.001	0.000	0.003	-0.001
Cls. St. Err.	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
(Ave=.033)						
Temporary Layoffs	-0.003	0.001	0.000	0.000	0.001	0.000
Cls. St. Err.	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
(Ave=.004)						
Year Fixed Effects	X	X	X	X		X
Firm Fixed Effects		X	X	X		X
Controls (RR & log-size)			X	X		
Tax-Rate Control				X		
Local Linear RDD					X	
Quadratic RDD						X
Observations	802,465	802,465	802,465	802,465	802,465	802,465

Note: This table presents the estimates of the deterrence effect using DID and RDD. The coefficients represent the estimated effect of a one-point tax increase response to layoffs.

Online Appendix Table VI
Deterrence in Industry-State Panel

	Δlog (Employment)			
	(1)	(2)	(3)	(4)
Panel A: All Years (1986–2013)				
Deterrence	0.001	0.001	-0.006	0.000
	(0.002)	(0.002)	(0.005)	(0.005)
Negative Shock	-1.106	-1.106	-1.107	-1.094
	(0.023)	(0.027)	(0.027)	(0.027)
Panel B: Recessionary Years Only				
Deterrence	0.002	0.002	-0.015	-0.012
	(0.002)	(0.002)	(0.006)	(0.006)
Negative Shock	-1.018	-0.910	-0.924	-0.894
	(0.033)	(0.052)	(0.052)	(0.053)
Industry FE		X	X	X
State FE			X	X
Weekly Benefit				X

Note: This table presents the estimates of the deterrence effect using national data from the County Business Patterns in. The negative shock represents a percent-unit reduction in industry employment outside the state, often used as a measure of demand shocks. Its inclusion here is to provide a basis for comparison for the *deterrence effect*. Deterrence would be reflected in positive coefficients, particularly in contraction years. Instead, the estimates are usually small and insignificant. The few cases where the estimates are statistically different from zero suggest that higher tax penalties accompany larger reductions in employment.

MISSOURI DATA AND TAX BACKGROUND

Missouri provided data parallel to that which Florida provided. It is full-population, firm-worker data encompassing a wage file, firm file, and claims file. The data include each firm's UI account balance, taxable payroll, UI reserve ratio, UI tax rate, and six-digit NAICS industry code. I follow the same course as described for Florida to calculate firm employment, hiring, separations, and exit. I exclude firms that are not experience-rated. The dataset represents 271,223 unique firms. The average firm has 19.1 employees, and each year the average firm hires 6.8 employees and separates from 4.4 employees. The average employee is paid $32,620. In a typical year, 8.7 percent of firms exit. As in Florida, I measure the number of layoffs using successful claims in the benefits data.

The system by which Missouri manages tax rates for each firm differs slightly from Florida's benefit ratio program. Unlike Florida, Missouri uses a *reserve ratio* model in which UI tax payments and charges are credits and debits in each firm's unemployment account. Every year, the state calculates the reserve ratio for each firm, which is the account's reserves divided by the firm's average taxable wages over the past three years:

$$RR_t = \frac{RES_t}{\frac{1}{3}\sum_{i=0}^{3-i} W_{t-i}}$$

Intuitively, the reserve ratio measures what the firm owes as a fraction of its payroll. The state inputs each firm's reserve ratio into the tax schedule, a tiered step function that does not change from year to year (see Online Appendix Figure II). The discontinuity in the schedule allows for a careful comparison of similar firms just above and below the cutoff who face different penalties for layoffs. Another important distinction between the two programs is that Florida's tax rate automatically resets after three years, even when a firm has not paid the cost of the benefits charged. In reserve-ratio states like Missouri, the tax rate does not decline until the firm has reimbursed the state for the cost of benefits charged against the employer. Intuitively, this is because the state keeps track of individual accounts rather than simply the cost of benefits charged over the past three years.

DETERRENCE

I leverage variation in the penalties that arise from a discontinuity in the tax penalty firms face because of their location on the tax schedule (Online Appendix Figure V). Firms just to the right of the cutoff face significantly higher penalties for layoffs than those just to the left. The discontinuity allows for a careful comparison of similar firms who face different penalties for layoffs based on a reduced-form regression discontinuity (RD) model:

$$y_{it} = f(RR_{it}; \lambda) + T_{it}\beta + \varepsilon_{it}$$

where y_{it} represents a firm's layoff rate (layoffs as a percentage of last year's employment) for firm i at time t; RR_{it} represents the firm's reserve ratio; f is a function that is continuous at the threshold with parameters λ; T is an indicator for whether the firm is on the high-penalty side of the threshold; and ε is an error term reflecting other factors. The base control function f is local-linear regression within the optimal bandwidth in which the control is interacted with the T indicator to allow the slope of layoffs to vary on either side of the threshold. I also show the estimates are robust to polynomial controls and wider bandwidths. The coefficient β represents the effect of higher penalties on the layoff rate, and negative values of β reflect deterrence. Standard errors are clustered by firm.

