ESSAYS ON UNEMPLOYMENT INSURANCE AND LABOR MARKET

By

Po-Chun Huang

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ABSTRACT

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This dissertation consists of three chapters studying the effects of unemployment insurance (UI) on the behavior of workers and employers.

The first chapter investigates how the low UI tax base in the United States creates a disincentive to hire low wage workers and by how much. In the United States, unemployment benefits are funded by a payroll tax levied on wages below a given ceiling. With this limit, the earnings of low-wage workers are taxed more heavily than are the earnings of high-wage workers. I offer evidence on the employment effects of the low UI tax base by exploiting state variation in the UI tax base over 30 years. Using a difference-in-differences design and data from the Current Population Survey, I estimate that indexing the tax base increases the teenage employment rate by about 2-3 percentage points, and doubling the tax base in the non-indexed states is estimated to raise the teenage employment rate by about 4 percentage points. These results offer evidence that the erosion of the real UI tax base has reduced the employment of low-wage workers.

The second chapter uses administrative unemployment insurance (UI) data and an unemployment benefits extension for workers aged 45 or older in Taiwan to estimate the effects of extended benefits on unemployment duration and reemployment outcomes. Using a regression discontinuity design, we estimate that a 90-day increase in potential duration increases the insured duration by 57 days and the nonemployment duration by 41 days. While we do not find wage gains overall for UI recipients around 45 years old, the benefits extension is estimated to increase the reemployment wage for the lower-wage workers, who are most likely to exhaust their benefits. Our findings suggest that the liquidity constraints at the exhaustion point might play an important role in the effect of a benefit extension on job match quality.

The third chapter estimates the effects of the Taiwanese reemployment bonus program on the timing of reemployment. The reemployment bonus in Taiwan has a unique structure—it offers

50% of the remaining UI entitlements to workers who become reemployed before exhausting their unemployment benefits. We exploit variation in bonus offer around the time the bonus was introduced to identify the incentive effects of the bonus program. Using administrative UI claim and corresponding earning records, we find that the bonus program in Taiwan provides unemployed workers strong incentives to accept reemployment—the program is estimated to increase the monthly hazard (conditional probability) of reemployment by nearly 7 percentage points (on a base of about 10 percent) in the first month of a nonemployment spell, and by 6.6 percentage points (on a base of about 9 percent) in the second month of a nonemployment spell (see Table 3.3). These are large increases in both absolute and relative terms—that is, they represent increases in the conditional probability of becoming reemployed of 68 percent and 75 percent in the first two months of nonemployment. Consistent with the declining bonus offer schedule, the estimated bonus effect on the monthly reemployment hazard gradually declines and disappears after the eighth month of the nonemployment spell.

This thesis is dedicated to to my parents and sister.
Thank you for your unconditional support and love.

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CHAPTER 1

EMPLOYMENT EFFECTS OF UNEMPLOYMENT INSURANCE TAX BASE

1.1 Introduction

Unemployment benefits in the United States are almost entirely financed by taxes on employers' payrolls. The taxable payroll amount is much lower than the total payroll, though, due to a low unemployment insurance (UI) tax base (Brechling, 1980). Because most states adjust their tax bases according to the federal UI tax base, which has been fixed at $7,000$ dollars since 1983, only 35% of the total payroll is taxable in 2013.[2] As shown in Figure B.1, the ratio of taxable payroll to total payroll has been a stable 50%-60% for states not adjusting their tax bases based on annual wages (hereafter referred to as indexed states), but it is lower, only 26% in 2013, for states that do not index their tax bases (hereafter, non-indexed states). Hamermesh (1977) demonstrates that having a low ceiling for UI taxable wages leads firms to substitute high-wage workers for low-wage workers, since employing workers whose wages fall below the ceiling is costly compared to those earning above the ceiling. Hamermesh (1977) proposed raising the ceiling and further indexing it to average annual wage in order to improve employment situations of low-wage workers. When the ceiling is raised, the relative demand of low-wage workers will increase, thereby stimulating their employment.

Despite these points about the UI wage ceiling and tax base having been established for decades, the actual effects of the UI tax base on the employment of low-wage workers have not yet been examined empirically. This article therefore investigates whether, and in what ways, the tax base affects low-wage employment. As seen in Table A.1, the teenage employment rate for indexed

[1] I thank Steve Woodbury, Steven Haider, Carl Davidson and Mark Skidmore for their generous advice and support. I also thank Robert Pavosevich in the U.S. Department of Labor for his valuable information on UI tax.

[2] Anderson and Meyer (2006)'s estimation shows workers in the lowest deciles pay almost 3 percent of their income in UI payroll taxes, while those in the highest deciles pay only around 0.5 percent.

states was 2.9 percentage points higher than that for non-indexed states in 1979, and increased to a difference of 5.5 percentage points by 2014. In contrast, the difference between adult employment rate in the two groups of states does not exhibit this increasing trend. The positive relationship between teenage employment rates and indexing may not be fully attributed to indexing. On one hand, the group of indexed states has changed over time. States that moved to indexation after 1979 might have had higher teenage employment prior to making that switch. On the other hand, the upward trajectory in the difference in employment rates between indexed and non-indexed states might have existed before 1979.

To identify the employment effects of increasing the tax base, I exploit two natural experiments in the form of variations in different states' tax bases. The first variation was created when certain states moved to indexation in different years; the majority of states continued to follow the federal tax base. Examining the increasing gap between the tax base for indexed and non-indexed states as the various states made this shift allows me to identify the employment effects of indexing the tax base. The second arose from substantial increases that occurred in the tax bases for some non-indexed states in the past decade, while the other non-indexed states' tax bases remained unchanged. I use these staggered increases in non-indexed states' tax bases to identify the employment effects caused by these increases. Together, these two natural experiments allow me to estimate the employment effects of the two different types of tax base increases.

Using these difference-in-differences designs and data from the 1979-2014 Current Population Survey, my estimates suggest that raising the tax base stimulates teenage employment, but has only a minimal effect on adult employment. Increases in the tax bases of indexing and non-indexing states influence low-wage employment in different ways. I find that the employment effect of indexing is less than 2 percentage points in the first four years of indexing, but from the fifth year after a state moves to indexation it begins to increase. Specifically, indexing the tax base is estimated to increase teenage employment rates by 2–3 percentage points in the fifth and sixth years of indexing, and 4 percentage points in the seventh and subsequent years on average. By contrast, doubling the tax base for non-indexed states increases teenage employment rates up to

4.7 percentage points in the first year following the tax base increase, but the effect decreases to less than 3 percentage points in the seventh year after doubling the tax base. These results suggest that the erosion of the UI tax base in real terms has reduced low-wage employment, and indexing the tax base seems to be an effective way to ameliorate the unemployment situation facing low-wage workers.

Literature on the effect of the UI tax wage ceiling on employment mix is mostly theoretical. Based on analysis of labor supply and demand, Hamermesh (1977) and Hamermesh (1990b) demonstrate that a low tax base reduces low-wage workers' employment. Increasing the ceiling removes the distortion and benefits low-wage workers. However, high-wage workers' employment would drop if we apply the same logic to this group. Wright and Loberg (1987)'s search model shows that in this case, the reservation wage for high-wage workers will rise, thereby increasing their unemployment. Moreover, while low-wage workers' unemployment situations improve because of a lower reservation wage, their average wage falls. Hence, Wright and Loberg (1987) propose an increase in the wage ceiling with a reduction of the UI tax rate. By doing so, low-wage workers' wages and reemployment probabilities both increase, while high-wage workers are not affected.[3] These proposed changes can actually be accomplished through the existing experience rating system. Since a higher UI tax base increases a state's contribution to the UI account, and the UI tax rate is a function of previous UI reserve ratio, an increased tax base will decrease the UI tax rate in subsequent years. Indeed, I found that an increase in the tax base raises the average tax rate for high-wage workers in the year of the increase, but afterward lowers the average tax rate for both low- and high-wage workers.

The outline of the paper is as follows. In Section 1.2 I present the expected impact of raising the taxable wage ceiling on both employment and wages for low- and high-wage workers. I introduce the basics of financing UI, and explore the relationship between the UI tax base and average UI tax rate in Section 1.3. Then in Section 1.4, I explain my identification strategies and estimate the employment effects of a) indexing the tax base and b) non-indexing increases in the tax base.

[3] See Davidson (1990, 51) for a concise discussion on the employment effects of an increase in the tax base.

Finally, in Section 1.5 I offer conclusions based on these analyses.

1.2 Conceptual Framework

This section presents a demand and supply analysis of the effects of an increased tax base on the mix of workers. To use a concrete example, consider a firm faced with a 5% UI tax rate in a state in which the UI tax base is $20,000. If this firm hires a worker who is paid $20,000 per year, the annual UI tax payment for this worker is $1,000. Since the taxable payroll amount is $20,000, the same amount of UI tax, $1,000, would be paid for any workers earning more than $20,000. Hence the UI tax is regressive, in the sense that the firm's UI tax rate is 5% for workers earning 20,000 or less, but lower for those workers who earn more. (E.g., for an employee who earns $30,000 annually, the UI tax rate would be only 3.33% of their salary.) In other words, the UI tax payment for high-wage employees can be viewed as a fixed cost, while the costs for employing low-wage workers are proportional to each worker's wages. In this case, if the state government raises the UI tax base to $25,000 it would help the employment situation for low-wage workers, since the average UI tax rate for high-wage workers would increase to 4% but the rate for lower-wage employees would remain the same.

To express this concept more formally, consider the following model based on Hamermesh (1990a, 178-179). There are two groups of workers: low-wage (l) workers, whose wages fall below the tax base, and high-wage (h) workers, whose wages are above the tax base. The tax rates for low-wage and high-wage workers are τ_l and τ_h, respectively. A given firm's production function follows CES:

$$Y = \{(1-\alpha)E_l^{\rho} + \alpha E_h^{\rho}\}^{\frac{1}{\rho}},$$

where $\sigma = \frac{1}{1-\rho}$ is the elasticity of substitution. Optimal labor demand equates the marginal productivity and marginal cost of each labor:

$$(1-\alpha)[\frac{Y}{E_l}]^{\frac{1}{\sigma}} = w_l(1+\tau_l)$$

$$\alpha[\frac{Y}{E_h}]^{\frac{1}{\sigma}} = w_h(1+\tau_h)$$

Hence, the relative labor demand function is

$$\frac{w_l}{w_h} = \frac{1+\tau_h}{1+\tau_l}\frac{1-\alpha}{\alpha}(\frac{E_h}{E_l})^{\frac{1}{\sigma}}.$$

The total supply of labor depends on the average wage, that is

$$E = E_l + E_h = [\frac{w_l E_l + w_h E_h}{E}]^{\varepsilon},$$

where ε is the elasticity of the labor supply. Occupation choice (low-skilled or high-skilled jobs) depends on relative wage.

$$\frac{E_l}{E_h} = [\frac{w_l}{w_h}]^{\theta},$$

where θ is the relative supply elasticity. In equilibrium,

$$\log\frac{E_l}{E_h} = \frac{\sigma\theta}{\sigma+\theta}\log Z;$$

$$\log\frac{w_l}{w_h} = \frac{\sigma}{\sigma+\theta}\log Z,$$

where $Z = (\frac{1+\tau_h}{1+\tau_l}\frac{1-\alpha}{\alpha})$. $\frac{\partial \log Z}{\partial \tau_h} \approx 1$, because $\log(1+\tau_h) \approx \tau_h$ if τ_h is small. Therefore,

$$\frac{\partial \log\frac{E_l}{E_h}}{\partial \tau_h} = \frac{\sigma\theta}{\sigma+\theta} > 0 \text{ iff } \sigma > 0 \text{ and } \theta > 0;$$

$$\frac{\partial \log\frac{w_l}{w_h}}{\partial \tau_h} = \frac{\sigma}{\sigma+\theta} > 0 \text{ iff } \sigma > 0.$$

Assume that an increase in the tax base, T, has no effect on τ_l, but increases τ_h; that is, $\frac{\partial \tau_l}{\partial T} = 0$ and $\frac{\partial \tau_h}{\partial T} > 0$. Given that low-wage and high-wage workers are substitutes ($\sigma > 0$) and that the relative supply elasticity is not perfectly inelastic ($\theta > 0$), the employment of low-wage workers relative to high-wage workers will increase.

