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Unemployment Insurance Taxes and Labor Demand: Quasi-Experimental Evidence from Administrative Data[†]

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To finance unemployment insurance, states raise payroll tax rates on employers who engage in layoffs. Tax rates are, therefore, highest for firms after downturns, potentially hampering labor-market recovery. Using full-population, administrative records from Florida, I estimate the effect of these tax increases on firm behavior leveraging a regression kink design in the tax schedule. Tax hikes reduce hiring and employment substantially, with no effect on layoffs or wages. The results imply unanticipated costs of the financing regime which reduce the optimal benefit by a quarter and account for 12 percent of the unemployment in the wake of the Great Recession. (*JEL* D22, E24, H25, H32, H71, J23, J65)

Employers pay a *dynamic* payroll tax to finance unemployment insurance (UI) benefits, an instance of experience rating. Under this regime, each firm has its own tax rate that rises in response to layoffs and falls when the firm avoids them. Because tax rates are linked to layoffs, firms face higher payroll taxes as they emerge from downturns, and troubled firms bear the largest tax increases. A potential unintended consequence of this regime is that it may discourage hiring when unemployment is already high, and firms are under strain.[1] The literature has focused on the role UI benefits play in prolonging joblessness through reduced labor supply (Farber, Rothstein, and Valletta 2015; Hagedorn, Manovskii, and Mitman

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[1] 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, and limits that range by almost an order of magnitude across the country. The maximum per-employee tax varies from $440 in Arizona, California, and Florida to $3,100 (seven times larger) in North Dakota, Minnesota, and Virginia. These large differences reflect disagreement among policymakers regarding the impact of mechanical tax hikes on distressed firms

2016; Johnston and Mas 2018), generally ignoring how UI taxes may affect employment by depressing labor demand (Anderson and Meyer 1997a). The objective of this paper is to fill that gap and measure the impact of UI tax increases on the demand side. I call this influence the "overhang effect," since UI tax hikes are a hangover from layoffs.

Measuring this effect empirically is challenging. First, large microdata combining UI tax rates and firm behavior are not publicly available. Second, tax rates are endogenous by design, making it difficult to separate the causal effect of taxes from unobserved, confounding factors.[2] In this paper, I employ new, full-population microdata on all workers and firms in Florida, linking firm behavior to tax records. I exploit quasi-experimental variation to disentangle the effect of tax rates from underlying stress. Specifically, I use a regression kink design (RKD), which leverages the kink in the tax schedule as a function of a firm's benefit ratio,[3] providing variation in the tax rate that is independent of other factors (Card et al. 2015). Firms near the kink point have recently had layoffs, so the local average treatment effect (LATE) generated by this variation precisely captures the influence described previously as the "overhang effect."

The results reveal that UI taxes reduce hiring and employment, with no discernible effect on layoffs, worker earnings, or firm exit. A 1 percentage point increase in a firm's UI tax rate reduces firm hiring by 2.8 percent and employment by 1.5 percent. Assuming no spillovers and the standard caveats regarding external validity, these estimates suggest that the overhang effect during the recent recovery accounted for 12 percent of unemployment in 2010 and 2011, and that changes in experience rating and UI costs explain about a quarter of the joblessness of the recent recovery.[4] There is no evidence firms manipulate their position on the tax schedule and, in practice, fine manipulation is extremely difficult since firms cannot control the costs of former employees with any precision. The results are unaffected when using a within-firm RKD, implying unobserved firm heterogeneity does not drive the results. The validity of the research design is further supported by the smooth evolution of predetermined covariates, uniform pre-trends across the kink, a variety of specification checks, nonparametric evaluations, and permutation exercises.

The firm response is substantial, the equivalent of a labor demand elasticity of 4.[5] While the response is large, analysts find responses with the same elasticity when studying a program that mirrors UI taxation in which labor costs were temporarily affected by hiring credits during the Great Recession (Cahuc, Carcillo, and

[2] Even in panel data, simultaneity is a crucial concern. Higher taxes are likely to reduce firm employment, but increases in firm-specific unemployment translate to higher taxes as well. Consequently, OLS estimates of the effect of taxes likely misrepresent the true magnitude, the bias of which depends largely on the sign of serial correlation in employment changes.

[3] The benefit ratio is the cost of UI benefits charged to the firm over the past three years divided by the taxable wage base over that same period.

[4] If hiring at one firm crowds out hiring at another, the implied unemployment effect found by scaling the estimates linearly may be overstated. If hiring at one firm instead promotes hiring elsewhere (employment raises incomes that increase labor demand at firms serving the newly employed), then the implied unemployment effect may be understated.

[5] Translating the effect size into an elasticity merely helps the reader evaluate the effect size relative to traditional labor demand responses and does not reflect an attempt to estimate a true elasticity generalizable to non-UI settings.

Le Barbanchon 2019).[6] Researchers register similar elasticities in the only other study seeking to estimate the relationship between UI tax rates and labor demand (Anderson and Meyer 1997a). My paper builds on this work using design-based methods to measure the influence of UI taxes on firm behavior. This paper explores potential mechanisms behind the large effects, including that (i) the regime functions as a head tax and (ii) affected firms appear to be cash constrained.

Because cash-constrained firms have little alternative than to reduce labor costs, they can exhibit substantial labor-demand reductions in response to cost increases. This prediction is borne out in data where labor demand is two to four times more responsive in cash-constrained industries. Firms that recently have had layoffs, moreover, behave as though they are cash constrained in other ways: they have higher exit rates, less hiring, more layoffs, and they are not deterred from additional layoffs by tax penalties—a hallmark of cash constraints. The substantial firm response is partially explained by the fact that UI taxes act like a head tax. That is, UI imposes a tax on *employment* itself, rather than payroll (Acemoglu and Shimer 1999, Chetty 2006). In theory, the labor demand response is substantially larger when introducing a head tax than an equivalent payroll tax, and the effect should be larger among low-wage firms because the tax does not scale with earnings. Accordingly, low-wage firms are several times more responsive to the UI tax kink than their high-paying peers.

Though a large and active literature surrounds UI benefits,[7] little has been done to understand the impacts of the unique tax structure that finances them. Most UI tax research has focused on the consequences of experience rating for temporary layoffs, a relatively small part of the modern labor market (Feldstein 1976, Topel 1983, Wolcowitz 1984, Card and Levine 1994). An exception to this is the work of Anderson and Meyer (1997a, 2000), who demonstrate that the influence of UI taxation is far broader. The authors relate endogenous tax rates to employment, using a sample covering firms in the early 1980s.[8] The present study extends and complements the literature, making three contributions. First, I exploit newly available administrative UI records capturing the universe of firms, workers, and claims in recent data from a large state. Second, this paper is the first to break the simultaneity of tax rates and firm distress using a regression kink design. Third, the paper proposes and tests reasons for the large labor demand response found here and in prior work.

The results contribute to a few areas. First, UI taxes track the business cycle, which may diminish the macroeconomic stabilizing influence of UI.

[6] The authors study the influence of a hiring credits program in France, *zéro charges*. The policy was implemented unexpectedly during the recession and reduced payroll taxes on new hires. Parallel to UI taxes, the policy temporarily altered labor costs at a time when firms were under strain. The main difference is that the tax credit provided by *zéro charges* applies to new hires only, whereas increases in UI tax rates affect the cost of all employees.

[7] See, for instance, Meyer (1989); Landais, Michaillat, and Saez (2010); Schmieder, von Wachter, and Bender (2012); Kroft, Lange, and Notowidigdo (2013); Hagerdorn, Manovskii, and Mitman (2015); Farber, Rothstein, and Valletta (2015); Card et al. (2015); and Johnston and Mas (2018).

[8] My study also relates to a literature on payroll tax incidence (Hamermesh 1979; Kugler and Kugler 2008; Bennmarker, Mellander, and Öckert 2009; Saez, Matsaganis, and Tsakloglou 2012; Egebark and Kaunitz 2014; Saez, Schoefer, and Seim 2019). The results here suggest that incidence changes markedly when tax rates fluctuate and are applied to firms in stress.

Second, optimal UI formulae assume taxes reduce wages while imposing no impact on employment and thus produce no economic loss (Baily 1978, Chetty 2006). The evidence here suggests that taxes reduce employment—not wages—diminishing welfare and reducing the optimal benefit calculation by 26 percent (Gruber 1994). Third, the influence of the tax sheds light on the impact of other mandates that implicitly tax employment. Finally, the results explore an unintended consequence of UI experience rating for countries considering the practice, principally in Europe and South America (Fath and Fuest 2005, Simonetta 2017).

I. Background on Unemployment Insurance Taxation

The US federal government mandates that each state administer a UI program, under which laid-off workers receive weekly benefits. While unemployed, workers receive a weekly payment that replaces approximately half of their earnings for up to six months in normal times. To finance these benefits, firms pay a dynamic payroll tax. A firm's tax rate *increases* when former employees collect more in benefits, and a firm's tax rate falls when former employees don't claim UI. Tax rates update at the beginning of each year. The assigned tax rate applies to each establishment of the firm, much like other business taxes, and there are no notable cutouts: states require UI participation for firms of all sizes, no matter how small, and for all industries.