The key assumption underlying the RD analysis is that assignment to either side of the threshold is functionally random (Lee 2008; Imbens and Lemieaux 2008). The assumption fails if firms precisely manipulate their location on the tax schedule. As discussed in the main body of the paper, precise manipulation is essentially impossible for a number of reasons. Moreover, manipulation would be evident if firms accumulated on the favorable side of the threshold. Online Appendix Figure V illustrates that firm density trends smoothly across the threshold, suggesting no manipulation (McCrary 2008). Even without manipulation, a regression discontinuity design can be compromised if other programs rely on the same cutoff or if firm characteristics change discontinuously at the threshold. Missouri's Department of Labor determines no other policies based on the reserve ratio and these records are kept private from other bureaus, precluding a confounding policy discontinuity. In addition to testing individual covariates, I construct an index of each outcome variable using all predetermined covariates available in the data. I use detailed firm industry code, observation year, and firm age to predict each firm's layoff rate, temporary layoff rate, and separation rate (Online Appendix Figure VI). I take the predicted values from these models and conducted an RD analysis to assess covariate balance. These tests suggest that characteristics evolve continuously at the cutoff, implying balanced covariates. I also estimate placebo "effects" of the cutoff on past outcome variables. There is no discernable evidence of placebo effects, suggesting that the penalty varies as though randomly at the threshold.

Consistent estimation of β requires that $f(RR_{it};\lambda)$ be specified correctly. I follow the recent RD literature and use a non-parametric local-linear model. Though unbiased, these estimates can be imprecise because they focus on data close to the threshold. I also experiment with models using low-order polynomials which generate similar results. To verify that my estimates are not the result of specification choices I graph the relationship between layoffs and the reserve ratio, providing visual evidence of the estimate.

In the results presented in Online Appendix Table V, I scale β by the estimated discontinuity in tax penalties (a rate increase of 1.2 percent, on a $13,000 per-worker tax base, amounting to a $156 penalty per employee for a modal layoff of 5 percent). Thus, the reported coefficient reflects the effect of a one-point higher UI tax in response to a layoff, and negative coefficients reflect deterrence. Firms facing higher penalties do not have lower rates of either layoffs or temporary layoffs; instead, they have slightly higher rates of layoffs (0.3 percent), suggesting no deterrence. The results are similar when using quadratic RD controls and firm fixed-effects, suggesting the null estimate isn't the product of firm heterogeneity.

To increase power, I exploit richer variation in the penalties firms confront based on their location on the tax schedule, not just those at the largest discontinuity. Intuitively, penalties vary depending on how near a firm is to the next step of the tax schedule and the height of the next step. I calculate the tax penalty each firm would receive for a modal layoff (five percent of their workers) depending on where they are in the schedule. The calculated tax penalties range from 0 percent to 1.4 percent and these penalties are not strongly correlated with a firm's current tax rate or other firm characteristics. Exploiting the panel structure of the data, I enrich the test using within-firm comparisons to account for unobserved heterogeneity among firms.

$$y_{it} = P_{it}\beta + \psi(RR_{it};\gamma) + \alpha_t + \delta_i + X_{it}\Gamma + \varepsilon_{it}.$$

Here, y represents a firm's layoff rate, P represents the tax penalty (the calculated tax penalty for a five-percent layoff in terms of a tax-rate increase in percentage-point units), RR represents a firm's reserve ratio, ψ is a flexible polynomial of the firm's reserve ratio with parameters γ, α_t captures time-specific factors influencing layoffs, δ_i controls for fixed firm characteristics, X_{it} represent log firm size and the firm tax rate, and ε represents other factors. Online Appendix Table V columns 3–6 displays the results. Like the RD, these panel specifications suggest small and insignificant effects of tax penalties on layoffs. The effects range from –0.1 to 0.1 percent with tighter standard errors than those available in the RD.

Nonparametric plot also demonstrate little systematic relationship between penalties and layoffs (Online Appendix Figure VII).