Intuitively, if the UI taxable wage ceiling increases and other things remain equal, we can see that the UI tax for employing workers whose wages are above the ceiling will increase, while the tax for hiring low-wage workers will not change. This relative shift in taxes will increase the demand for low-wage workers but decrease that for high-wage workers. In the new equilibrium, the number of low-wage workers, and their wages, are higher, while both the number of high-wage workers and their wages decrease. The degree of the impact will depend on how much labor demand shifts and the elasticity of the labor supply for each group.

The above analysis is based on several implicit assumptions. First, I assume that low-wage and high-workers are substitutes instead of complements. This assumption seems reasonable to the extent that raising the base mainly affects the workers whose salaries fall near the tax base. Second, an increase in the fixed costs for hiring high-wage workers not only creates a substitution effect, but also a scale effect, which decreases the demand for both high- and low-wage workers. Hence, the predicted increase in the demand for low-wage workers is based on the assumption that the substitution effect is larger than the scale effect.

Third, the analysis also ignores the possibilities of changes in individual labor supply. Hamermesh (1977, 74-75) argues that if employers can successfully shift the extra taxes back to high-wage workers, low-wage jobs will become more attractive relative to high-wage jobs. Therefore, the labor force participation of low-wage workers will rise relative to that of high-wage workers. This will shift the labor supply of low-wage workers to the right, and that of high-wage workers to the left. While the impact of the tax base increase on the demand side will be instant, the supply side effect will take a longer time to manifest. How long it takes depends on not only how fast firms can shift the extra taxes to high-wage workers (or consumers through higher prices), but also how mobile workers shifting are between low-wage and high-wage jobs.

Fourth, this model ignores the experience-rated UI tax and dynamic labor demand. If an increase in the tax base has a displacement effect, it will raise the UI tax rate in the future. However, as I will show in the next section, raising the tax base in fact actually leads to a lower UI tax rate in the following years, especially for low-wage workers, because of the nature of experience rating.

Finally, the model assumes there is only one sector. A two-sector model makes more sense when sectoral shifts are present, and when low-wage and high-wage workers are complements instead of substitutes within a firm. In a situation where one sector hires more low-wage workers and the other one hires more high-wage workers, the low tax base will be more beneficial to the high-wage sector. Increasing the tax base will partly remove the distortionary effect of the UI tax base, and stimulate low-wage employment.

1.3 Background on UI Taxes

1.3.1 UI Tax System

Unemployment insurance benefits are financed exclusively by taxes on employers in almost all U.S. states. Only in Alaska, New Jersey, and Pennsylvania do employees also pay a small amount of UI tax. According to the Federal Unemployment Tax Act (FUTA), employers are liable for a payroll tax of 6% on wages below a ceiling of $7,000$ per employee. The federal government allows states to waive 5.4% of the federal tax if states' UI tax systems meet the federal requirements. To be qualified, states have to finance their UI programs through an experience-rated payroll tax on the federal tax base (or a higher state-legislated tax base), and set a maximum tax rate no lower than 5.4%. The UI tax is experience-rated in the sense that employers' tax rates are based on their previous layoff experience.

Most U.S. states use either the reservation ratio (*RR*) or benefits ratio (*BR*) approach to determine their UI tax rate. The *RR*, utilized by 30 states, equals the difference between all of an employer's past contributions and benefits divided by their average payroll over the past three years. In this approach, a lower *RR* corresponds to a higher UI tax rate. The second popular method, the BR approach, is used in 18 states; *BR* is equal to the percentage of the past 3 years' charged benefits to the average payroll over the same period of time. In contrast to *RR*, when using the *BR* approach, the higher the *BR*, the higher the UI tax rate. These two systems both charge for current benefits in the future. This means that a firm's layoffs in the current year do not affect that year's UI tax rate, but instead increase their future UI tax rates.

$$RR = \frac{\text{All Past Reserves}}{\text{Avg. Payroll in the Past 3 Years}};$$

$$BR = \frac{\text{Charged Benefits in the Past 3 Years}}{\text{Avg. Payroll in the Past 3 Years}}$$

There are two differences between *RR* and *BR*. First, *RR* takes employers' contributions into account, but *BR* does not. And second, *RR* has an infinite memory, while BR only reflects firms' layoff experiences over the prior three years. The latter has important implications for the effects of the tax base on the UI tax rate. As Vroman and Woodbury (2014) demonstrate, in an *RR*-based system, an employer's tax rate responds slowly to increased layoffs. Similarly, since *RR* takes all past reserves into account, its UI tax rate should respond to tax base changes more slowly compared to a *BR*-based system.

1.3.2 UI Tax Base

In 2014, each of the 50 U.S. states and the District of Columbia had a taxable wage base higher than or equal to $7,000. [4] Figure B.2, which I reproduce from Figure 3 of Vroman and Woodbury (2014, 262), shows that the average tax base for non-indexed states is very much the same as the federal tax base, while that for indexed states steadily increases over time. There are three major sources of state-year variation in the UI tax base. First, 16 states index their taxable wage bases to their average annual wage. Each indexed state differs in its average annual wage and method of computing its taxable wage base. For example, Hawaii computes the taxable wage base as 100% of average annual wage, while New Jersey computes it as 28 times the average weekly wage.[5] Second, the difference in the tax bases between the indexed and non-indexed states changes over time. As we can see from Figure B.2, the gap between the average tax base for indexed and non-indexed states has widened since mid-1970. The exceptions to this trend are the years when the federal tax base increased (1972, 1978 and 1983). Third, non-indexed states adjust their tax bases as the federal tax base increases, and have substantially increased their tax bases in the past decade. In the 1970s and 1980s, the variations in the tax bases for non-indexed states are mainly driven by federal changes over time. However, notably, the average tax base for non-indexed states has

[4] California and Arizona have a taxable wage base equal to $7000 in 2014. See Table A.2 for the UI tax bases of all 50 states and the District of Columbia as of 2014.

[5] See Table A.3 for a summary of the computations of the taxable wage base for the indexed states.

experienced an upward trend since the 1990s, especially in the past decade.

1.3.3 Average UI Tax Rates

An increased tax base raises the relative demand for low-wage workers through increasing the tax rate for high-wage workers. The ET Financial Data Handbook 394 Report from the U.S. Department of Labor provides two useful average tax rate variables to use in assessing the tax burden borne by high-wage and low-wage workers: ATR^H and ATR^L.

ATR^H is UI taxes divided by total payroll.

$$ATR^H = \frac{E^H \cdot TB \cdot t + \sum_i^{E^L} w_i^L \cdot t}{\sum_i^E w_i} = \left(\frac{E^H \cdot TB}{\sum_i^E w_i} + \frac{\sum_i^{E^L} w_i^L}{\sum_i^E w_i}\right) \cdot t, \tag{1.1}$$

where TB is the tax base. E^H and E^L denote the number of workers who earned above and below the tax base. w_i represents the wage for worker i and the superscript L indicates he earns less than the tax base. t is a given state's average UI tax rate. ATR^H can be considered as an approximation for the average tax rate for high-wage workers if workers earning below the tax base only account for a small portion of all workers covered by UI.

ATR^L equals the UI taxes divided by the taxable payroll, as demonstrated by

$$ATR^L = \frac{E^H \cdot TB \cdot t + \sum_i^{E^L} w_i^L \cdot t}{E^H \cdot TB + \sum_i^{E^L} w_i^L} = t. \tag{1.2}$$

As seen in Equation 1.2, ATR^L is exactly equals to t, which represents the UI tax rate firms pay for workers earning below the tax base. Therefore, I consider it as the average tax rate for low-wage workers.

If all other things are equal, raising the tax base results in a higher ATR^H in the year of the tax base increase, while ATR^L in the year of the increase will not be affected. In other words, raising the tax base increases the share of UI tax that high-wage workers pay, so their average tax rate goes up. The prediction, however, is incomplete, because it ignores the dynamics of average tax rates.

If an increase in the tax base increases the UI taxes in the present, UI tax rates will go down in the future because of experience rating, no matter whether a state uses a *BR* or an *RR* system.

In this type of situation, the percentage change in future average tax rates due to tax base changes will be larger in magnitude for ATR^L. To see this, note that ATR^H can be written as the UI tax rate multiplied by the sum of taxable payroll as a percentage of total payroll for low-wage and high-wage workers, because only wages below the tax base are taxable. Since the sum of these two terms is less than one, a decrease in t will lead to a smaller decrease in ATR^H.

Figures B.3 and B.4 show ATR^H and ATR^L for indexed and non-indexed states from 1970 to 2014; the composition of indexed and non-indexed states has not changed since 1986. There are two observations that can be drawn from these figures. First, ATR^H is consistently higher for indexed states, and this follows a similar, and stable, trend over time. Second, ATR^L for indexed states has been lower than that for non-indexed states since 1990. In 1970, ATR^H and ATR^L were 0.91 and 1.725 for indexed states, while they were 0.64 and 1.23 for non-indexed states. In contrast, in 2014, ATR^H and ATR^L were 1.31 and 2.43 for indexed states, and they were 0.80 and 3.18 for non-indexed states. In short, ATR^L is about 4 times higher than ATR^H in non-indexed states, but it is less than two times higher in indexed states. It seems that the current low tax base has subsidized high-wage industries with low-wage ones, especially in non-indexed states.

To probe the relationship between the tax base and average tax rates, I estimate the following fixed-effect regression:

$$\log ATR_{st}^{K} = \alpha_s + \lambda_t + \phi_0 \log TB_{st} + \varepsilon_{st}; K = H, L. \tag{1.3}$$

State fixed effects control for any fixed differences between states. Year fixed effects absorb variability in the tax base. The tax base, however, does not vary across states in any given year, so the variation in tax base mainly comes from recent individual state increases in the tax base rather than the federal tax base changes that occurred in 1972, 1978 and 1983. Columns 1 and 3 of Table A.4 show the results, after removing the dynamic effects of the tax base on average tax rates. Estimates suggest doubling the tax base increases ATR^H by 29%, but decreases ATR^L by 42%. These estimates, however, are hard to interpret, because they capture both the present and lag effects (or experience-rating effects) of tax base increases.

To distinguish these two channels, I include the log of tax base in $t-1$, $t-2$,.., and $t-6$:

$$\log ATR_{st}^{K} = \alpha_s + \lambda_t + \sum_{k=0}^{6} \phi_k \log TB_{st-k} + \varepsilon_{st}; K = H, L. \tag{1.4}$$

Columns 2 and 4 of Table A.4 present the dynamic effects of tax base increases on average tax rates. Doubling the tax base is estimated to increase the current ATR^H by 65%, and decrease the ATR^H in the years following the tax base increase due to experience rating. In the long run, these estimates suggest the ATR^H will be 26% higher than it would otherwise have been. On the other hand, the present increase in the tax base has no effect on the present ATR^L, despite it resulting in a lower ATRL in subsequent years. Furthermore, the lag effects are stronger in magnitude for ATR^L. Although they are not significantly different, these estimates are consistent with what we would expect.

On average, the experience rating effect does not become visible until two years after a tax base increase. This is possibly because most U.S. states adopt an *RR* approach to experience rating. Table A.5 shows the estimates for states using *RR* and *BR* systems. For states using an *RR* system, experience rating takes effect in the second year after the tax base increases, while increasing the tax base results in a lower average tax rate in the year immediately following the increase for states using a *BR* system. Nevertheless, in the long run, the estimated effects for *RR* and *BR* states are comparable.