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

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

The Florida Department of Economic Opportunity uses each firm's benefit ratio to calculate individualized tax rates, $\tau_{it} = \min\left(\alpha_t + (1+\lambda_t) \times BR_{it}, 5.4\right)$ for firm i in calendar year t.[9,10] Parameters α and λ are chosen by the state and vary slightly from year to year based on a need to balance revenues with the expense of payments.[11] Tax rates rise with the benefit ratio until the rate reaches the maximum, 5.4 percent. Thus, the max generates a *kink* in the tax rate as a function of the benefit ratio, as seen in Figure 1.[12] In the empirical analysis, I uncover the impact of the tax by measuring corresponding kinks in firm behavior. Many firms reside around

[9] Florida's Department of Economic Opportunity is equivalent to the Department of Labor in other states.

[10] Notice that the cost of a given layoff affects a firm's benefit ratio and tax rate for three years; thus, tax rates for a given firm are serially correlated even if layoffs are not.

[11] For interested readers, the specific values of α and λ in Florida each year can be found in online Appendix Table 1.

[12] To provide context, a firm would need to lay off 4.4 percent of its workers in a single year to go from the minimum rate to the maximum rate. This calculation uses the average tax-formula parameter values and the average cost of a UI spell in 2012 ($5,200).

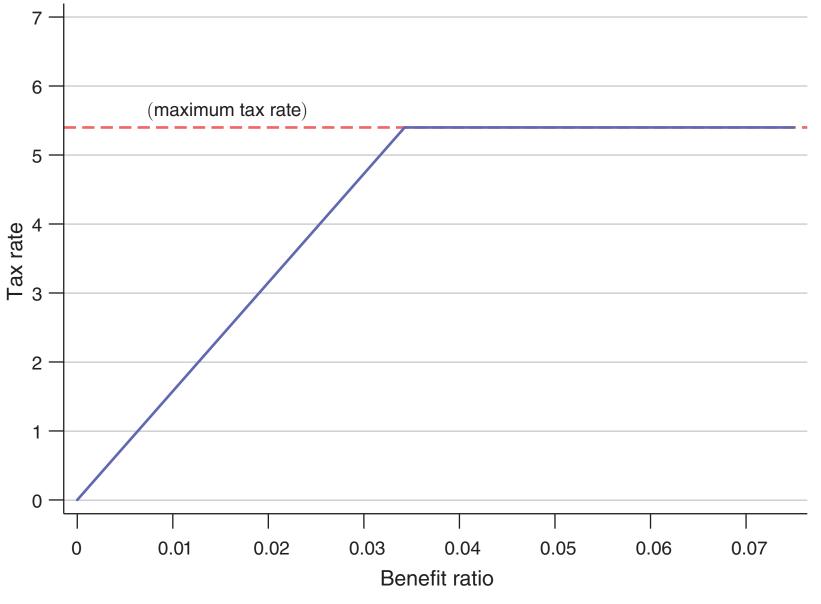


Figure 1. The Kink in the Tax Formula (Tax Schedule in Florida)

Notes: This figure shows the UI tax formula in Florida in 2010. The tax is a linear function of the benefit ratio, subject to a maximum. Intuitively, the benefit ratio reflects the per-employee cost of a firm's layoffs. The tax rate is capped at 5.4 percent, which generates a kink in the tax as a function of the benefit ratio.

Florida's kink due to the state's low maximum rate (one of the several states that uses the minimum allowable maximum rate under federal law) and large population (the third largest in the country), which, together, provide statistical power at the kink point. New firms, those without three years' experience, are not rated and instead pay a fixed rate of 2.7 percent until they have three years of employment history.

Firms pay their assigned tax rate only on the taxable wage base of each employee. In Florida and a dozen other states, this means an employer pays its marginal tax rate only on the first $7,000 it pays each worker during the year, after which the firm pays a zero-marginal tax rate on all wages paid to that worker until the new year. Thus, for most employees, UI taxes function as a *head tax*—a tax on the quantity of workers a firm employs rather than a tax on total wages paid.

Whereas marginal rates automatically fall during recessions under a progressive income tax, UI rates *rise* in response to downturns.[13] Figure 2 demonstrates how unemployment and UI tax payments vary over time. The tax bill rises quickly while unemployment is high and tends to peak before employment fully recovers, with the tax closely following the pattern of the unemployment rate, lagged by eight quarters. After the 2001 recession, the average per-worker tax bill increased by 40 percent.

[13] A progressive income tax, by contrast, automatically reduces average marginal tax rates during recessions and increases rates during periods of growth and high income. Whereas progressive taxes attenuate the magnitude and persistence of shocks (Blundell, Graber, and Mogstad 2015), the UI tax may magnify them.

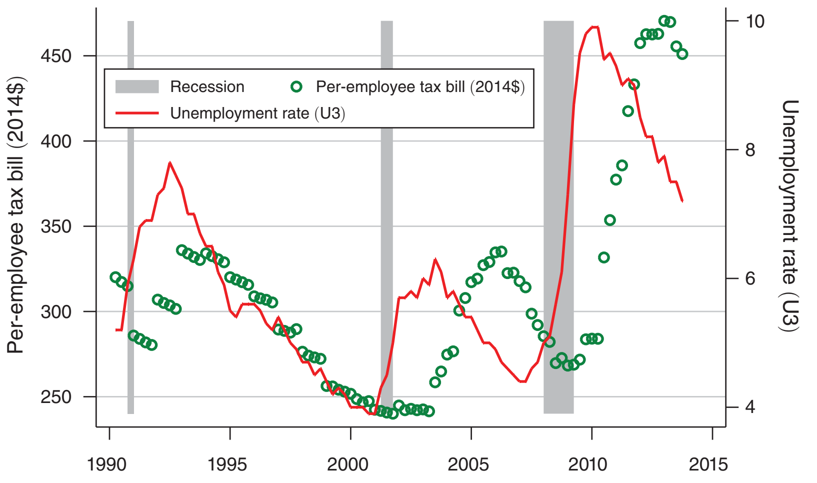


Figure 2. Unemployment Insurance Taxation over the Business Cycle

Notes: The open dots represent the average per-employee tax paid by employers in 2014 dollars using the Bureau of Labor Statistics' Unemployment Insurance Data Summary (UIDS). The continuous line represents the average unemployment rate using U3. The shaded periods are NBER-designated recessions.

After the 2008 recession, the per-worker tax bill increased by 70 percent.[14] Figure 2 obscures enormous heterogeneity in which distressed firms confront much larger increases than stable firms who see essentially no change in their rate over the business cycle. In states with the highest experience rating, the most distressed firms face taxes up to seven times the peak shown in Figure 2.

II. Administrative Data

To understand the effects of UI taxes, one needs longitudinal data on workers and firms that are linked to UI tax rates. I obtained new data. The data are full-population, administrative records for the universe of workers and firms in Florida running from 2003 to 2012, with each worker or firm identified by a unique ID.[15] 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 people who received benefits during the period.[16] The wage file indicates the earnings each worker received from each of his employers in each quarter; the firm file includes each firm's tax rate, benefit ratio, benefit cost, wage base, NAICS industry code, and county; the claims file indicates which employees claimed benefits, the cost of

[14] Marriner S. Eccles, the chairman of the Federal Reserve Board under Franklin D. Roosevelt and a forerunner to John Maynard Keynes, advocated for the inverse of this policy in which the federal government would *raise* payroll tax rates in good times and reduce them during recessions as countercyclic policy (Israelsen 1994).

[15] 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 1 employee working at least a portion of any day during any 20 weeks of a calendar. The coverage includes businesses, nonprofit organizations, state or local government employers, and Indian tribal units (Florida State Government 2014). In practice, all lawful employers are included in the data.

[16] 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.

benefits they received, and which employer was charged. Because the administrative earnings records are based on firms' tax filings and are subject to audit, they avoid predictable measurement issues that arise in survey data (Bound and Krueger 1991).

I use these records to calculate the employment of each firm, as well as the number of new hires and layoffs. A firm's employment is the number of employees who received payroll at firm i during calendar year t. The number of new hires is the count of employees at firm i in year t who did not work at the firm in the previous year; the hiring rate divides this number by employment. Separation tracks the number of employees with positive earnings at firm i in year t who do not work at the firm in $t + 1$. Layoffs measure the number of employees working at firm i in year t, who collect unemployment benefits within a year of separating from the firm. Temporary layoffs reflect layoffs who return to the firm within a year of separating. Median earnings reflect typical quarterly earnings of workers at a firm. Firm exit is coded in the last year a firm is observed in the data.

In the main analysis, I exclude firms that are too new to be experience rated and those that are at the minimum rate, since they do not face tax variation related to the kink.[17] Table 1 presents summary statistics. In the cleaned sample, the average firm employs 58.8 workers with significant turnover (column 3). Each year, a representative firm hires 46.8 percent of its employees and separates from 45.9 percent. For reference, public-use LEHD data suggest essentially identical churn rates (see online Appendix Figure 1). A total of 4.9 percent of an average firm's employees claim benefits (a measure of layoffs), and 6 percent of those are recalled to their original employer within a year of receiving benefits. About 7.9 percent of firms exit annually, with smaller, newer firms exiting at a higher rate. The median worker receives yearly earnings of $21,600. The average firm pays a UI payroll tax rate of 1.9 percent and has a benefit ratio of 0.069.