What effect size should we expect? In theory, a layoff penalty has the same effect as a wage reduction on of the decision to lay off a worker since the employer will pay the dismissal cost regardless. Therefore, average estimates of labor demand elasticities (0.5) (Katz 1996; Lichter, Peichl and Siegloch 2015) suggest that firms should reduce their layoffs by 6.6 percent, which is equivalent to an estimated –0.0022 (6.6 percent times .033 layoff rate).[29] The confidence intervals on the deterrence estimates tend to rule out magnitudes of this size, suggesting that firms are less responsive to penalties than we would expect.

It is possible these penalty differences are not salient. Importantly, the state informs firms of their location on the tax schedule and the rates around their location in yearly UI statements (see sample statement in Online Appendix Figure VIII). In the paper, I performed a similar test to that above using a natural experiment in Florida in which the maximum rate shields firms from tax increases in a nonlinear way that gives rise to an instrumental variables strategy. Because the maximum is binding, persistent, and well-known, firms intuitively should be aware that the potential penalty is smaller for firms nearer the maximum rate. The results using this natural experiment mirror those from the Missouri RD, suggesting larger penalties fail to deter layoffs (Online Appendix Table V). As in Missouri, the estimates rule out deterrence effects predicted by economic theory. Possibly, local differences in tax rates fail to deter because they are relatively minor. To probe this explanation, I exploit variation in experience rating across states and over time, to estimate whether industries in states that allow for larger penalties are less likely to downsize, using data from the County Business Patterns:[30]

$$\Delta \log(\mathrm{y_{ist}}) = T_{ist}\beta + Bartik_{is't} + \gamma_i + \alpha_s + \delta_t + \varepsilon_{ist}$$

Here, $\mathrm{y_{ist}}$ represents the employment of industry i in state s at time t; T_{ist} represents the maximum per-employee tax normalized by the standard deviation of the maximum per-employee tax in the data; $Bartik$ represents the changes in log-employment in industry i in all other states at time t representing labor demand shocks. Fixed effects for industry (γ_i) and state (α_s) account

[29] A 1-point tax increase will raise $3,990, about 80 percent of the cost of an average unemployment spell in recent years.
[30] To investigate whether the deterrence effects are consistent with macro level data, I construct a panel of industries in each state from the County Business Patterns (CBP) and link them to data on UI tax limits in each state over time from the Unemployment Insurance Data Summary (UIDS) and Commerce Clearinghouse UI Data (CCUID) covering 1986–2012.

for differential trends among geographies or industries, and δ_t captures broad time-based changes in labor demand. Deterrence would imply a positive coefficient β on tax exposure, T. Importantly, tax exposure (taxable wage base × maximum rate) reflects the scope for tax increases rather than the average tax rate. The maximum rate has little to do with average tax rates, while increasing the range for punishing layoffs.

In Online Appendix Table VI, I present the results of this analysis. An industry facing a one-unit negative Bartik shock reduces its employment by 0.9 percent. But industries in states with higher tax penalties are no less likely to downsize under a number of specifications (Panels A and B, columns 1–4). Lazear (1990) and Anderson (1993) argue that tax penalties should have the largest effect during downturns when dismissal costs are expected to increase employment. Instead, I find that industries downsize *more* when exposed to higher tax penalties during recessionary years, with the confidence intervals ruling out a deterrence effect.

In the conceptual framework, I hypothesized that taxes may not deter firms if they are cash-constrained. Intuitively, firms that need layoffs to cover their costs and survive this period cannot be discouraged by a future penalty. I test this by performing the analysis on subsamples that are likely to be more and less cash-constrained. First, I bifurcate the data into recessionary years (2008–2012) and expansionary years (2003–2007). As before, I also use Compustat to identify the average cash-per-employee and average cash-flow-per-employee of each three-digit NAICS industry. For each measure of cash-constraint, I split the sample above and below the median and estimate the deterrence effect separately using panel estimation (polynomial of the running variable, firm FE, tax and size controls) to maximize efficiency. Consistent with cash-constraints, layoffs are deterred by penalties in the expansionary period, but not the recessionary one. A one-point increase in tax penalties reduces layoffs by 0.5 percentage points ($p < 0.02$). The estimated effects do not vary significantly by measure of industry cash-constraint over the period. This may be because industry cash levels may be a noisy measure for a particular firm's cash-constraint (Farre-Mensa and Ljungqvist 2015).