To sum up the results presented in Tables A.4 and A.5, in the present period, increasing the tax base leads to a more than 50% higher average tax rate for high-wage workers, while it has no significant impact on the average tax rate for low-wage workers. After a short period of time (1 year for *BR* states and 2 years for *RR* states), the average tax rates for both groups drop significantly due to experience rating. However, for the average tax rate for high-wage workers, the magnitude of decreases in future periods is not as large as the initial increase. As a result, taking account of experience-rating effects, an increase in the tax base still creates incentives for firms to hire low-wage workers.

1.4 Data and Empirical Strategy

For the analyses in this paper, I obtain my data from several sources. First, I draw on the 1979-2014 Current Population Survey (CPS) Merged Outgoing Rotation Group (MORG) files. Using information on state of residence in the CPS, I then match the individual status of the labor force and the UI tax base variables from the ET Financial Data Handbook 394 Report. In addition, states' total employment data are taken from the Local Area Unemployment Statistics (LAUS) program, published by the Bureau of Labor Statistics. Finally, data for state populations are gathered from the U.S. Census Bureau.

To estimate the effects of increasing the tax base on low-wage and high-wage employment, I consider workers ages 16-19 as low-wage workers and workers ages 20-60 as high-wage workers. Table A.18 shows the average annual wages of teenagers and adults, accompanied by the average UI tax base for indexed and non-indexed states from 1979 to 2014. The average wage for teenagers is much lower than the average tax base of indexed states, while it is close to the average tax base of non-indexed states. The adult counterpart shows a much higher average annual wage relative to the average tax base.

There are two kinds of variation in the UI tax base: indexed increases and non-indexed increases. On one hand, an indexed increase in the tax base is a gradual and persistent increase due to to an increase in the prior average annual wage. On the other hand, a non-indexed increase is a permanent, large increase that occurs without changing the tax base in the intervening years. Indexed increases and non-indexed increases are thus likely to have quite different effects on employment. For example, an indexed increase in the tax base from $\$25,000$ to $\$26,000$ is unlikely to affect low-wage employment, because most low-wage workers earn less than $\$25,000$ per year. On the contrary, a non-indexed increase from $\$7,000$ to $\$10,000$ might have a significant impact on low-wage employment. Therefore, instead of pooling the two kinds of variation together for analysis, I investigate the employment effects of each type of variation independently, and discuss the differences in the estimated effects.

To clarify variation in the tax base, I plot the UI tax base from 1979 to 2014 for all 50 states and the District of Columbia, grouped according to the nine census divisions, Figures B.5 and B.6. Within each census division, whenever a state moved to indexation, the path of its tax base diverged. For example, in Figure B.5 (e), North Carolina is the only state having a 2010 UI tax base higher than $\$19,000$ among the nine states of the South Atlantic census division. North Carolina moved to indexation in 1984, while the other states do not index their tax bases. A simple difference-in-differences estimate is obtained by comparing the employment trend for North Carolina and the other South Atlantic states before and after 1984.

As shown in Table A.7, though, there were in fact nine indexed states in 1979 (NJ, ID, NV, HI, WA, IA, ND, NM, and MT), and seven other states (RI, AK, MN, NC, WY, UT, and OK) had moved to indexing by 1986. I consider these seven states my "treatment" states, and the other 34 states and Washington, D.C. (those which were not indexed prior to the sample period and which did not shift to indexing during that time) my control states. Pooling all observations from 1979 to 2014, except for those from the nine states that moved to indexing before 1979, I estimate the following fixed effects model:

$$E_{ist} = \alpha_s + \lambda_t + \beta^{index} Index_{st} + \delta log(\frac{emp}{pop})_{st} + \varepsilon_{ist}, \tag{1.5}$$

where $Index_{st}$ is an indicator for each year starting with the year states moved to indexation. The dependent variable is denoted as E_{ist}, which is 1 if an individual is employed in state s in year t. The state effects, α_s, control for fixed differences between states; the year effects, λ_t, capture trends in employment common to all 50 states and the District of Columbia. β^{index} is interpreted as the effect of indexing the tax base on employment rate. Average tax rates are not adequate control variables, because an increase in the tax base increases low-wage employment due to an increase in the average tax rate for high-wage workers. Moreover, the two types of average tax rates are both functions of firms' hiring and layoff experiences. Hence, they are likely to be endogenous.

Equation 1.5 estimates the effects of indexing on employment rates by comparing the rates of indexed and non-indexed states before and after indexing. However, the low-wage employment rates for indexed and non-indexed states might have experienced different trends prior to indexing. To control for possibly heterogeneous employment trends, I include the log of the state employment to population ratio from the LAUS program and the Census Bureau. I also report estimates that control for census division-year fixed effects and state linear time trends.

The other useful variation in the tax base is the recent increases for non-indexed states. As we can see in Figures B.5 and B.6, some non-indexed states have substantially increased their tax bases since 2007, whereas the tax bases for other states barely changed in this time period. Therefore, using samples from non-indexed states collected between 2000 and 2014, I estimate

$$E_{ist} = \alpha_s + \lambda_t + \beta^{non} \log TB_{st} + \delta log(\frac{emp}{pop})_{st} + \varepsilon_{ist}, \tag{1.6}$$

where I interpret β^{non} as the employment effects of the tax base. This specification relates recent tax base changes in non-indexed states to changes in employment rates. Observations from indexed states are not included in the sample for this analysis because those states increased their tax bases gradually over time, and are thus not suitable for comparison in this instance.

One important concern for the empirical strategies is that the timing of indexing and recent tax base increases are likely not random. On the one hand, if firms to some extent anticipated when states would move to indexation during this period, they might have responded to this change prior to the indexing of the tax base. On the other hand, several non-indexed states increased their tax bases following the Great Recession. It seems possible these states raised their tax bases to ameliorate deteriorations in their UI reserves caused by the recession. However, if these factors are present, they will cause our estimates to tend to underestimate the effect of increasing the tax base on low-wage employment.

1.4.1 Employment Effects of Indexing the Tax Base

In Table A.8, I present the estimated effects of indexing the tax base on teenage employment rates. Column 1 estimates Equation 1.5 without additional controls. The estimated effect is positive at 0.9 percentage point, but insignificant different from zero. Column 2 adds census division–specific year fixed effects. This specification implicitly uses the non-indexed states in the same census division as each indexed state as a control group. The estimate suggests that indexing the tax base increases the teenage employment rate by 1.6 percentage points, with a smaller standard error. The model controlling for census division-specific year dummies may be more reliable if we believe states geographically closer to the treatment states are a more adequate control group.

Column 3 adjusts state-specific teenage employment trends by including state linear time trends into the model. The estimate is significant at 5 the percent level and appears to be larger than the estimates in Column 1 and 2, suggesting teenage employment rates might have declined faster in non-indexed states prior to when each state moved to indexation. One concern of these estimates is that the timing of indexing might be related to other policy changes, such as minimum wage increases. The estimate in Column 4 shows that adding state minimum wage as a control does not affect the results. Finally, as seen in Column 5, the results are also robust to adding demographic variables, including age, race, gender, and marital status. Overall, the estimates indicate that indexing the tax base increases teenage employment rates by about 2 percentage points.

In Table A.9, I report the estimated impact of indexing the tax base on the employment rates of individuals aged 20–60. The estimate in each specification shows that indexing the tax base has a small (less than 0.3 percentage points) and statistically insignificant effect on adult employment. This result contradicts the theoretical discussion in Section 1.2, which indicated that an increase in the tax base will decrease the employment of high-wage workers. There are two possible explanations. The first is that the employment effect of increasing the tax base on high-wage workers might be small for those earning salaries much higher the tax base, because the increase in the fixed costs caused by a higher base is just a small portion of their wages. Alternatively, adult workers are a mixture of low-wage and high-wage workers. The small, positive, yet insignificant estimates might imply that the negative effects on high-wage workers are just offset by the positive effects on low-wage employees.

The models I have considered by now assume that indexing the tax base has had the same effect in every year since states moved to indexation. However, it is plausible that some firms might not be willing to adjust for employment structures until the tax base increase has reached some specific level, due to adjustment costs. To investigate the dynamic effect of indexing the tax base on employment, I add leads and lags into the model and estimate

$$E_{ist} = \alpha_s + \lambda_t + \sum_{k=-2}^{\bar{k}} \beta_{-k}^{index} Index_{st-k} + \delta log(\frac{emp}{pop})_{st} + \varepsilon_{ist}, \tag{1.7}$$

where $Index_{st-k}$ is an indicator for k years after moving to indexing for each $k < \bar{k}$, while $Index_{st-k}$ is equal to 1 for each year at least $\bar{k}$ years after indexing. β_0 is the employment effect of indexing in the year states moved to indexation. β_{-1}, β_{-2}, .., and $\beta_{-\bar{k}}$ are post-treatment effects one year, two years, ... and at least $\bar{k}$ years after indexing. β_{+1} and β_{+2} are anticipatory effects. If these effects are present, it implies that some firms adjusted their employment mix before indexing occurred, state governments increased their tax bases due to employment changes, or the moves to indexation confound with other policy changes.

Table A.10 shows estimates from the models including leads and lags. My benchmark estimates in Columns 1 to 6 consider $\bar{k}$ equal to 6 such that β_{-6} captures the average impact of indexing on the teenage employment rate after at least six years moving to indexation. Importantly, the effects in the two years before indexing are insignificant different from zero in each specification, suggesting the positive relationship between indexing and teenage employment is not driven by reverse causality. Column 1 presents estimates without additional controls. Indexing the tax base is estimated to have small, negative, and insignificant effects on teenage employment in the first five years of indexing and a small, positive, but insignificant effect from the sixth year after indexing. Including census division-year dummies in column 2 increases the precision of the estimates. Although the estimates in columns 1 and 2 are insignificant different from zero, the effects in later years after states indexed their bases appear to be larger.

In column 3, I add state linear time trends to the model. The estimates suggest that indexing the tax base has small, positive, but again insignificant impacts in the first five years of indexing. However, indexing the tax base significantly increase teenage employment rates by an average of 4.8 percentage points in the seventh and subsequent years. The pattern of estimated coefficients is comparable to column 3 when state minimum wage and demographic variables are added as controls in columns 4 and 5. Columns 6 and 7 use seven and eight lags of indexing. Similarly to the benchmark specification, the last lag is equal to 1 for each year after at least 7 years (8 years) of indexing. As we can see, the estimated coefficient of the last lag of indexing becomes larger when more lag variables are included in the model.

The fact that the estimated coefficient of the last lag is always the largest and it increases each time the lag length increases is consistent with dynamic misspecification problem in distributed lag models (Pakes and Griliches, 1984; Haider and Klerman, 2004). Pakes and Griliches (1984) study the problem of estimating distributed lag models in short panels and show truncated distributed lags load on to the last included lag. Haider and Klerman (2004) examine the relationship between welfare caseload and lags of unemployment rate and find that the coefficient of the longest included lag is estimated to be the largest regardless of the number of included lags. Similarly, as Table A.10 suggests, the truncated lag indexing variables load on to the last included lag. As a result of this misspecification, little can be concluded about any time pattern in the effect of indexing the tax base on teenage employment. This is an important topic for future research.

1.4.2 Employment Effects of Non-Indexed Increases in the Tax Base

The estimates exploiting the differential timing of indexing are relevant for the seven states that moved to indexation after 1979, but theses estimates might not be generalizable to other states. Table A.11 illustrates the estimated effects of increasing the tax base on teenage employment rates for non-indexed states using Equation 1.6. These estimates suggest a 10% increase in the tax base will increase teenage employment rates by 0.3-0.4 percentage points. Similar to the estimated effects of an indexed increase in the tax base, a non-indexed increase in the tax base shows no significant impact on adult employment.