To assess whether firms around the kink are like other firms, I present the summary statistics for all firms and for firms in relevant subgroups approaching the sample around the kink. Firms at the kink are older and larger, though they have similar rates of hiring, separation, and layoffs. The reader will notice enormous heterogeneity in firm size and behavior as evidenced by the SDs of each field; to limit the influence of outliers on the results, I winsorize hiring, employment, and earnings at the 0.1 percent tails, like Edmans (2012) and Lachowska, Mas, and Woodbury (2018).[18]

Though the data are well tailored to the question, they have important limitations. It is not possible to distinguish layoffs from separations if the employee does not claim UI, a clear source of measurement error. Since layoffs are a dependent variable, however, such measurement error will not likely lead to bias in the estimates.[19]

[17] In Florida firms do not receive a firm-specific tax rate for their first three years of operation; firms too new to be experience rated make up 29 percent of firms, but employ only 9 percent of workers. Instead of paying an experience-rated tax, they pay a fixed rate of 2.7 percent, and the state keeps record of the firm's benefit charges and wage base to calculate an individualized tax rate after three years of experience are accumulated. Firms with no charges tend to be smaller, stable firms like insurance offices and family restaurants.

[18] That is, if a value is above the 99.9th percentile, its value is set the value of the 99.9th percentile of the distribution.

[19] About half of eligible unemployed workers claim benefits (Anderson and Meyer 1997b).

Table 1—Summary Statistics

	All firms	Rated firms	Rated firms with non-zero benefit ratios	Rated firms, nonzero BR, near kink point
Firm size	26.0	33.4	58.8	51.9
	(411)	(487)	(659)	(476)
Hiring rate	0.447	0.429	0.468	0.444
	(7.69)	(7.09)	(5.53)	(4.07)
Separation rate	0.431	0.413	0.459	0.402
	(7.17)	(6.61)	(5.30)	(3.22)
Layoff rate	0.041	0.040	0.049	0.052
	(0.04)	(0.04)	(0.05)	(0.05)
Temporary layoff rate	0.002	0.002	0.003	0.003
	(0.06)	(0.06)	(0.05)	(0.05)
Median earnings	5,427	5,863	6,581	7,018
	(57,504)	(60,727)	(19,236)	(13,030)
Exit rate	0.079	0.067	0.074	0.074
	(0.27)	(0.25)	(0.26)	(0.26)
Tax rate	1.86	1.51	3.08	4.17
	(0.02)	(0.02)	(1.95)	(1.14)
Benefit ratio	0.069	0.030	0.066	0.026
	(2.42)	(1.24)	(1.74)	(1.14)
New firm	0.291	0.000	0.000	0.000
	(0.45)	(0.00)	(0.00)	(0.00)
Observations	2,596,948	1,841,585	706,779	269,758

Notes: Cleaned data from Florida DEO comparing firms in subsamples approaching the analytic sample seen in column 4. A firm's employment is the number of employees who received payroll at firm i during calendar year t. The number of new hires is the count of employees at firm i in year t who did not work at firm i in the year previous; the hiring rate divides new hires by employment. The separation rate tracks the number of employees at firm i in year t who do not work at firm i in $t+1$, over employment. The layoff rate is based on the number of employees working at firm i, who collect unemployment benefits within a year of departing. The temporary layoff rate reflects the share of employees who are laid off and return to the firm within a year of separating. Median earnings are the typical quarterly earnings of workers at firm i in year t. Firm exit is coded in the last year a firm is observed.

The data, moreover, cover only legal employment relationships, leaving unobserved any informal arrangements.

III. The Overhang Effect

Experience rating in the UI program raises taxes in times, industries, and locations where unemployment is already common. To protect firms under strain, Florida caps taxes so that rates cannot rise above 5.4 percent, generating a kink in firm tax rates as a function of the benefit ratio. Since the maximum tax rate in Florida is low compared to other states, it is well populated and firms there do not appear substantially different from other firms (see Table 1). The kink is situated at the eighty-ninth percentile of layoff histories (i.e., the 89th percentile of the benefit ratio).[20] Importantly, the kink identifies the effect of the tax among firms who have

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

recently had layoffs, meaning the local average treatment effect (LATE) recovered by the kink precisely reflects the impact described as the "overhang effect."

The tax schedule generates a kink in tax rates, allowing the analyst to compare the behavior of similar firms whose tax rates vary independent of underlying firm factors, conditional on a few controls (Card et al. 2015). The panel nature of the data provides a unique opportunity to estimate the impact of tax rates using within-firm comparisons over time. I model a firm's behavior y (principally in terms of hiring, employment, exit, layoffs, and log average worker earnings) 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 the tax rate. The tax formula changes slightly from year to year and a small number of firms have rates that do not comply with the formula (fewer than 0.01 percent), so I implement a fuzzy RKD, instrumenting rates with the prevailing kink in the tax formula:

$$(2) \quad R_{it} = \alpha_t + \delta_i + \sum_{p=1}^{\overline{p}} \left\{ \gamma_p (v_{it} - k_t) + \pi_p (v_{it} - k_t) \cdot D_{it} \right\} + u_{it},$$

$$(3) \quad y_{it} = \mu_t + \eta_i + \beta \hat{R}_{it} + \sum_{p=1}^{\overline{p}} \left\{ \omega_p (v_{it} - k_t) \right\} + \varepsilon_{it}, \quad \text{all where } \left| v_{it} - k_t \right| \leq h.$$

Here, R_{it} is the (endogenous) UI tax rate of firm i in year t; v_{it} is the running variable (the benefit ratio), and k_t is the value of the running variable at the kink point in year t; $D_{it} = \mathbf{1}(v_{it} > k_t)$ indicates a firm being above the kink threshold in a given year; ω_p accounts for the relationship between the outcome and the running variable; and h is the bandwidth. Year fixed effects flexibly model secular time trends. The linear coefficient on $(v_{it} - k_t) \cdot D_{it}$ captures the average slope change in tax rates at the kink point, which provides the identifying variation for β, the effect of a one-point tax increase. The preferred specification relies on a local-linear approach, but the design yields similar results using a quadratic specification. The first stage is quite strong (see Figure 3 and online Appendix Table 2). The instrument has a t-statistic of 1,416 and the model explains more than 98 percent of the variation in tax rates around the kink.[21] To preserve hiring and layoff observations of zero, the specification estimates the effect in levels rather than logs. Aiding interpretation, I divide the estimated level effects in hiring, layoffs, and employment by the average employment around the kink $(P = \hat{\beta}/E[S \,|\, \text{abs}(v - k) < 0.02])$, so the estimates reflect the impact of the tax on hiring, layoffs, and employment changes as percentages of employment. By the same logic, I log-transform earnings to present coefficients interpretable as percent changes. Throughout the analysis, standard errors are clustered at the firm level to account for correlated outcomes within a firm over time.

To assess the influence of unobserved firm differences on the estimates, I leverage the panel structure of the data. The estimates are highly robust to firm fixed effects. This robustness demonstrates that the estimates are not the result of spurious firm

[21] The model explains less than 100 percent of the tax variation because the schedule changes slightly from year to year and there are a small number of firms whose tax rates do not follow the formula (see online Appendix Table 1).

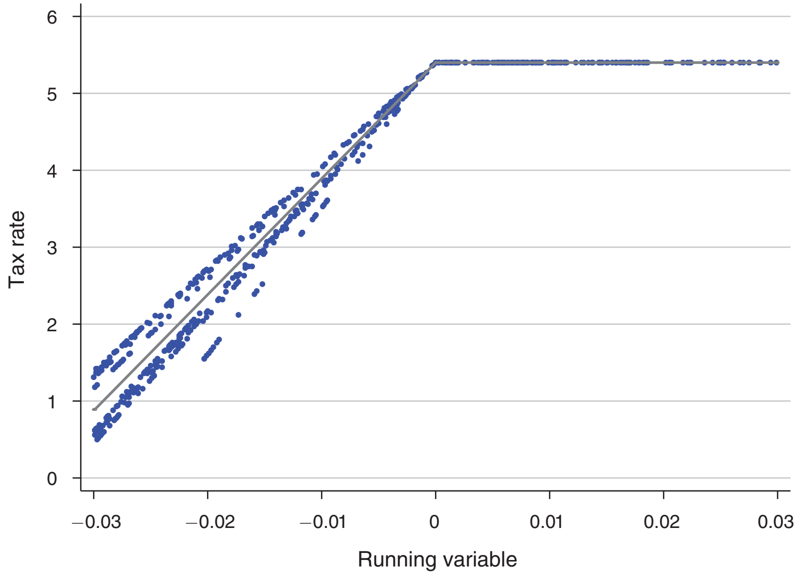


Figure 3. First Stage in Florida Tax Formula

Notes: The figure plots a random sample of firms around the tax kink to demonstrate the first stage. The dispersion in this figure reflects the fact that the tax formula changes slightly from year to year. To see how the parameters of the tax function evolved over time, see online Appendix Table 1.