In Table A.13, I add two periods of leads and six periods of lags. I find no evidence that firms respond in advance of tax base rises. The estimates show a strong employment effect for teenagers in the first year of a non-indexed increase in the tax base. These non-indexed increases are usually much larger than an average annual indexed increase, so it is not surprising that firms have to make adjustments when the tax bases are raised in the non-indexed states. Furthermore, over half of the estimated lag effects show a small, negative, and imprecise impact in the years following tax base increases. This is possibly because wage and price increases erode the tax base increase in the non-indexed states.

1.4.3 A Comparison of Indexed and Non-Indexed Increases

To evaluate the magnitude of the estimated effects of indexed and non-indexed increases in the tax base on low-wage employment, I compare the employment effects of indexing the tax base to those of doubling the tax base. In Figure B.7, the blue dashed line plots the estimated coefficients of column 5 in Table A.10. The effect of indexing on teenage employment shows an upward trend, and reaches almost 5 percentage points after the sixth year of indexing. In contrast, using column 5 in Table A.13, teenage employment rates are predicted to increase by 4.8 percentage points in the first year of the doubled tax base. The effect fluctuates between 3–4 percentage points in the following 4 years, and declines to less than 3 percentage points in the seventh year of the tax base increase.

The final consideration regarding my estimation results is how to correctly interpret them. On one hand, the low-wage group I consider in this paper are a mixture of people earning above and below the tax base. The estimates hence suggest that the positive employment effects of raising the tax base on people below the ceiling are higher than the negative ones on people above the ceiling. On the other hand, increasing the tax base also enhances the degree of experience rating Brechling (1977), which provides a disincentive for firms to lay off workers. This may increase employment as well. However, it is difficult to disentangle the experience-rating effect from the effects caused by the difference in the average tax cost between low-wage and high-wage workers. Therefore, I interpret the coefficient of the tax base as the composite effect of the experience rating and the average tax cost difference.

1.5 Conclusion

In 2015, the tax base of the Social Security Administration's Old-Age, Survivors, and Disability Insurance was $\$118,500$, which was about 16 times as large as the federal UI tax base. The regressive nature of the UI tax is unusual, in the sense that we have been disproportionately using taxes on the employment of low-wage workers to finance UI benefits. This paper intends to answer the question of whether the eroding tax base has encouraged employers to substitute high-wage workers for low-wage ones. Empirically, I exploit two natural experiments to assess the employment effects of tax base changes. The first was generated by a small number of states moving to indexing in different years, while most states did not change their tax bases. The other resulted from several non-indexed states having substantially raised their tax bases since the year 2000, while the tax bases for the other non-indexed states only changed to a limited degree.

When states move to indexation, teenage employment rates are estimated to increase by about 2-3 percentage points compared with their level without indexing. Increases in the tax base in non-indexing states are also estimated to increase teenage employment rates: specifically, the estimates suggest that doubling the tax base without indexing increases the teenage employment rate by about 4 percentage points. My efforts to examine the time-pattern of the effects of indexing and one-time increases in the tax base were hampered by dynamic misspecification. This is a topic for future research.

Moves to indexation permanently increase the real tax base, while tax base increases in non-indexed states erode in real terms year by year. Since the changes in firms' decisions regarding how many and what kind of workers they should hire can depend on whether the real tax base change is permanent or temporary, assuming firms have a stronger incentive to react to a permanent change, we should expect indexing the tax base to wage levels to be a more effective policy for stimulating the employment of low-wage workers.

As Vroman and Woodbury (2014) point out, indexing the UI tax base is central to the long-term health of financing UI. Following the Great Recession, UI trust funds in most states became insolvent, requiring them to take out federal loans and raise payroll taxes. This trust fund insolvency also created pressure to cut the potential duration of unemployment benefits. Indeed, seven states—Arkansas, Florida, Georgia, Michigan, Missouri, North Carolina, and South Carolina—have cut the potential duration to less than 26 weeks since 2010; these states are all non-indexed states except North Carolina. Therefore, indexing the tax base is important not only because the low tax base harms low-wage employment but also because states' responses to trust fund insolvency, in the form of decreasing potential unemployment benefits duration, has reduced the consumption smoothing benefits of UI.

Finally, it is still unclear what the optimal UI tax base is. Brechling (1977, 1980) suggests that 50% of the average wage is optimal, based on the theoretical finding that this amount will minimize labor turnover. However, other than the effect of the tax base on layoffs, it is also important to evaluate the effects on the employment mix and hours of work. This paper offers evidence that the eroding real tax base has encouraged employers to substitute high-wage workers for low-wage workers. The effect of the low tax base on the intensive margin still needs to examined.

CHAPTER 2

THE EFFECTS OF EXTENDED UNEMPLOYMENT BENEFITS: EVIDENCE FROM A REGRESSION DISCONTINUITY DESIGN

2.1 Introduction

One objective of unemployment insurance (UI) is to enable unemployed workers to search longer for suitable jobs that are in line with their previous earnings and experience. It is not yet clear, however, whether workers use the extra time allowed by more generous UI to search for better employment, or use it as leisure. Evidence from Gruber (1998), Chetty (2008), and Landais (2015a) suggests that UI has significant consumption smoothing benefits. It is therefore puzzling that only a few well-identified estimates show UI having a positive effect on reemployment wages (Nekoei and Weber, 2016), while the majority of the estimates find either an insignificant or a negative UI wage effect (Card et al., 2007; Schmieder et al., 2016).

In this paper, we approach this issue by studying the extension of unemployment benefits for older workers in Taiwan. Since 2009, workers in Taiwan aged at least 45 when they lost their job have been eligible for 9 months of unemployment benefits, rather than the 6 months offered to younger workers. The differential eligibility for extended benefits, in combination with administrative data on the population of UI recipients in Taiwan, allows us to estimate the effects of the extended benefits on the duration of unemployment and reemployment outcomes using a regression discontinuity (RD) design.

Taiwan's benefits extension is unusual, however, because claimants are paid 50% of their remaining unemployment benefit entitlement as a bonus after they become reemployed. As a result, the benefits extension not only increases the potential benefit duration, but also the bonus amount received by those who became reemployed before exhausting their UI benefits. A vast body of literature has shown that extended benefits increase unemployment duration (Solon, 1979; Mof-

[1] This chapter is coauthored with Tzu-Ting Yang. Dr. Tzu-Ting Yang is an Assistant Research Fellow at Institute of Economics, Academia Sinica. I am the first author and led the project at every stage of the research.

fitt and Nicholson, 1982; Card and Levine, 2000; Schmieder et al., 2012; Landais, 2015a). On the other hand, three random experiments conducted in the United States provide strong evidence that reemployment bonuses significantly reduce unemployment duration (Woodbury and Spiegelman, 1987; Decker et al., 2001). The counteracting effect of the reemployment bonus makes it unclear what effects Taiwan's benefits extension might have on labor supply. To help improve our understanding of how Taiwan's benefits extension might affect workers' job search behavior, we incorporate reemployment bonuses into a search model with borrowing constraints (Lentz and Tranæs, 2005; Chetty, 2008). We predict that Taiwan's benefits extension still increases the duration of unemployment because the bonus offer only partly offsets the moral hazard effect of extended benefits, while the liquidity effect of extended benefits stays intact.

In contrast to our prediction regarding the effect of a benefits extension on unemployment duration, our model does not produce sharp predictions regarding an extension's effect on reemployment wages. Job search models with borrowing constraints (Mortensen, 1986) have suggested that workers increase their job selectivity when benefits are extended, while their reservation wages decline over the unemployment spell as they become more and more liquidity-constrained (i.e., the closer they get to the exhaustion point).[2] Since extended benefits increase the duration of unemployment, it is theoretically undetermined whether UI extensions improve job match quality. Nevertheless, analysis using a job search model that included borrowing constraints has an important prediction—the reservation wage response to the benefits extension is stronger for the workers who are more likely to be exhaustees, and for those who are more liquidity-constrained by the exhaustion point.

Consistent with the theoretical prediction, our RD estimates show that a 90-day increase in potential duration (from 180 to 270 days) increased the insured duration of unemployment by 57 days (63%) and the nonemployment duration by 41 days (46%). Similar to Katz and Meyer

[2]In the classic job search model of Burdett (1979) and Mortensen (1977), increasing unemployment benefits shift the reservation wage curve outward; the reservation wage declines before the exhaustion point and stays constant afterwards. Learning about the unknown wage distribution (Burdett and Vishwanath, 1988) or skill depreciation (Pissarides, 1992) also generates a declining reservation wage.

(1990), Nekoei and Weber (2016), and Schmieder et al. (2012), our distribution of nonemployment duration displays clear spikes around exhaustion points, suggesting that workers delay finding a job, to some degree, until they have exhausted their benefits. Benefits extension not only increases the nonemployment duration for workers who would have exhausted their benefits in the absence of extended benefits, but also for those who would not. Specifically, we estimate that the benefits extension decreases the probability of finding a job within nine months of the initial claim by 21%, but that within six months of the initial claim (the original duration of benefits) the probability also declines, by 12%, suggesting workers are forward-looking in their search behavior. Furthermore, the effect of the benefits extension appears to be persistent: the probability of being reemployed within two years of the initial claim is still 3% lower for workers eligible for extended benefits.

While the estimated duration responses to extended benefits are substantial, our RD estimates show no significant effect on the reemployment earnings for overall UI recipients around 45 years old. However, we estimate that a 90-day increase in potential duration increases reemployment earnings by 2.8% for workers at the fourth quartile of the predicted probability of exhausting benefits, but has no significant impact for workers at the lower quartiles. Using average pre-unemployment earnings as an index for liquidity constraints, we estimate that the benefits extension only generates match-quality gains for lower-wage workers who are most likely to exhaust benefits. These results provide a new, more comprehensive explanation of why the majority of previous estimates did not find any positive wage effects from UI extension—the wage gains from extended benefits for these lower-wage potential exhaustees are canceled out by smaller wage effects for other workers.

The next section of this paper describes Taiwan's UI system and benefits extension program. Section 2.3 then provides a brief review of the related literature. In Section 2.4, we present our search model for predicting the effects of Taiwan's benefits extension program. Section 3.4 introduces the administrative data and the sample for empirical analysis. We apply the RD design to estimate the effects of extended benefits in Section 2.6, and discuss the liquidity effect of a benefits extension in Section 2.7. Section 2.8 concludes.

2.2 Institutional Background

2.2.1 Unemployment Insurance in Taiwan

Unemployment benefits in Taiwan form one part of the overall employment insurance program, which is a mandatory national program that offers unemployment benefits, reemployment bonuses, vocational training living allowances, parental leave allowances and national health insurance premium subsidies. It covers all Taiwanese workers, excluding civil servants and the self-employed. It is financed by 1% of the monthly insured wage: 20% is imposed on workers, 70% on employers, and the government pays the remaining 10%.

To be eligible for unemployment benefits, individuals aged 15 to 65 who lose their jobs must have at least one year of employment history in the three years prior to the job loss.[3] To receive the first month's benefits, a claimant must register with the government employment service and complete a 14-day waiting period. If the worker does not find a job by the end of the waiting period, the insured period begins (up to the maximum duration of benefits the claimant is entitled to). Since 2009, the maximum duration of benefits has been six months for workers aged below 45 at the time of job loss, and nine months for those aged 45 or older when they lost their job.[4] Unlike in the United States, where benefits are paid weekly, unemployed workers in Taiwan claim benefits on a monthly basis. The Bureau of Labor Insurance treats one month as a period of 30 days. If a worker is reemployed before the end of a given 30-day interval, the amount of benefits paid in that month is prorated. The monthly UI benefits replace 60% of the average insured wage during the six

[3] Only workers losing their jobs involuntarily or due to the ending of a fixed term contract are eligible. According to Employment Insurance Act and Labor Standard Act, involuntary separation from employment refers to separation from employment because the insured unit has closed down, relocated, suspended business, dissolved, filed bankruptcy, or business cycle induced layoff and downsizing. Employment history is the number of days for which a worker has been enrolled in the employment insurance. Since part-time workers must be insured according to the Employment Insurance Act, history as a part-time worker is included when determining eligibility.

[4] There is only one exception: UI recipients who hold disability cards are eligible for nine months of benefits regardless of their age at the time of job loss. However, few UI recipients are disability card holders; our data showed that only 0.8% of workers younger than 45 received unemployment benefits for longer than six months during our study period.

months prior to job loss[5] for those without non-working dependents. For UI recipients with non-working dependents the replacement rate is increased, and can reach as high as 80% depending on the number of dependents. Monthly unemployment benefits are subject to a maximum cap, which was NTD $26,340 ($\approx$ USD $878) throughout our study period from 2009 to 2012.

Workers are required to actively search for a job while receiving benefits. Specifically, they have to list at least two job contacts for each continued claim. In general, this work search test plays the role of the stick, promoting rapid employment via undesirable consequences.[6] The other strategy is the carrot: Taiwan's UI program offers a generous financial incentive to workers who return to work quickly. This incentive, which takes the form of a reemployment bonus, offers 50% of any remaining unemployment benefits to UI recipients who find jobs before the end of their eligibility period, and who then accumulate at least three months of employment history after reemployment. For example, a worker eligible for six months of benefits who found a job after just two months would be eligible to receive an amount equivalent to an additional two months of benefits as a bonus if she accumulated at least three months of employment history after reemployment. The three months of reemployment does not have to be continuous, or with a single employer. A person who worked for multiple employers for three months after reemployment would also qualify for the bonus.

Unlike many European countries (e.g., Austria and Germany), Taiwan's UI program does not offer means-tested unemployment assistance after the benefits have been exhausted. However, job losers in Taiwan are eligible for six months of vocational training subsidies, regardless of age, if they register with the employment service and participate in full-time vocational training. Like unemployment benefits, the monthly training subsidies equal 60% of the average insured wage during the six months prior to job loss. Workers are not eligible for unemployment benefits when they participate in vocational training. However, workers are not prohibited from claiming unem-

[5]This refers to the last six months for which a worker was enrolled in employment insurance prior to their job loss.

[6]For an introduction to and discussion of the work search test in the United States, see Woodbury (2015).

ployment benefits after completing a training program if still unemployed, or from participating in training after they have received benefits for a certain amount of time. During our study period, about 6.5% of UI benefits recipients participated in vocational training.

2.2.2 Benefits Extension for Older Workers

As shown in Figure B.8, Taiwan's reemployment bonus program took effect in 2003, and the UI extension went into effect on May 1, 2009. As a result of these two changes, Taiwan's UI extension not only extended the unemployment benefits for certain workers, but also increased the bonuses for which those workers were potentially eligible. Consider the examples of the two UI recipients in Figure B.9. Both recipients find a job at the end of the second month of their UI benefits period. UI recipient 1 aged under 45 at the time of job loss, however, is eligible for only six months of unemployment benefits, and is thus eligible for an additional two months of benefits as a bonus, while UI recipient 2 aged 45 at job loss, eligible for nine months of unemployment benefits, is eligible for a bonus equivalent to an additional three and a half months of benefits. Therefore, the reemployment bonus creates a counteracting force that mitigates the moral hazard effect of the benefits extension. In Section 2.4, we use a job search model to predict the effects of a benefits extension in a UI system with this type of reemployment bonus.

2.3 Previous Research

Our study contributes to the literature on the labor supply and wage effects of extended unemployment benefits. Here, we summarize previous works in these areas and compare our work with these closely related studies.

Early research correlating unemployment duration and reemployment outcomes to the potential benefit duration conditional on worker characteristics and the unemployment rate suffers from omitted variable biases and reverse causality, since the potential duration of benefits is a function of previous earnings and the (insured) unemployment rate, which are determinants of the labor supply.[7] Researchers have exploited the differing timing and degree of benefits extensions in various U.S. states to identify the effects of extending benefits on labor supply (Farber and Valletta, 2015; Farber et al., 2015). However, it is hard to completely eliminate the possibility that any increase in potential duration may be confounded by changes in labor market conditions when using this identification strategy. To control for market-level factors, Johnston and Mas (2015) exploit the unanticipated cut in potential duration in Missouri, comparing new UI claimants before and after the benefit cut. Studies from Europe, including Schmieder et al. (2012) in Germany, Barbanchon (2016) in France, Card et al. (2007), Lalive (2008), and Nekoei and Weber (2016) in Austria, and Centeno and Novo (2009) in Portugal, use age- or tenure-based discontinuities in the eligibility for extended benefits to isolate the labor supply response to extended benefits.

The estimated nonemployment duration elasticities in European countries and the United States range from 0.1 to 1, with a median around 0.4, and the insured duration elasticities range from 0.52 to 1.35, with a median of 0.58. Our estimated nonemployment duration elasticity of 0.3, and the insured duration elasticity of 0.78, are therefore within the ranges of previous estimates.[8] Inter-country comparison, however, is a difficult task, because the estimates from different countries can differ for many reasons, including factors such as variations in institutionals and labor market conditions, sample years, affected workers, and the measure of duration.

For simplicity, we compare our findings with those of Schmieder et al. (2012) and Nekoei and Weber (2016), who study benefits extensions for older workers in Germany and Austria, respectively. Our estimated nonemployment duration elasticity of 0.3 is larger than theirs. There are two possible explanations for this. First, the potential benefit duration in Taiwan is shorter than in Germany or Austria. This shorter potential duration increases the exhaustion rate. Since exhaustees are more affected by these benefits extensions, the duration elasticities tend to be larger for countries with shorter potential durations. Second, Taiwan does not have an Unemployment Assistance (UA) program like those in Germany and Austria. Their UA programs offer second-tier benefits

[7]Hamermesh (1977) provides a concise survey of the literature from the 1970s. For studies before the 2000s, see Krueger and Meyer (2002).

[8]See Schmieder and von Wachter (2016) for an excellent survey.

with lower replacement rates for exhaustees. The existence of these UA programs mitigates income loss following exhaustion of benefits, and decreases the net replacement rate of extended benefits, reducing the effects of the benefits extensions.[9]

On the other hand, most studies have obtained insignificant estimates for the effects of extended benefits on the reemployment wage for overall UI recipients (Card et al., 2007; Lalive, 2007; Van Ours and Vodopivec, 2008; Barbanchon, 2016). Schmieder et al. (2016) and Nekoei and Weber (2016) are notable exceptions to this trend. Schmieder et al. (2016) determined that a six-month increase in potential duration lowered the reemployment wage by roughly 0.7%, whereas Nekoei and Weber (2016)'s estimates showed that a nine-week increase in potential duration increased the reemployment wage by about 0.5%. Nekoei and Weber (2016) argue that the positive wage effect can be attributed to the smaller duration response to extended benefits in Austria. They reconciled their findings with previous estimates by demonstrating, both theoretically and empirically, a negative relationship between the effects of the benefits extension on unemployment duration and on the reemployment wage. Given that our estimated nonemployment duration elasticity is larger than that of Schmieder et al. (2016), it is not surprising that we did not find wage gain for UI recipients overall.

Several studies have identified heterogeneous effects resulting from benefits extensions or related UI programs on job match quality. Nekoei and Weber (2016) and Caliendo et al. (2013) find that benefits extensions cause a larger wage gain for benefits exhaustees. Centeno and Novo (2009) estimates that a benefits extension in Portugal increased the reemployment wage for lower-wage workers. Lachowska et al. (2016), who examine the effect of a work search test on long-term employment outcomes using the Washington Alternative Work Search experiment, provide evidence that the work test increases job tenure with the first post-claim employer for permanent job losers, and tends to select lower-wage workers into reemployment. They conclude that a work test may be

[9] The role of UA in the effects of extended benefits on unemployment duration are discussed in Schmieder et al. (2012). Schmieder et al. (2012) mention "the presence of UA after exhaustion of UI benefits should lead to smaller effects, since the strength of the disincentive effect of UI depends on the net change in replacement rates at exhaustion." They find individuals who have lower propensities to receive UA have somewhat larger duration responses to extended benefits.

an important policy for improving reemployment prospects for lower-wage, permanent job losers. Our paper relates to this line of research in that it shows that the UI extension in Taiwan generated significant wage gain for lower-wage potential exhaustees.

2.4 Theorectical Discussion

For our empirical analyses of the effects of Taiwan's benefits extension on unemployment duration and reemployment wage, we use a discrete-time search model with borrowing constraints from Chetty (2008, 178-179). We make two adjustments to Chetty (2008)'s model. First, we incorporate the reemployment bonus into the model. Second, we assume workers control their reservation wages rather than search intensities to help understand the effects of extended benefits on reemployment wages.[10] Since this chapter focuses on the effects of extended benefits rather than the effects of increasing benefit level analyzed in Chetty (2008, 178-179), we also consider Landais (2015b, 34-38)'s extension to Chetty (2008)'s model when deriving the effects of extended benefits.[11]

Our model generates three sets of predictions. First, extended benefits increase the reservation wage and decrease the reemployment probability in each period of unemployment because the reemployment bonus only partly offsets the disincentive effect of extended benefits. Second, the reservation wage response to extended benefits is stronger for workers who are more likely to exhaust their benefits and those who are more liquidity-constrained during unemployment. Third, although extended benefits increase the reemployment wage through a higher reservation wage, this effect is at least partly offset by the declining reservation wage over unemployment spell.

Consider an unemployed worker in period t, where $t \in [0, \infty)$. She samples an $i.i.d.$ wage offer from a known and stationary wage distribution, $F(w)$. If she accepts the job offer, she earns a wage

[10] The model from Chetty (2008) assumes that workers control their search effort, which determines the job finding rate. However, the model assumes a fixed wage rate, so workers' job search decision does not change their reemployment wages. For search models with reservation wages, see Burdett (1979) for a discrete-time model and Mortensen (1977) for a continuous-time model.

[11] Although Chetty (2008, 225-228) and Landais (2015b, 42-43) further include reservation wages into their models, they do not derive the effects of extended benefits on reservation wages.

rate of w, pays a tax rate of τ, and keeps the job forever.[12] If she rejects the offer, she receives an unemployment benefit, b, for the first P periods of unemployment. The worker can borrow and save: she holds assets A_t in period t and faces a borrowing constraint: $A_{t+1} \geq L$. The worker is eligible for a reemployment bonus, r_t, equal to θ percent of her remaining benefits if reemployed before running out of benefits.[13] Formally, the bonus offer can be written as

$$r_t = \theta \cdot \sum_{k=t}^{P-1} b, 0 < \theta < 1.$$

The worker receives flow utility of consumption $u(c_t^e) = u(A_t - A_{t+1} + w_t + r_t - \tau)$ when employed. Assuming a time discount rate, β, we can write the value of being employed in period t as below

$$V_t = \max_{A_{t+1}} u(A_t - A_{t+1} + w_t + r_t - \tau) + \beta V_{t+1}(A_{t+1}).$$

On the other hand, the worker receives $u(c_t^u) = u(A_t - A_{t+1} + b_t)$ when unemployed. The value of being unemployed in period t is

$$U_t = \max_{A_{t+1}} u(A_t - A_{t+1} + b_t) + \beta J_{t+1}(A_{t+1}),$$

where $J_{t+1}(A_{t+1})$ is the value of entering period $t+1$ unemployed with assets of A_{t+1}. The value of entering period t without a job is

$$J_t(A_t) = \max_{R_t} [1 - F(R_t)] E[V_t(A_t) | w > R_t] + F(R_t) U_t(A_t),$$

In equilibrium, the reservation wage, R_t, is the wage offer such that the worker is indifferent between accepting or rejecting the offer, that is

$$V_t(A_t) = U_t(A_t).$$

[12]The timing definition in Chetty (2008) is different from the conventional timing definition in the search model such as Burdett (1979). Burdett (1979) assumes the search intensity and reservation wage at time t determines the probability of reemployment at time $t+1$. Chetty (2008) follows Figure 1 of Lentz and Tranæs (2005, 471) and assumes the search intensity at time t determines the job finding rate at time t itself. The advantage of doing so is that it is easier to decompose the effect of unemployment benefits on search effort into a liquidity and a moral hazard effect.