Source: Administrative data are from the Florida Department of Economic Opportunity (DEO).

differences around the kink.[22] I use Imbens-Kalyanaraman optimal bandwidths and show that the estimates are remarkably robust to a wide variety of bandwidth choices (Card et al. 2015; Imbens and Kalyanaraman 2012; Calonico, Cattaneo, and Titiunik 2014), where the optimal bandwidths are estimated on the full sample of rated firms. The typical IK bandwidth is about 0.02 (in terms of the benefit ratio). The bandwidth effectively restricts the analysis to firms within a narrow region around the kink point, implicitly comparing the behavior of firms with similar layoff histories that confront different tax rates owing to the tax schedule. Comparisons within this restricted window address concerns that observations at the maximum are radically different from other firms in the sample. All the firms within the usual IK bandwidth of the kink differ by less than 0.05 standard deviations of the benefit ratio, suggesting that even the highest-rated and lowest-rated firms in the analytic sample are quite similar in their layoff profile.

[22] This within-firm 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. 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.

A. Diagnostics of the Natural Experiment

Unbiased estimation relies on two assumptions. First, the assignment variable must have a smooth effect on the outcome. Second, unobserved determinants of the outcome must evolve without kinking at the same point as the tax formula. Simply stated, other factors of firm behavior must be smooth at the kink point.

To explore whether other factors evolve smoothly around the kink, I test for kinks in predetermined covariates around the threshold by constructing covariate indices using all the predetermined covariates in the data (Card, Chetty, and Weber 2007). To construct an index, I regress each outcome variable (hiring, employment, layoffs, firm exit, and log average worker earnings) on all the predetermined firm characteristics including entity type (e.g., s-corporation, c-corporation, not-for-profit, trust, local government, etc.), two-digit NAICS industry code, proxies for firm age, county location, and observation year. Figure 4 plots each covariate index over the running variable, which shows covariates evolve smoothly across the kink point. To test for covariate balance more formally, I estimate RKD models using the covariate indices as the outcome variable, the results of which are presented in online Appendix Table 4. The estimated kinks in the covariate indices are extremely small and usually wrong-signed, suggesting that any estimated kink in firm behavior is not an artifact of observed covariates around the kink.[23] A related concern is that firms differ across the kink in their pre-trends. I estimate local-linear regression discontinuity models with pre-trends as the outcome variable. Consistently, the pre-trends are identical across the kink point (see online Appendix Table 5).

A key consideration for evaluation of the RKD design is whether firms can precisely manipulate their placement on the tax schedule, since this can generate a break in unobserved factors arising from selection (Lee and Lemieux 2010). Although firms can know the location of the kink, it is essentially impossible for them to precisely manipulate their placement. First, tax rates depend on the value of benefits drawn by former workers, a variable that firms would find difficult to control: both claiming (Anderson and Meyer 1997b) and the duration of their spell are outside of the firm's influence. Second, benefit costs increase in discrete weekly increments, making precise manipulation still more challenging. Finally, the schedule changes from year to year with little warning, rendering fine-tuning essentially impossible. More to the point, there is no *evidence* of strategic manipulation as firm density does not bunch or break around the kink point (see online Appendix Figure 2). Firm density thins as the benefit ratio rises, but there is no increased density on the favorable side of the kink. Following the intuition of McCrary (2008), I find no discontinuity or kink in firm density.

Other policies are not affected by the benefit ratio. I corresponded with Florida's program administrators who indicate that no other policies at the local, state, or federal level are based on the benefit ratio; in fact, such a policy would be impossible

[23] I investigate whether quasi-experimental variation in tax rates predicts tax differences in other years, which occurs mechanically to some degree because benefit charges affect a firm's benefit ratio for three years. A kink-induced 1-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 placebo in Figure 6, I use years further out, specifically three years before time t.

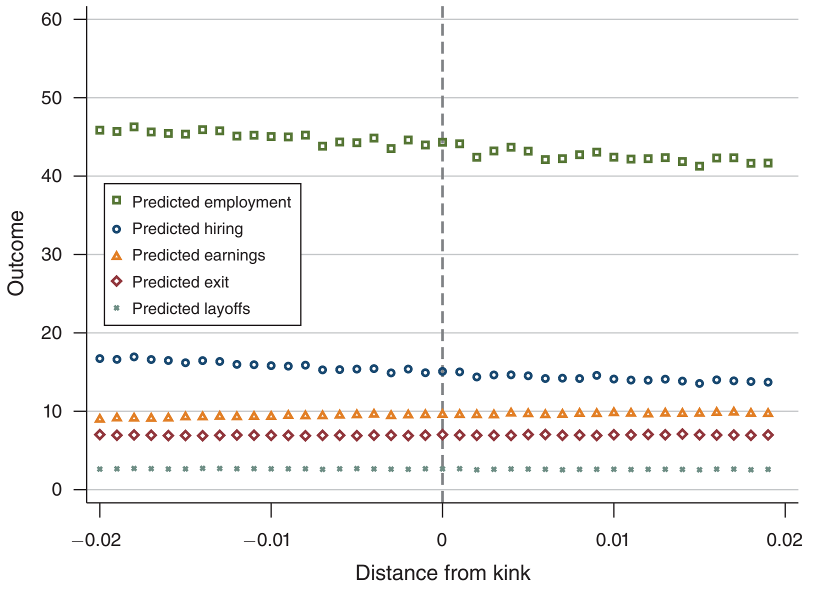


FIGURE 4. SMOOTHNESS OF PREDICTED VALUES AROUND THE KINK

Notes: This figure shows the predicted values of major outcome variables in bins around the kink point. In each, the outcome is predicted with NAICS codes, county, entity type (legal structure), a proxy for firm age, and firm age squared. The indices show that, based on predetermined covariates, predicted outcomes are smooth across the kink, suggesting that observable factors of the outcome variable do not confound RKD estimation.

Source: Administrative data are from the Florida Department of Economic Opportunity (DEO).

since no other agency has access to these statistics. In summary, there is no bunching, covariates and pre-trends evolve smoothly across the kink, the estimated placebo effects from the covariate indices are null, and other government policies do not change at the kink, suggesting that the setting represents a clean quasi-experiment for unbiased estimation of the influence of UI taxes on firm behavior.

B. Results and Their Robustness

The central findings are presented in Table 2. Each coefficient corresponds to a different regression estimating the effect of a one-point UI tax increase on firm hiring, employment, layoffs, exit, and log average earnings, each in percent terms. A one-point tax increase raises a firm's tax fee by $70 per employee (1 percent times the taxable wage base, $7,000), introducing a $3,580 tax increase for the average firm at the kink. This increase corresponds to about 0.3 percent of median yearly payroll and a 24 percent increase in average UI tax liability among firms around the kink.

The estimates suggest that a one-point tax increase reduces yearly firm hiring rate by 1.3 percentage points ($p < 0.001$, $t = 10.9$). Firm size falls by a corresponding 1.5 percent ($p < 0.001$, $t = 3.9$). The results are robust to including

Table 2—Regression Kink Estimates of Overhang Effect

Outcome	RKD (1)	RKDFE (2)	RKDFE (3)	RKDFE (4)	RKD (5)
Hiring rate	−0.014	−0.013	−0.013	−0.020	−0.024
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Employment (percent change)	−0.015	−0.015	−0.019	−0.013	−0.020
	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)
Firm exit	0.002	0.001	0.003	−0.003	0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Average earnings (percent)	0.008	0.001	−0.001	0.002	0.002
	(0.002)	(0.002)	(0.001)	(0.002)	(0.005)
Layoff rate	0.001	0.000	0.000	−0.006	0.000
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
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	0.5

Notes: Each coefficient represents a separate regression reflecting the RKD estimate of a 1-point increase in the tax rate on the outcome indicated on the left column. In the first stage, taxes are instrumented with the prevailing kink. The standard errors are presented under each estimate.

Source: Administrative data are from the Florida Department of Economic Opportunity (DEO).

firm-specific fixed effects, challenging objections that the kink in firm behaviors is an artifact of firm heterogeneity along the benefit ratio. It is reassuring that the hiring and employment estimates closely align. These estimates are in line with a labor demand elasticity of 4, with CIs ruling out elasticity equivalents smaller than 3, similar to the labor-demand elasticity found for hiring credits in Cahuc, Carcillo, and Le Barbanchon (2019).[24] Consistent with wage rigidity, tax changes do not affect worker earnings, another parallel with Cahuc, Carcillo, and Le Barbanchon. I find no evidence of cascading: tax increases do not induce additional layoffs, which would trigger further tax increases for firms not already at the maximum. Nor do tax increases affect firm exit, a fact that helps confirm that there is not selective attrition arising from the kink. The estimates are robust to varied bandwidths and polynomial specifications. To harness all of the available variation in the tax formula, I also provide estimates that allow the kink to differ each year in the first stage in Table 3. These results are similar but register somewhat larger effects on hiring and employment.

To evaluate the sensitivity of estimates to bandwidth, I vary the bandwidth while implementing the preferred specification (that from Table 2, column 2: linear controls, firm and year fixed effects), the results of which are shown in Figure 5 and online Appendix Figure 3. At bandwidths smaller than optimal, the estimates on hiring tend to be larger, but the estimates are remarkably stable, at around –0.013, from

[24] The tax reduces employment by about 1.2 percent and increases the wage bill by 0.3 percent.

Table 3—Regression Kink Estimates of Overhang Effect Using Yearly Kinks

	RKD	RKDFE	RKDFE	RKDFE	RKD
Outcome	(1)	(2)	(3)	(4)	(5)
Hiring rate	−0.016	−0.017	−0.016	−0.017	−0.027
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Employment (percent change)	−0.022	−0.024	−0.029	−0.053	−0.022
	(0.002)	(0.002)	(0.002)	(0.006)	(0.004)
Firm exit	0.002	0.001	0.002	−0.005	0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
log average earnings	0.015	0.002	0.000	0.003	0.006
	(0.002)	(0.002)	(0.001)	(0.003)	(0.005)
Layoff rate	0.003	0.001	−0.001	−0.006	0.000
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)
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	0.5

Notes: Each coefficient represents a separate regression reflecting the RKD estimate of a 1-point increase in the tax rate on the outcome indicated on the left column. In the first stage, there is a separate kink variable for each year, allowing the estimate to incorporate all the kink-related variation which changes slightly over time. The standard errors are presented under each estimate.

Source: Administrative data are from the Florida Department of Economic Opportunity (DEO).

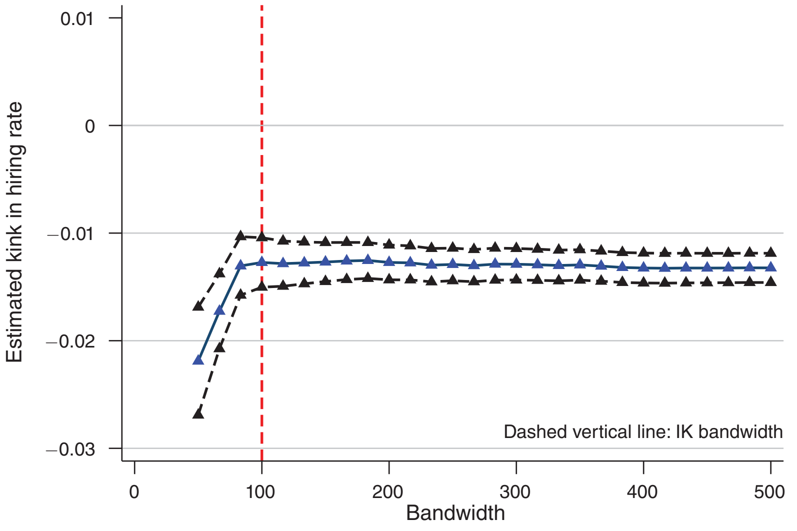
60 percent of the optimal bandwidth to 500 percent of the optimal bandwidth.[25] The main estimates are on the conservative side of the range of estimates. I also implement a score of placebo tests in which I reproduce the main table using outcomes from a previous year in online Appendix Table 6. Whereas the main effects are significant and robust, placebo estimates tend to be small, insignificant, or wrong-signed.

I present residualized plots that account for firm and year fixed effects in Figures 6 and 7 to visualize the kink in the outcomes that identifies the main effects. The figure shows a distinct kink in hiring. A parallel figure using the firm's hiring from a placebo year shows no kink, suggesting the role of the tax. The benefit ratio proxies for firm distress, as evidenced by the fact that firm exit and layoffs increase as the benefit ratio rises (see online Appendix Figures 4 and 5). Notice that the kink reflects the fact that hiring stops declining as the benefit ratio increases at the exact point where taxes stop rising. If the kink in hiring were spurious, we would expect the opposite relationship, in which higher benefit ratios corresponded to lower hiring. Figure 7 shows the corresponding kink in total employment and no break in firm exit.

To evaluate the likelihood of recovering the main estimates by chance, I estimate placebos at regular intervals along the benefit ratio and compare the empirical distribution of these placebos to the main estimate on hiring (see online

[25] An important question when implementing RKD is whether the analyst is misspecifying a curve with linear approximations (Ganong and Jager 2018). If the recovered behavioral kinks were the product of misspecifying a curve, narrower bandwidths would produce smaller (less negative) estimates and larger bandwidths would generate larger ones (more negative). Instead, the largest estimates exist at small bandwidths, suggesting the results are not the product of misspecification.

Panel A. Effect on hiring



Panel B. Effect on employment

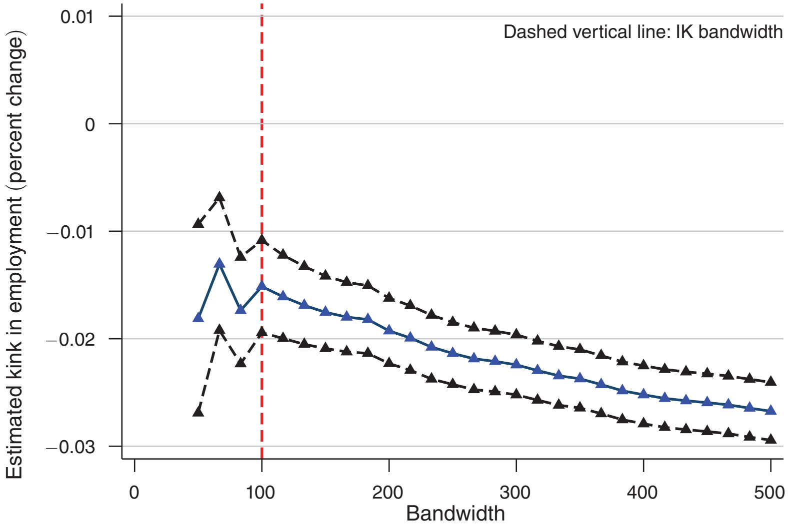


FIGURE 5. RKD ESTIMATES WITH VARYING BANDWIDTHS

Notes: Each center triangle represents the estimated effect of UI taxes for a given bandwidth with the accompanying standard errors. The red dashed line represents the optimal IK bandwidth.

Source: Administrative data are from the Florida Department of Economic Opportunity (DEO).

Appendix Figure 6).[26] This process generates 450 placebo estimates, which cluster near zero, averaging –0.001. Only one of the placebo estimates is smaller than the

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

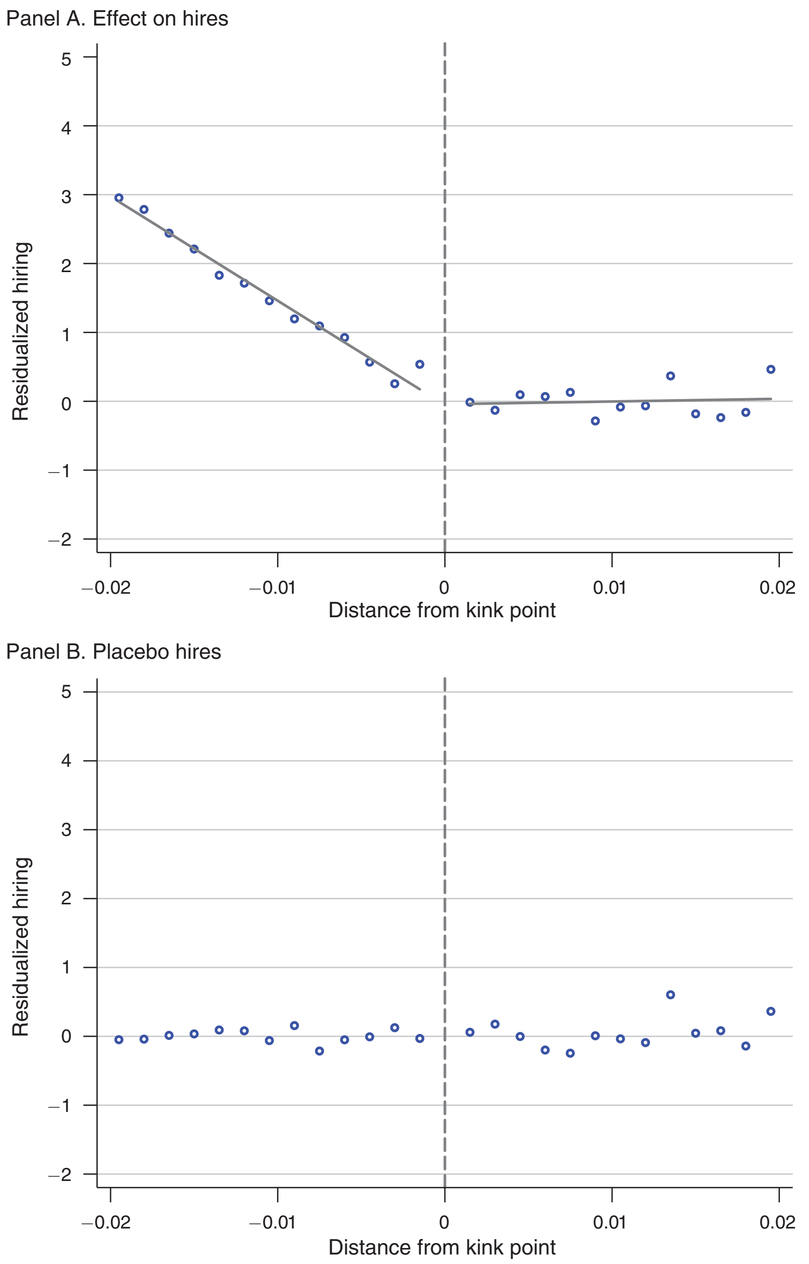


Figure 6. Hiring Kink and Placebo

Notes: The top figure shows the kink in the residuals accounting for firm and year fixed effects. The bottom figure shows placebo residuals using outcome data from a placebo year, $t - 3$.

main effect, suggesting the probability of randomly estimating an effect of this size is remote.