[13]In the case of Taiwan's reemployment bonus program, $\theta = 0.5$. Here, we assume an arbitrary θ between 0 and 1.

How the benefits extension affects the reservation wage depends on how V_t and U_t change when the benefits are extended. When the bonus is present, increasing potential duration, P, not only increases U_t but also increases V_t. As equations C.1 and C.2 in Appendix C demonstrate, the partial derivatives of V_t and U_t with respect to the potential duration, P, are complex. In Appendix C, we apply Euler equations as Landais (2015b, 34-38) and show that the increase in V_t is strictly smaller than the increase in U_t if the liquidity constraint does not bind. We predict that workers will increase their selectivity for jobs since being employed is less attractive when the potential duration is extended. Specifically, we can write the effect of extended benefits on the reservation wage at time t before the exhaustion point, P as follows:

$$\frac{\partial R_t}{\partial P} = (1-\beta)[\frac{u'(c_t^u) - u'(c_t^e)}{u'(c_t^e)} + S_{t+1}(P)(1-\theta)] > 0 \tag{2.1}$$

where $S_{t+1}(P)$ is the survival rate in period P, conditional on the person being unemployed in period $t+1$. In a UI system such as Taiwan's that offers reemployment bonuses, extending the potential unemployment duration increases that duration by a smaller magnitude than in the absence of bonuses, because reemployment bonuses partially countervail the disincentive effect of extended benefits. Equation 2.5 also yields an important prediction: workers who are more likely to exhaust their benefits and experience a larger consumption drop at exhaustion point should see a larger increase in reservation wage.

Figure B.10 illustrates how an increased potential duration might affect the reservation wage over a period of unemployment. First, workers become more selective when they are eligible for a longer duration of benefits, which is reflected in the outward shift of the reservation wage curve. As shown in Equation 2.5, the magnitude of this outward shift is larger for workers who are more likely to exhaust their benefits, and to become liquidity-constrained at the exhaustion point. Second, the reservation wage declines over time partly because there are fewer remaining benefits closer to the exhaustion point, and partly because workers become more liquidity-constrained as the unemployment duration increases. For a non-exhaustee, extended benefits reduce the reservation wage in the period of reemployment if the declining reservation wage dominates the outward shift in the reservation wage. For an exhaustee, extended benefits weakly increase the reservation wage in the period of reemployment, since the reservation wage cannot go any lower.

The reservation wage, however, is not observed in the data. Instead, we observe workers' reemployment earnings. Define $\mu = E[w|w > R_D]$ as the mean accepted wage for a worker who becomes reemployed at time D. We can write the effect of extended benefits on the mean accepted wage when reemployed as

$$\begin{aligned} \frac{dE[w|w>R_D]}{dP} &= \mu' \cdot \frac{dR_D(D,P)}{dP} \\ &= \mu' \cdot [\frac{\partial R_D}{\partial D}\frac{dD}{dP} + \frac{\partial R_D}{\partial P}], \end{aligned} \tag{2.2}$$

where $\mu' = \frac{d\mu}{dR_D}$ and R_D is the reservation when workers become reemployed. Equation 2.2 demonstrates that the effect of a benefit extension on the mean accepted wage is a combination of two effects. On the one hand, extended benefits lengthen unemployment duration, D, decreasing the mean accepted wage if reservation wage declines over unemployment spells. On the other hand, extended benefits increase workers' selectivity for jobs, increasing the mean accepted wage. The effect of extended benefits on the reemployment wage is therefore theoretically undetermined. However, the empirical evidence from Krueger and Mueller (2016) suggests that the reservation wage declines only modestly over unemployment spells, and the decline is concentrated on workers with higher savings. Assuming the decline in reservation wage due to a benefits extension is smaller for lower-wage, potential exhaustees, these workers should experience larger wage gains.

2.5 Data Description

We use two sources of data from the Taiwanese Bureau of Labor Insurance: the administrative unemployment benefits files and the employment insurance enrollee file (earnings records) dating from January 1999 to December 2013. Each entry in the unemployment benefits file represents one beneficiary case, and contains each UI recipient's date of birth, date of job loss, first and last date of benefit receipt, average previous insured earnings in the six months prior to layoff (hereafter, previous earnings), an individual identifier, and some demographic information, including gender, number of dependents, place of birth, and a four-digit code indicating the recipient's previous occupation. Using the unemployment benefits file alone, we can create a dataset in which each observation represents one UI spell, containing information on the UI recipient's exact age at job loss and the insured duration of each period of unemployment, which is the total number of days for which a worker received unemployment benefits while unemployed.

We match the unemployment benefits records to the earnings records corresponding to each UI spell to construct a dataset of reemployment outcomes. In the earnings records, each entry represents a change in the employment record, such as new enrollments in employment insurance, cancellations of employment insurance (job separation), or wage changes. Using the matched dataset, we define the nonemployment duration as the total number of days from the start of receiving unemployment benefits to the next registered date of employment. UI recipients not observed to have been reemployed during our sample period were deemed to have either failed to find a job, become self-employed, or dropped out of the labor force altogether. We capped nonemployment duration at 730 days.

We examine three reemployment outcomes: the monthly earnings with the first post-claim employer, and the monthly earnings one year after and two years after the initial unemployment claim. All three of these outcomes are conditional on employment. The minimum insured earnings used in our study is equivalent to the actual minimum wage in each study year: NTD $17,280 in 2009, NTD $17,880 in 2011, and NTD $18,780 in 2012. The maximum insured earnings was NTD $43,900 ($\approx$ USD $1463) throughout the sample period. Within each year, the insured earnings was divided into 20 categories.[14]

We impose three sample restrictions on our data. First, since the benefits extension took effect on May 1, 2009, we drop any UI spell starting before that date. Second, we exclude UI spells starting after January 1, 2012; since our data ends in December 2013, any nonemployment periods for those UI spells would have a maximum potential duration shorter than 730 days. Third, we focus on workers around the age 45 cutoff. Column 1 of Table A.14 reports the summary statistics for the UI recipients aged 25-65 during the study period. This is the sample we use to choose the

[14] In 2012, for example, these categories had thresholds of $18,780, $19,200,.., $42,000, up to a maximum of $43,900.

optimal bandwidths in the RD design. The baseline RD sample is comprised of workers aged 43-46 at time of job loss, shown in Column 2 of Table A.14. The exhaustion rate for workers around age 45 is high, and their nonemployment duration is on average longer than 180 days, suggesting a substantial amount of workers do not find employment after exhausting their benefits.

2.6 Regression Discontinuity Design

Using the fact that UI recipients in Taiwan aged 45 or older at the time of job loss are eligible for nine months of unemployment benefits, while those younger than 45 get only six months of benefits, our RD design isolates variation in potential duration, as long as the workers around the age 45 cutoff are similar to each other except for their potential UI duration. Consider

$$y_i = \alpha + \beta_{EB} Age45_i + f(a_i) + v_i, \tag{2.3}$$

where y_i is an outcome variable that includes insured duration, nonemployment duration, and difference in log earnings between two periods of employment. $Age45_i$ indicates that a UI recipient was at least 45 years old at the time of job loss, and a_i is the worker's age at job loss. β_{EB} is the coefficient of interest, capturing the effects of a three-month increase in potential duration. The key identification assumption here is that the outcome variables should evolve smoothly over the cutoff in the absence of extended benefits. Note that $Age45_i$ depends solely on a_i. If the effects of age at job loss are adequately controlled by $f(a_i)$, such that $E(v_i|a_i) = 0$, β_{EB} will identify the effects of the extended benefits. For our baseline results, we estimate the equation using a sample of workers aged 43-46 at time of job loss, and consider $f(a_i)$ to be a linear function interacting with the extended benefits dummy. Specifically,

$$f(a_i) = (1 - Age45_i)[\pi_0(a_i - 45)] + Age45_i[\pi_1(a_i - 45)]. \tag{2.4}$$

Using UI recipients aged 43-46 at time of layoff as the estimation sample was an arbitrary choice, so we also calculate RD estimates using the optimal bandwidth proposed by Calonico et al. (2014), and test for the sensitivity of the bandwidth choice.

The optimal bandwidth minimizes the mean square error (MSE) of the RD estimator (Imbens and Kalyanaraman (2012)). However, Calonico et al. (2014) points out that the MSE optimal bandwidth selector chooses a bandwidth that is too large to ensure an unbiased estimate. Although conventional bias correction method removes the bias, it introduces additional variability when estimating the bias. Calonico et al. (2014)'s robust standard error take this added variability into accounts. We therefore report the robust standard errors when using bias-correction estimates.

2.6.1 Identifying Assumptions

The validity of the RD design depends on whether the UI recipients around the age-45 cutoff are similar except for their eligibility for extended benefits. Under Taiwan's UI program, it is unlikely that workers will be able to manipulate the eligibility rule for extended benefits because it is based on their age at the time of job loss rather than their age when claiming benefits.[15] It seems possible, however, that some firms might be willing to delay laying off workers for a certain period of time, so that they would qualify for extended benefits. If many firms were doing this, we would likely see a larger-than-expected number of workers just above age 45 claiming UI benefits. Furthermore, if these workers or employers fell into certain types or industries, then this sorting would not be random, and would need to be addressed. We investigate the validity of our RD design by examining the frequency of UI recipients over different ages, and the means of the observables around the cutoff, as Imbens and Lemieux (2008) and Lee and Lemieux (2010) suggest.

Figure B.11 shows the number of UI recipients of each age, from 40 to 50, at layoff. Each age bin represents the total number of new claimants in a 30-day interval. Below the age 45 cutoff, there are roughly 450 new claimants within each age interval, and the number of new claimants decreases with age at job loss. Consistent with Schmieder et al. (2012), we find that there are about 150 more workers losing their jobs within the first 30 days past age 45 than just before that cutoff,

[15]The eligibility rules for extended benefits in Germany and Austria are based on an applicant's age when claiming unemployment benefits. Schmieder et al. (2012) found a slight increase in the number of new claimants on the right of each age cutoff, and addressed this concern using a variety of methods, including adding covariates, a donut RD, and bounding.

and that the number of new claimants within a few months past age 45 is still slightly higher than that just before age 45. This increase in the number of UI recipients at and just above the cutoff is significant at the 5% level using the density test proposed by Cattaneo et al. (2016). However, it accounts for less than 1% of workers aged 43-46 at the time of job loss, and is thus unlikely to invalidate our RD design. To alleviate the concern that this small discontinuity in bin size at the cutoff might bias the RD estimator, we implement the donut RD strategy suggested by Barreca et al. (2016). We exclude observations within 180 days around the cutoff to examine how selective layoff around the cutoff affects the results. Our results are robust to this removal of observations.