Supporting the validity of the results is the peculiar timing of the firm response, which closely follows the payment schedule. I examine the timing of the tax effect

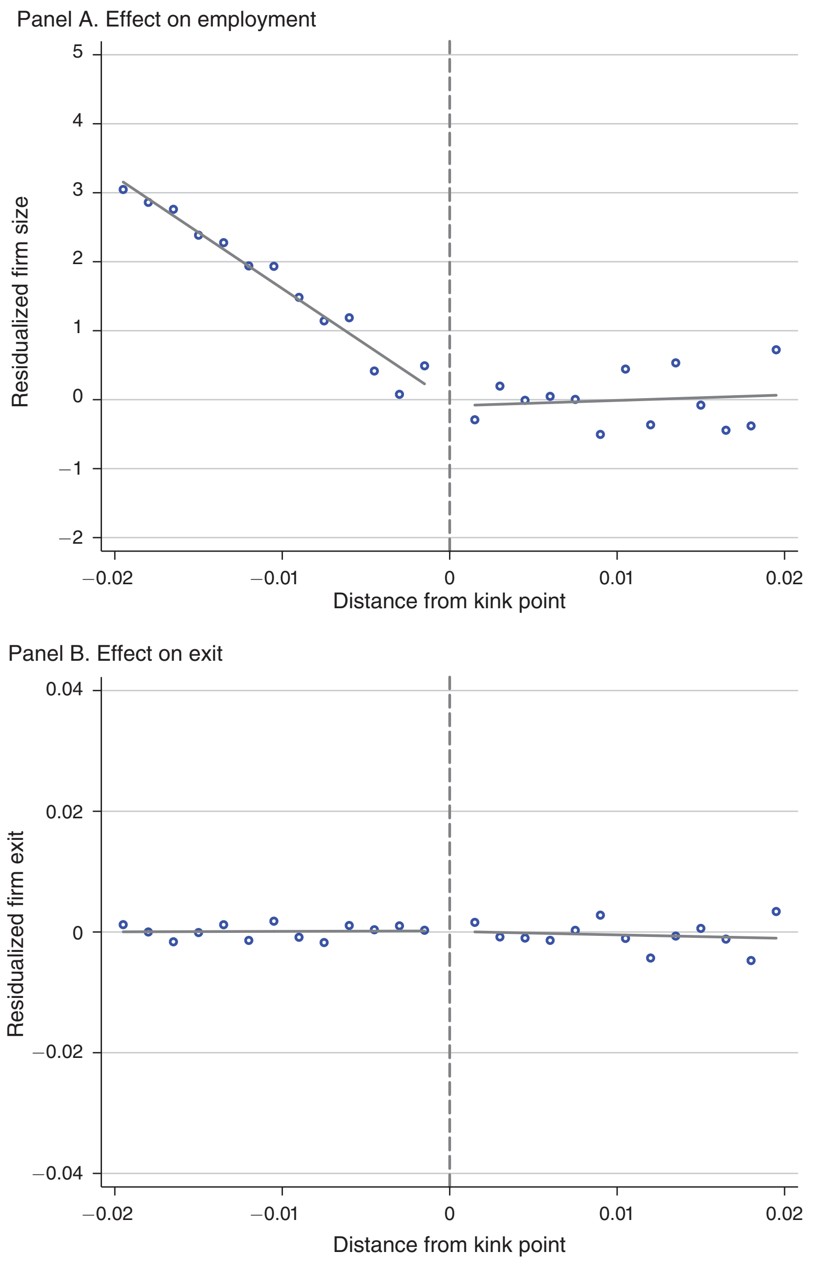


Figure 7. Employment and Exit Kink

Note: These panels show the kink in the residuals accounting for firm and year fixed effects for employment and firm exit.

over the year by estimating models in which the outcome of interest is a firm's hiring behavior in each quarter (online Appendix Table 7). Firms pay the lion's share of their UI tax bill in Q2 and Q3. Firms pay their UI tax bills by the last day of the first month *after* the quarter in question. So, firms pay their UI taxes for Q1 (which make up the two-thirds of their yearly bill) in April by the 30th, during Q2, and the largest remainder of the tax during the first month of Q3 (Employer's Quarterly Report Instructions 2017). Accordingly, the firm response is concentrated in the second and third quarters in about equal measure ($p < 0.001$), while the response in quarters one and four are smaller and statistically insignificant. In discussions with firm managers, the fact that firms are not forward looking is not entirely surprising. Their schedule is dictated by pressing daily needs: a neglected shipment, a burning legal matter, a truant employee, an insolvent customer. Most of their time is consumed by urgent, unpredictable tasks. Consistent with the cash-constraint framework, paying the tax may induce a binding cash constraint, concentrating the employment effect in those quarters where the firm is required to pay the tax.

To examine the dynamic effects of the tax, I estimate the model using lagged firm hiring, conditioning on concurrent tax rates. The effect of the tax kink in the following years is small and statistically insignificant, suggesting that firm hiring recovers as soon as the tax changes. Moreover, these analyses suggest that the tax does not merely alter the timing of hires, which would present as an approximately equal-and-opposite increase in hiring after time t. Instead, estimates of lagged effects reveal small, insignificant, same-sign impacts (see online Appendix Figure 7).

C. *Reasons for Response Size*

The hiring and employment response in this study are quite large relative to that implied by typical labor demand elasticities. Two primary factors play a role in driving the substantial response. First, firms at the kink are cash constrained, which generates large labor demand responses as shown in the conceptual framework in the online Appendix (Schoefer 2015).[27] Second, UI taxes function as a head tax, which places considerable downward pressure on employment when taxes rise.

To evaluate the extent to which cash constraints magnify the overhang effect, I investigate whether the effect varies by measures of cash flows. First, I estimate the effect separately in "expansionary" years (2003–2007) and "recessionary" years (2008–2012), implementing the preferred model on the two 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 than in the expansionary one ($p < 0.01$) and with larger reductions in employment during the recession than expansion (see Table 4).[28] I incorporate a measure of (per-worker) cash flow for each industry over the observation period from CompuStat and estimate the model separately for firms in industries that exhibit above- and below-median cash per employee. Again, the effect is significantly larger for firms in industries that are more cash constrained (significant differences with $p < 0.05$ in employment in Table 4), a finding consistent with the hypothesis that cash constraints magnify the overhang effect of UI

[27] In short, a cash-constrained firm may be forced to reduce employment to cover its expenses and prevent closure.

[28] I estimate the tax effect in each year to demonstrate how the effects evolve over the business cycle (see online Appendix Figure 8). These coefficients are 58 percent larger in the wake of the Great Recession than before.

TABLE 4—KINK HETEROGENEITY BY CASH CONSTRAINT

Outcome	RKDFE 2003–2007 (1)	RKDFE 2008–2012 (2)	Difference (2) − (1) (3)	RKDFE High cash (4)	RKDFE Low cash (5)	Difference (5) − (4) (6)
Hiring rate	0.000	−0.014	−0.014	−0.015	−0.026	−0.011
	(0.004)	(0.003)	(0.005)	(0.003)	(0.005)	(0.006)
Employment (percent change)	−0.009	−0.022	−0.013	−0.010	−0.037	−0.027
	(0.018)	(0.015)	(0.023)	(0.006)	(0.009)	(0.011)
Exit	0.003	−0.001	−0.004	0.002	0.000	−0.001
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Firm/year fixed effects	X	X	X	X	X	X
Linear control	X	X	X	X	X	X

Notes: This table presents results from the preferred specification on subsamples that focus on firms in more- and less-cash-constrained subsamples. Columns 3 and 6 calculate the difference in the estimates from high- and low-cash subsamples.

taxes. Industry-average cash is a noisy measure of firm cash constraints. Because of measurement error, the true differences are likely more pronounced than those represented in these tests (Farre-Mensa and Ljungqvist 2016). Finally, I estimate the effect by industry group. The hiring and employment effects are concentrated in manufacturing, retail, transportation, and communication. By contrast, the effects are smaller and statistically insignificant in industries that tend to have access to credit, such as finance, insurance, and real estate (see online Appendix Table 8).