To check for the possibility of non-random sorting, we look for any discontinuities in the means of workers' characteristics around the cutoff. Figure B.12 plots the number of days between losing one's job and claiming benefits, whether workers previously worked in the manufacturing industry, their average log wage in the six months prior to layoff, whether workers were female, workers' number of dependents, and workers' predicted nonemployment duration. The means either evolved smoothly or showed economically small discontinuities around the cutoff. To make sure the small discontinuities at the cutoff did not invalidate our RD design, we estimate the average nonemployment duration by regressing nonemployment duration on available observables, excluding the treatment indicator as suggested by Card et al. (2007). Figure B.12 shows that the predicted nonemployment duration is smooth around the age 45 cutoff. In Table A.15, we estimate a local linear regression using the pre-determined observables as dependent variables of equation 2.3. The estimates are either insignificant or small. In particular, the estimated effect of extended benefits on the predicted nonemployment duration is insignificantly different from zero, suggesting the workers near the cutoff are comparable to each other.

2.6.2 Results on Duration Outcomes

The eligibility rule for extended benefits in Taiwan generates clear discontinuities in the relationship between age and duration outcomes, with no discernible change in wage growth at the age cutoff. Figure B.13 plots the average outcomes against age at job loss using a [40, 50] age window.

Each bin indicates the conditional mean within a width of 30 days. As shown in Figure B.13 (a) and (b), the average number of days of receiving benefits shifts up by about 60 days after the cutoff, while the average nonemployment duration shifts up by roughly 40 days. In Figure B.13 (c), we plot the probability of having a nonemployment duration of less than 180 days. The probability shows a sharp decline at the cutoff. It means some workers having nonemployment duration shorter than 180 days in the absence of the extension have nonemployment duration longer than 180 days when the potential duration increases from 180 to 270 days, suggesting workers are not myopic in terms of labor supply. Importantly, there are no comparable discontinuities at other age points, and the trend relationships between age and duration outcomes is approximately linear. Overall, the RD graphs of the duration outcomes imply that extending the potential duration lowered search intensity in a UI system with a variable reemployment bonus.

Column 1 of Table A.16 reports our baseline estimates for the effect of extended benefits on insured duration, nonemployment duration, and the probability of finding a job within 180 days of the initial claim. An increase in potential benefits duration from 180 to 270 days is estimated to increase the insured duration of unemployment by about 58.0 days, a 39% increase in the average insured duration that implies an elasticity of 0.78 with respect to potential duration. The estimated effect of extended benefits on nonemployment duration is about 41.1 days, a 15% increase in nonemployment duration that implies an elasticity of 0.3.[16] A benefits extension also decreases the likelihood of having a nonemployment duration lower than 180 days by about six percentage points, a 12% decline in the baseline mean. Including covariates in column 2 barely affects these estimates or their precision.

In columns 3 to 6 of Table A.16, we use the optimal bandwidth by Calonico et al. (2014) and Calonico et al. (2016). This algorithm chooses a somewhat larger bandwidth than two years, in the range of three to six years. In general, our estimates are robust to using the optimal bandwidths, and the robust standard errors were also similar to the conventional ones.

[16] The insured duration elasticity equals the percentage change in insured duration divided by the percentage change in potential duration, which is $\frac{57.96/147.32}{(9-6)/6}$. Similarly, the nonemployment duration elasticity is $\frac{41.14/276.39}{(9-6)/6}$.

In order to investigate how a benefits extension changes the distribution of nonemployment duration, we examine the cumulative distribution of nonemployment duration (shown in Figure B.14) and the density of nonemployment duration (shown in Figure B.15). Specifically, we plot $P(\text{nonemployment duration} \leq k)$ and $P(k-30 < \text{nonemployment duration} \leq k)$, where k ranges from 30 to 730 days, for workers aged 43-44 and 45-46. The cumulative distribution of nonemployment duration highlights three points. First, the cumulative probability is significantly smaller for workers eligible for extended benefits even when k is smaller than 180 days, indicating that workers respond to the benefits extension before exhausting their regular benefits. Second, the difference in the cumulative probabilities is particularly large between $k = 180$ and $k = 270$. This supports the theoretical prediction that workers who would otherwise exhaust their benefits are most affected by an extension. Third, the difference begins to shrink around the exhaustion point of extended benefits, but this difference has not completely disappeared even two years after the initial claim, providing evidence that a benefits extension might have medium-term effects on labor supply. On the other hand, the density distributions show that the density for each age group declines over nonemployment duration, while it exhibits a spike right after the maximum duration of benefits. These spikes are similar to those found by Schmieder et al. (2012), Schmieder et al. (2012), and Nekoei and Weber (2016), all of whom find spikes in the reemployment hazard around exhaustion points.

To make sure the difference between the distribution of nonemployment duration for the two age groups is not due to the difference in age, we plotted the probability of being reemployed within k days conditional on age in Figure B.16. The probabilities show visible drops at the cutoff, but they are smooth at other age points, suggesting that this difference in the distribution of nonemployment duration can be attributed to the differential eligibility for extended benefits. In Table A.17, we estimate the effect of the benefits extension on the probability of finding a job in k days using Equation 2.3. We use the optimal bandwidths, and specified a linear function on either side of the cutoff. Column 1 shows the effect of the benefits extension on the probability of finding a job within 90 days to be negative, but statistically indistinguishable from zero. In Columns 2 and 3, we estimate the probability of finding a job within 180 days and 270 days of the initial claim declined by 12% and 21%, respectively. The magnitude of the decline in cumulative probability decreases after the extended benefits exhaustion point, but the declines are still statistically significant: 8% and 3% in terms of the probability of being reemployed after 360 and 730 days.

We are especially interested in the declines in the probability of being reemployed around the exhaustion point, because the main policy goal is to improve these workers' ability to maintain consumption upon benefit exhaustion. The decline might occur because extended benefits provide additional liquidity for exhaustees, or because workers decrease their job search efforts in order to claim extended benefits. Rothstein and Valletta (2014) and Ganong and Noel (2016) show that workers experienced a significant consumption drop upon exhausting their benefits, suggesting that the liquidity effect of extended benefits might play an important role in the effects of extended benefits on the timing of reemployment. We will return to this issue in Section 2.7 below.

2.6.3 Results on Reemployment Earnings

A natural follow-up question is whether workers invest the extra time afforded by a benefits extension in searching for better employment, or use that extra time for leisure. We examine this issue by estimating the effect of the benefits extension on reemployment earnings. Figure B.13 (d) displays the difference between the log pre-unemployment earnings and the log earnings with the first post-claim employer conditional on age at job loss, where the solid lines are fitted values from a local linear regression on either side of the cutoff. While job losers around age 45 suffer from a 23% wage decline upon reemployment, there is no discontinuity at the cutoff, indicating that extended benefits have little impact on the reemployment earnings with the first post-claim employer for UI recipients around age 45 overall. The RD estimates in Table A.18 also contain no evidence of wage gains with the first post-claim employer in any of the specifications.

It is somewhat puzzling that the benefits extension is not estimated to produce match quality gains. As Equation 2.5 shows, a benefits extension increases workers' reservation wages in each post-claim period, so we should expect higher reemployment wages. This argument, however, ignores the role of the declining reservation wage over a spell of unemployment. On the one hand, models with liquidity constraints (Mortensen, 1986; Lentz and Tranæs, 2005) demonstrate that workers lower their reservation wage closer to the constraint. On the other hand, previous literature has discussed possible mechanisms for the declining reservation wage, including a time-varying benefit schedule (Mortensen, 1977), learning (Burdett and Vishwanath, 1988), and skills depreciation (Pissarides, 1992). The insignificant wage effect implies that increased selectivity is offset by the declining reservation wage.

Another explanation for the insignificant estimated effect of the benefits extension on reemployment earnings with the first post-claim employer is that the match quality gain from extended benefits is reflected in other dimensions of job quality (Card et al., 2007). For example, a benefits extension might help workers find jobs with higher wage growth after reemployment, which would not be captured by the reemployment earnings with the first employer. In Panels B and C of Table A.18, we report the estimated effect of increasing the potential duration of benefits from 180 to 270 days on the log of monthly earnings one and two years after the initial claim. If a benefits extension helps workers to find jobs with higher wage growth, we expect to find the wage effect of the benefits extension was higher two years after the initial claim than one year after. However, the estimates suggest that a three-month increase in potential duration had no significant impact on the monthly earnings one year after the initial claim, and actually indicate a insignificant decrease in the monthly earnings two years after the initial claim. Thus, our RD estimates show no evidence that a benefits extension improves wage growth after reemployment.

One concern regarding our estimated wage effects is that they might be affected by dynamic selection since the wage sample is conditional on employment (Lachowska et al., 2016; Heckman and Hotz, 1989). If, for example, lower-wage workers are more affected by the benefits extension, we will be less likely to observe lower-wage workers who are employed after initial claim, leading to an upward bias of our estimated wage effects. Table A.19 examines whether the previous earnings and other predetermined variables for reemployed UI recipients who are eligible for extended benefits are different from those who are eligible for regular benefits. Specifically, we estimate the

RD regression using previous earnings (and predetermined variables) as the dependent variable and sample from workers who were reemployed before the end of sample period, one year and two years after initial claim. We find reemployed workers on the right of the cutoff are about 2% more likely to have worked in manufacturing sector prior to unemployment, but the means of the other covariates, including previous earnings and predicted reemployment earnings, do not show significant differences around the cutoff. Therefore, the observations around the cutoff conditional on employment appear to be comparable to each other.[17]

2.6.4 Effects of Extended Benefits by Predicted Liklihood of Exhausting Benefits

Equation 2.5 predicts that workers who were more likely to exhaust their benefits would be more affected by the extension. As seen in Figure B.14 and Table A.17, the labor supply response is largest around the exhaustion point. If the labor supply response to extended benefits mostly occurs around exhaustion, then the duration and reservation wage response should be largest for those most likely to exhaust benefits.

To explore how the effects of the benefits extension on duration and the reemployment earnings vary across the likelihood of exhausting one's benefits, we first estimate the probability that workers will exhaust their six months of unemployment benefits, similarly to Nekoei and Weber (2016). Specifically, using all unemployment spells before the reform, we estimate a linear probability model for a dummy variable denoting whether workers exhausted their unemployment benefits using a set of predictors, including previous earnings, square of previous earnings, previous industry, gender, birthplace, number of UI spells prior to job loss, month when unemployed, number of days between job loss and initial claim, and whether a worker was recalled to work by a previous employer. We then divide our sample into quartiles based on their predicted probability of exhausting benefits, and estimate the effects of a benefits extension for each group using the RD design.

[17] However, it worth further examination that whether the earnings a few years prior to the initial claim are different between reemployed workers around age 45, since Lachowska et al. (2016) finds that reemployed workers in the work test group on average earn lower wages two years and three years prior to initial claims comparing to those in no work test group.

In Table A.20, we report the baseline means and RD estimates for each quartile. The more likely workers are to be exhaustees, the longer the insured duration and nonemployment duration are likely to be. Workers also experience a larger wage drop if they are more likely to exhaust their benefits. Consistent with the predictions of Equation 2.5, the RD estimates demonstrate that the benefits extension has the strongest effects on duration outcomes and the reemployment earnings for workers who are most likely to exhaust their benefits. To be specific, the estimates shown in Column 4 suggest that, for workers in the fourth quartile of the predicted probability of exhausting benefits, a 90-day increase in potential duration raises insured duration by 64 days and nonemployment duration by 50 days, larger increases than the point estimates for the lower quartiles. Meanwhile, the estimates show a significant increase in reemployment earnings by 2.8% for workers in the highest quartile, but not for the other subgroups. Hence, the UI extension seems more beneficial for workers who are more likely to exhaust their benefits.

2.6.5 The Role of Liquidity Constraints

Since workers tend to experience a significant consumption decline when exhausting their unemployment benefits (Ganong and Noel, 2016; Rothstein and Valletta, 2014), we would expect extended benefits mitigate exhaustees' consumption loss and give them more time to find better employment. In this subsection, we examine the role of liquidity constraints for potential exhaustees by further splitting the workers by the median of average previous earnings (Centeno and Novo, 2009). Columns 1 and 2 of Table A.21 indicate that the wage gains for workers most likely to be exhaustees were only significant for lower-wage workers, suggesting the liquidity effect of extended benefits might play an important role. The findings that only lower-wage potential exhaustees experience wage gains are consistent with Equation 2.5—workers who experience a larger consumption drop will increase their reservation wage in a greater magnitude.