To evaluate whether the analytic sample behaves as if cash constrained in other ways, I test whether firms that have recently had layoffs fail to be deterred from additional layoffs by larger tax penalties, a hallmark of cash-constrained firms. In previous research, authors have found that firms reduce their reliance on layoffs in the presence of UI penalties (Topel 1983, Anderson 1993). I exploit the fact that the penalty a firm faces for a 1 percent layoff varies based on a firm's placement on the tax schedule. Firms sufficiently below the maximum face the full penalty, firms near the maximum are partly shielded, and firms just at the maximum rate face no penalty at all. Thus, within a narrow region around the onset of the maximum, the penalty for a 1 percent layoff ranges from a 0 to 1.8 percentage point tax increase. Though firms facing larger penalties have fewer layoffs in the cross section, this relationship reverses once firm differences are accounted for using firm fixed effects (see online Appendix Table 9). The confidence intervals on the main estimates in online Appendix Table 9 rule out the magnitudes predicted by a typical labor-demand model, suggesting that firms with recent layoffs are less responsive to penalties, consistent with the hypothesis firms that have recently had layoffs are cash constrained. The details of this test and analysis are described in depth in online Appendix B.

Another factor that seems to play a role in driving the large firm response is the fact that UI taxes are effectively a head tax—a tax on the *number* of workers a firm employs (Chetty 2006). Based on a model derived and calibrated in online Appendix C, the head tax feature of the UI regime explains about half of the "excess" response recovered in the RKD estimates. To test this empirically, notice that if firms responded to UI fees as head taxes, we would expect that high-wage firms effectively pay a lower rate than firms with low average earnings. I split the sample into firms paying above-median earnings to workers on average and firms paying below-median and reestimate the preferred specification for each sample (see online Appendix Table 10). In line with the head-tax explanation, the labor demand response is approximately four times larger among low-wage firms for whom the UI tax makes up a larger share of labor costs, differences that are statistically significant at the 0.001 level in most specifications.

IV. Discussion

A review of the literature suggests two additional factors associated with larger labor-demand elasticities that apply in the UI context (online Appendix Table 11).

First, labor demand is more elastic when labor costs are heterogeneous (e.g., varying by firm rather than affecting firms uniformly). As an example, wage variation at the city-industry level generate an elasticity of -1 while wage variation at a broader city level generates a more modest elasticity of -0.3 (Beaudry, Green, and Sand 2018). Anderson and Meyer (1997a) explain this dynamic succinctly: in a competitive labor market, a firm with an anomalously high labor cost (e.g., tax rate) cannot fully shift that cost onto its employees in the form of lower wages, so the bulk of adjustment must be on the employment margin. The authors there report own-wage labor demand elasticities of 2.2–2.3, with confidence intervals spanning elasticities as large as 3.2. These estimates are based on industry-level variation, and estimates using firm-level variation are expected to be still larger.[29] Thus, part of the large elasticity recovered in the UI setting is likely the result of UI taxes finely varying across firms in the same market.

Second, labor demand is more elastic in settings where wages are difficult to adjust. For instance, wages are difficult to adjust in a short period because nominal wages are rigid. In papers that study unexpected policy changes (like the tax subsidy in Cahuc, Carcillo, and Le Barbanchon 2018) the authors argue that employers had little ability to adjust wages in the short period between the policy announcement and implementation. Similarly, studies in settings where wages are set by law (e.g., those bound by the minimum wage) also report larger demand elasticities (Kramarz and Philippon 2001). In UI, a firm's new tax rate is revealed a month or so before it is implemented, leaving little ability for firms to adjust wages. A final reason that labor demand responses may be larger, as suggested by the literature, is that labor demand elasticities have been increasing over time (Lichter, Peichl, and Siegloch 2015).

[29] Anderson and Meyer (2000) buttress this claim by exploiting the introduction of experience rating in Washington state in 1985, focusing on the impact of UI taxes on earnings rather than on employment. Consistent with larger labor demand elasticities for firm-level variation, Anderson and Meyer (2000) find smaller earnings responses for firm-level tax variation than industry-level. One interesting question is why Anderson and Meyer (2000) find that UI taxes reduce wages, whereas I find precise zero effects. Because Anderson and Meyer (2000) contemplate the effect of the introduction of experience rating in Washington state, they measure the influence of tax changes that are expected and largely permanent. Construction firms will bear a permanent increase in their tax rate, and insurance firms will enjoy a permanent decrease in their tax rate. Whereas temporary changes can affect employment when wages are rigid, permanent changes primarily burden wages since labor supply is inelastic.

Changes in UI Taxation over Time.—Over the past four decades, UI taxes have become much better targeted at offending firms. Prior to the 1980s, states largely shielded distressed firms using low maximum rates which limited a firm's tax exposure when distressed. 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. Figure 8 shows that in the mid-1980s the average maximum rate rose from 4.2 percent to 6.9 percent,[30] a two-thirds rise that dramatically increased the potential tax hikes distressed firms could face.

Analysts in the 1970s and 1980s estimated that firms paid approximately 50 percent of the benefit costs they originated due to low maximum rates that shielded offending firms (US Department of Labor 1985, Hamermesh 1996). Anderson (1993) reports that the average firm paid only 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 in Florida pay a much larger share of benefits charged than firms had in the 1970s. On average, a firm in Florida pays 87 percent of the cost of benefits originated by the firm within two years of a charge. Florida employs the federal minimum for maximum rates and taxable wages, suggesting that Florida's payback rate represents a lower bound for other states.

Since rates reflect the cost of benefits paid, more generous benefits or longer unemployment spells also increase the tax hikes that firms face for a given layoff. From 1990 to 2015, weekly benefit payments and the average duration of UI receipt increased, resulting in the average claim costing 65 percent more today than it did in 1990, adjusted for inflation (see Figure 9).[31] The confluence of increased experience rating and increased costs means that a typical firm pays 187 percent more for an average layoff today than it did in 1980 in real terms.

Generalizability.—Because there has been so little previous work, it is unclear how the results presented here correspond to the effects on firms away from the kink point, as well as firms around the country. An estimator that is feasible for firms not at the kink is a changes-in-changes design, which accounts for firm differences and evolving layoff histories using a within transformation.[32] The estimator is useful to evaluate generalizability because it is feasible in different settings: in each state, the tax is a nonlinear function of the firm's benefit ratio (or similar statistic), allowing the analyst to account for underlying changes in the firm's health by controlling for changes in the benefit ratio, while adjusting for evolving time trends

[30] Hamermesh (1996) estimates that TEFRA boosted experience rating by 15 percentage points, or 30 percent.

[31] Additionally, the 1991 Emergency Unemployment Compensation Act raised firms' tax liability during recessions by increasing the duration of extended benefits from 13 to 20 weeks, a payment that was financed by state and federal UI programs in equal measure.

[32] The changes-in-changes technique implements the following specification:

$$Employment\,\Delta_{it} = Rate\,\Delta_{it} + BenefitRatio\,\Delta_{it} + \alpha_i + \delta_t + \varepsilon_{it}.$$

Here, I regress employment changes on rate changes, while controlling for changes in the underlying benefit ratio. Including firm-specific fixed effects accounts for firm-specific trends in employment and time fixed effects nonparametrically account for broader trends in employment.

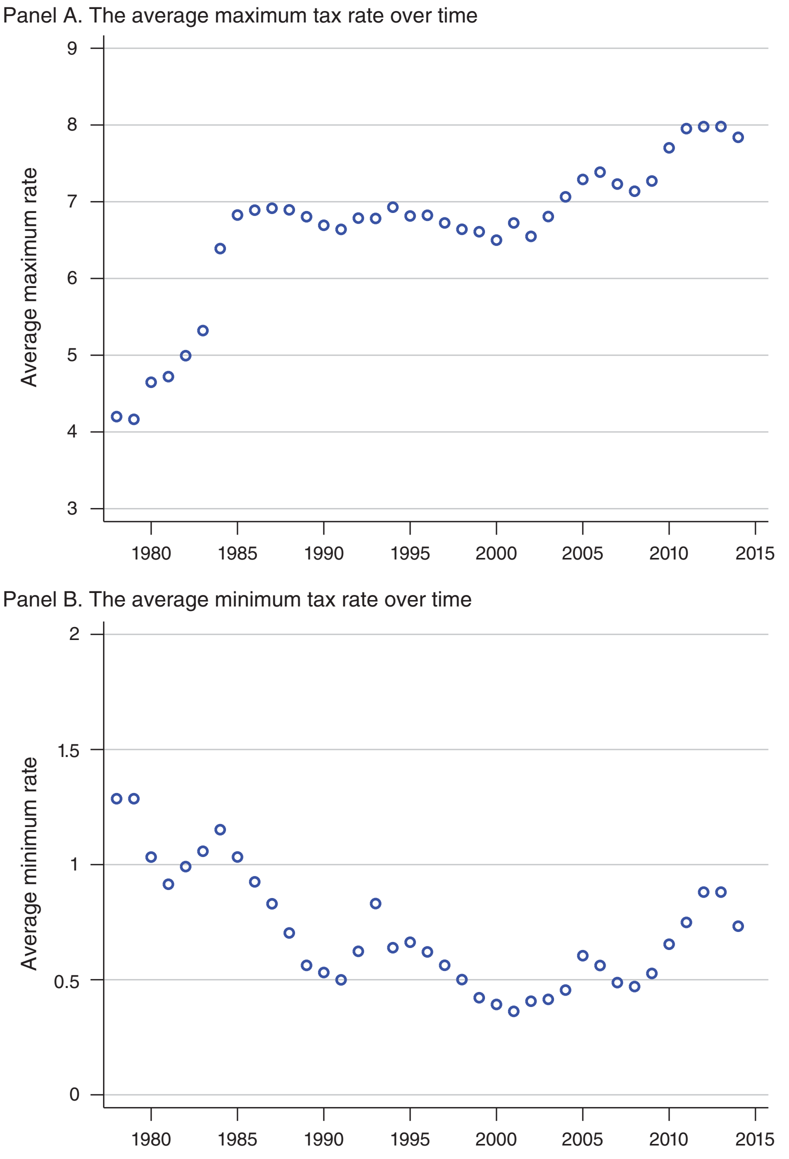


Figure 8. Evolution of UI Tax Systems over Time in the United States

Notes: The dots represent the average maximum/minimum UI tax rate across states in the United States from 1978 to 2015. There was a significant increase in maximum rates around 1985, when federal law (the Tax Equity and Fiscal Responsibility Act) induced states to raise their maximum rates substantially. In recent years, the rising maximum rate was instigated by states to help cover 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 each year.

Source: The information in this figure was provided by the Employment and Training Administration (ETA) of the Department of Labor (DOL).

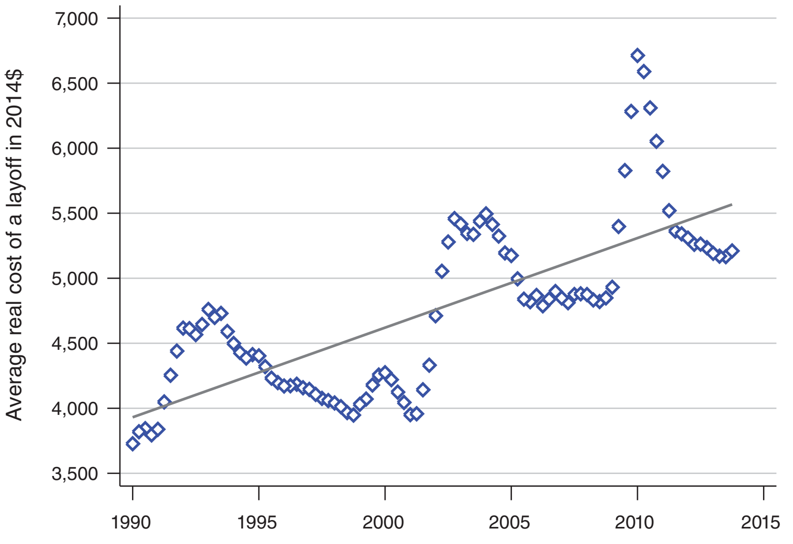


Figure 9. Average Cost of Unemployment Spells over Time

Notes: The diamonds represent the average real benefit cost of a layoff across states over time from 1990 to 2015. Some growth comes from increases in real value of weekly benefits implemented around 2001, and most cost growth is driven by lengthening unemployment durations.

Source: Data from UIDS

with year-specific fixed effects. I present comparable changes-in-changes (CIC) estimates using administrative data in Florida as well as Missouri.

The estimates reflect a broader array of firms, not just those at the kink, and allow me to assess whether estimates in one state correspond closely to those in another (online Appendix Table 12, columns 2 and 3). The CIC estimates in Florida are remarkably similar to the CIC estimates in Missouri. In each state, a one-point increase in the tax rate (adjusting for the taxable wage base so that the rates in each state are comparable) is associated with a 1.2–1.3 percent reduction in firm employment, suggesting that firms in different states have similar responses to tax increases. Moreover, the results from this changes-in-changes design are extremely similar to the main estimates from RKD, suggesting that the RKD in this context provides a representative estimate.[33]

I provide a comparison of Florida's demographics to all other states using the 2010 census in online Appendix Table 13. Florida has a similar education profile, income distribution, and ethnic makeup as other states, suggesting Florida may be relatively representative.

[33] I evaluate how similar Florida is to the rest of the country in online Appendix Table 13. Florida's demographic and labor market characteristics look fairly similar to the rest of the United States in many, though not all, dimensions. Using the characteristics in the table we investigate Florida's "representativeness" by summing each state's rank-distance from the national median for each variable. Using this criterion, Florida is the eighth-closest state to the national median.

Macroeconomic Implications.—I calculate what the RKD estimates would suggest about how automatic UI tax hikes in 2010 and 2011 affected employment in the aftermath of the Great Recession, ignoring possible spillovers.[34] A conservative hiring reduction estimate is 1.25 percent per tax-point increase, in the range of the RKD estimates and in the center of the more conservative CIC estimates.[35] Multiplying this number by the average annual tax increase at the time (the equivalent of a 1.4-point tax increase in 2010 and again in 2011) and then by the number of employees in the United States at that time (114.5 million), the total reduction in employment would be approximately 1,865,000, or 1.4 percent of employment in both 2010 and 2011. This would account for approximately 12 percent of the unemployment at the time.

If experience rating had stayed constant since the early 1980s, tax increases during the Great Recession would have reduced employment by 1 percent, a third less. The increase in taxes during labor market recovery may explain part of the emergence of jobless recoveries since the 1980s. The post-WWII average recovery experienced 7.5 percent employment growth over the 24 months after the recession ended. The recent three recessions (incidentally, the three recessions since TEFRA increased experience rating) have averaged 0.5 percent growth. My estimates suggest that 28 percent of the emergence of jobless recoveries can be explained by increases in experience rating and benefit costs, with about half (52 percent) of the effect accounted for by experience rating and the other half (48 percent) explained by cost increases.

Optimal Unemployment Insurance.—The estimates speak to the social costs of UI benefits under the current tax regime. The workhorse model of optimal benefits maximizes social welfare by increasing generosity until marginal costs and benefits equate (Baily 1978; Chetty 2006; Schmieder, von Wachter, and Bender 2012). The model assumes that benefits are financed from workers' wages with no effect on employment, a premise that would be approximately accurate if rates were stable. Instead, UI taxes exhibit large fluctuations and reduce employment rather than wages. Whereas wage reductions can provide welfare-enhancing transfers, employment reductions represent deadweight loss, which is unaccounted in optimal UI formulae. This finding alters the calculus of optimal benefits.[36] In online Appendix D, I incorporate the employment cost of UI taxes into the optimal UI formula and show that the demand-side distortion from benefit generosity via tax increases is slightly larger than the behavioral distortion exhibited by claimants in response to benefits; accounting for the distortions arising from UI taxes reduce optimal UI benefits by 26 percent.

[34] The $300 billion received by the unemployed from state UI programs during the aftermath of the Great Recession automatically triggered $300 billion in UI tax increases. At peak, UI benefits made up 1 percent of GDP, triggering a 1 percent-of-GDP tax increase in the years that followed.

[35] The calculation that follows would be somewhat larger and less precise if we were to use the estimate on employment instead. I use the hiring estimate to present the more conservative calculation.

[36] Chetty (2006) flags this issue, explaining "the model abstracts from the effects of UI on firm behavior by assuming that the supply of jobs and wage rates are not endogenous to the benefit level."

Policymakers might consider a few adjustments to mitigate the overhang effect while preserving insurance for the unemployed. Naively decreasing the maximum rate while increasing the minimum would succeed in reducing the tax incidence on distressed firms but would also encourage layoffs (Topel 1983). Policymakers could, however, mitigate the head-tax aspect of UI taxation by expanding the taxable wage base to reflect insurable yearly earnings (raising the wage base to $30,000 per employee in Florida), a natural reform. To reduce the concentration of the tax on firms in stress, states could experiment with unemployment insurance savings accounts, in which firms finance UI spells *before* separating, thus avoiding tax incidence when firms are already under strain (Feldstein and Altman 2007).

V. Conclusion

States raise tax rates on firms that lay off workers, a practice that serves the dual purpose of financing benefits and discouraging layoffs. Because tax rates are linked to layoffs, firms mechanically face higher payroll taxes when unemployment is high, and troubled firms bear the brunt of tax increases. An active policy debate centers on the role of unemployment insurance benefits in prolonging joblessness because of reduced labor supply (Farber, Rothstein, and Valletta 2015; Hagedorn, Manovskii, and Mitman 2016), generally ignoring the role unemployment insurance taxes play in affecting labor demand.

In this paper, I measure the influence of UI tax increases, which target distressed firms by design. The regression kink analysis reveals that UI taxes have substantial effects on firm labor demand (Anderson and Meyer 1997a). The large response appears to be driven by two primary factors. First, firms facing tax increases have recently had layoffs and thus are likely to be in distress, generating large employment responses. Second, UI taxes are effectively a head tax, increasing the labor demand elasticity substantially. Several other factors discussed may also play a role.

Unemployment insurance is traditionally considered an automatic stabilizer because benefits are paid in proportion to unemployment; however, taxes for UI track the business cycle proportionally, potentially burdening labor market recovery with higher labor costs when unemployment is already high, thus eroding the stabilizing influence of the program. My results, moreover, assess the optimal UI literature, which assumes UI taxes impose no employment distortion. The findings here suggest a need for models that account for employment costs when assessing the optimal generosity and potential duration of UI.

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