An alternative explanation for the positive wage effects for lower-wage, potential exhaustees is that lower-wage workers might have a higher chance to draw a better offer in a given wage distribution. This explanation implies lower-wage workers are more likely to experience wage gains regardless of their predicted probability of exhausting benefits. However, for workers in lower quartiles of predicted benefits-exhaustion probability, neither lower-wage nor higher-wage workers are estimated to experience a positive increases in reemployment earnings. Therefore, the exercise conducted in Table A.21 offers some evidence that extended benefits alleviate workers' liquidity constraints when they run out of benefits and allow them search longer for better jobs.

2.6.6 Robustness

We have demonstrated that our results are robust to a variety of specifications. This section discusses the additional robustness analyses: checking the sensitivity of the bandwidth choice, using a donut RD design, and conducting a placebo test. We also examined whether the presence of a training program played a role in our RD estimates.

To test the sensitivity to the bandwidth choice, we plotted the estimated effects of extended benefits and their 95% confidence intervals using our baseline specification and a bandwidth from 40 to 2000 days (see Figure A1). When the bandwidth was smaller than about 400 days, the estimates were relatively volatile and imprecise. As the bandwidth size increased, the confidence intervals became narrower, and the estimates stabilized at a bandwidth of around 600 days. The robustness of our RD estimates to a variety of specifications and bandwidths assured that the estimates did not pick up a nonlinear relationship between the worker's age and the outcomes.

To mitigate the concern that the small discontinuity at the cutoff might bias our RD estimates, we intentionally exclude observations close to the cutoff, as suggested by Barreca et al. (2011) and Barreca et al. (2016), and estimate a linear regression on either side of the cutoff using the optimal bandwidth. Tables A1 and A2 show the estimated effects of extended benefits on duration and earnings, after removing observations from within one, two, three, four, five, and six months of the cutoff. The estimated effects on duration outcomes and reemployment earnings are robust to excluding observation within six months of the cutoff.

As a placebo exercise for our RD estimates, in Figure A2, we plot the average outcomes conditional on age at layoff before the reform. There are no discernible discontinuities at the cutoff.

The estimates contained in Table A3 also suggested there were no permanent differences between workers laid off on either side of the age threshold. The smooth relationships of workers' age at layoff with the observed covariates after the reform and the outcomes before the reform raise our confidence in the validity of the RD design.

Finally, the fact that job losers in Taiwan are not only eligible for unemployment benefits but also vocational training subsidies arouses concern over whether the benefits extension induces any interaction between the two programs. It is possible that extended benefits might be a substitute for the training subsidies. In particular, claiming extended benefits might be less costly than participating in a full-time training class. Since extended benefits increased the duration of unemployment, though, it is also possible that a longer period pf unemployment might increase a worker's chances of participating in the training program. To explore these possibilities, we examine whether an increase in potential duration changed the probability of participating in training and the number of days for which workers received training subsidies (see Table A4). We find that the use of the training program is not affected by the benefits extension, mitigating the concern that our RD estimates might have been affected by the presence of training subsidies. The results, however, are not surprising given the fact that only 8% of UI recipients ages 43–46 participated in the training program.

2.7 Discussion of Liquidity Effects of Benefits Extension

The positive wage effect of the UI extension for potential exhaustees (those with a predicted probability in fourth quartile above) who earned below the median of the income distribution provides evidence suggestive of the existence of a liquidity effect resulting from the benefits extension. In this section, we gauge the magnitude of the liquidity effect of extended benefits by comparing the wage responses to the benefits extension for low-wage and high-wage potential exhaustees in Table A.21, and discuss its limitations.

In the Appendix C, we show the effect of extended benefits on the reservation wage is a combination of a liquidity effect ($\frac{\partial R_t}{\partial A_t}$) and a moral hazard effect ($-\frac{\partial R_t}{\partial r_t}$).

$$\frac{\partial R_t}{\partial P} = b(1-\beta)[\frac{\partial R_t}{\partial A_t} - S_{t+1}(P)(1-\theta)\frac{\partial R_t}{\partial r_t}]. \tag{2.5}$$

Both effects increase the reservation wage. The reemployment bonus counteracts the moral hazard effect by offering θ percent of remaining benefits for workers reemployed before the benefits' exhaustion point. Nevertheless, extended benefits are predicted to increase the reservation wage because the bonus offer only partly offsets the moral hazard. Equation 2.5 is the reservation wage version of the formulas in Chetty (2008) and Landais (2015a), with an adjustment for the moral hazard effect.

Based on Equation 2.2, the difference between the wage responses for low-wage and high-wage potential exhaustees is the difference in the two group's liquidity effects if the decline in reservation wage are similar between the two groups and the moral hazard effect of extended benefits does not vary over the wage distribution. Assuming the liquidity effect for high income individuals is zero, we can obtain the ratio of the liquidity effect to the total effect of UI extension via:

$$\frac{0.035-0.023}{0.035} = 0.34. \tag{2.6}$$

This means that the liquidity effect of the benefits extension explains 34% of the effect of the benefits extension on the reemployment wage for potential exhaustees whose previous wages were below the median. This calculation is not satisfactory for at least four reasons. First, it underestimates the liquidity effect if the liquidity effect also exists for high-wage workers. Second, it overestimates the liquidity effect if the marginal utility of consumption when employed is lower (smaller substitution effect) for high income individuals. And third, Krueger and Mueller (2016) find that the reservation wage declines significantly over unemployment spells for workers with higher savings, but not for those with lower savings. Hence, if wage and savings are positively correlated, the smaller wage effect of extended benefits for high-wage workers might be attributed to a steeper decline in reservation wage over an unemployment spell, rather than a smaller liquidity effect. Fourth, and most importantly, we have imposed a strong assumption that the effect of extended benefits on the reemployment wage is the same as the reservation wage response, which almost surely does not hold true when the reservation wage or wage offer distribution over unemployment spells exhibit significant non-stationarities.

We are currently investigating a different way of disentangling the liquidity effect from the total effects of extended benefits. We are using the variation brought about by the reach back provision of the reemployment bonus program to estimate the effects of the reemployment bonus as well as the moral hazard effect of extended benefits. This will perhaps be the most important extension of this paper, because it will allow us to recover the liquidity effect and estimate the welfare effect of a benefits extension.

2.8 Conclusion

In this article, we study the unemployment benefits extension for Taiwanese workers aged 45 and over. Taiwan's benefits extension is different from those in other countries due to its reemployment bonus program. Workers can receive half of their remaining benefits as a bonus after they become reemployed, meaning the benefits extension not only increases the potential duration of benefits but also the bonuses for workers reemployed before exhausting their benefits. Using a search model with borrowing constraints, we find that the bonus offer reduces the moral hazard effect of the benefits extension by 50%, while it did not change the liquidity effects of extended benefits. Since both the liquidity and moral hazard effects increased unemployment duration and the reservation wage, the model predicted that Taiwan's version of a benefits extension lengthens unemployment duration as well as raising the reservation wage. Furthermore, the model generated a testable prediction that workers who were more likely to exhaust benefits and become liquidity-constrained at the exhaustion point would be more responsive to a benefits extension.

Empirically, we use the administrative records for the population of UI recipients and the RD design to estimate the effects of extended benefits on unemployment duration and reemployment wage. Our RD design yields three sets of findings for UI recipients around 45 years old. First, an increase in potential benefit duration from 180 to 270 days increases nonemployment duration by 39% and insured duration by 15%, both of which are within in the range of previous estimates from the United States and European countries. Second, the three months' increase in potential duration decreases the probability of being reemployed within 180 and 270 days of the initial claim by 12% and 21%, respectively. Moreover, the probability of finding a job within 730 days is still 3% lower for workers eligible for extended benefits. Third, we do not find any job match quality gain due to the benefits extension for overall UI recipients.

Guided by the theoretical predictions of the search model, we examine how the duration and wage effects of the UI extension varied across the probability of exhausting one's benefits as well as across the degree of liquidity constraint. Our estimates show that workers who are most likely to run out of benefits see a greater increase in duration, and experience a significant wage gain of 2.8%, while other workers did not. The positive wage effect for potential exhaustees was significant at 3.5% for lower-wage workers, but it was insignificant for higher-wage workers. The wage effect for overall UI recipients was small and insignificant, which might be because the wage gains are canceled by the insignificant wage effect for potential non-exhaustees or workers who could sustain consumption after exhausting their benefits. These results suggest that the ratio of exhaustees who are liquidity-constrained at the exhaustion point can be an important determinant of the wage effect of extended benefits. It will be helpful for a meta analysis to be conducted in the future, with the exhaustion rate and some measure of liquidity constraint correlated to the estimated wage effects of a benefits extension.

Since extended benefits are estimated to increase the reemployment earnings only for lower-wage potential exhaustees, one might argue it is welfare enhancing to tie the potential benefit duration to workers' previous earnings and their predicted likelihood to exhaust benefits. However, it might not be appropriate to do so because the duration response to extended benefits is also larger for these workers. In the future, it will be useful to study the optimal UI design in a theoretical framework that allows potential duration varies over individual characteristics, such as previous earnings or age.

Finally, we still have limited knowledge of the ratio of the liquidity effect to the moral hazard effect of UI extension. To fill this knowledge gap, we propose an approach that compares the wage response to a benefits extension between high-wage and low-wage workers. Assuming the mean wage response is the same as the reservation wage response and the moral hazard effect was constant over the wage distribution, we estimate the liquidity effect accounts for 34% of the total effect of a benefits extension on the reemployment wage for potential exhaustees who earned below the median wage prior to job loss. The robustness of this result requires further investigation.

CHAPTER 3

ESTIMATING THE EFFECTS OF A TIME-VARYING REEMPLOYMENT BONUS

3.1 Introduction

An inevitable cost of unemployment insurance (UI) is that it reduces unemployed workers' incentives to search and lengthens unemployment duration. To put unemployed workers back to work, countries like Netherlands, Hungary and South Korea provided reemployment bonuses for workers finding jobs early before they run out of unemployment benefits.[2] In the U.S., field experiments suggest bonuses significantly reduce duration of unemployment (Woodbury and Spiegelman, 1987; Decker et al., 2001). However, the structure of Taiwan's bonus program differs from the bonuses tested in the U.S., so it is useful to examine Taiwan's program separately.

The purpose of this chapter is to estimate the incentive effects of the reemployment bonus program in Taiwan. It offers 50% of the remaining UI entitlements to workers who became reemployed before exhausting their unemployment benefits and remain employed for at least three months after leaving UI. The Taiwanese bonus program was implemented in 2003, and it reached back to incumbent claimants who entered UI prior to the reform, as discussed below. We consider the period before the bonus program took effect as a comparison group for the bonus offer periods. Since only incumbent claimants after 2003 are eligible for a bonus, and the bonus offer declines over duration of unemployment, whether we can identify the effects of the bonus program on reemployment hazard relies on whether business cycle and duration dependence are adequately controlled. We use a discrete-time hazard model to attempt to separate calendar time effects and duration dependence from the effect of the bonus program.

[1]This chapter is coauthored with Tzu-Ting Yang. Dr. Tzu-Ting Yang is an Assistant Research Fellow at Institute of Economics, Academia Sinica. I am the first author and led the project at every stage of the research.

[2]Van der Klaauw and Van Ours (2013) estimate the labor supply effect of the reemployment bonus program for welfare recipients in Rotterdam. The bonus programs in Hungary and South Korea are discussed in Lindner and Reizer (2016) and Kim et al. (2012), respectively.

Using administrative UI claim and corresponding earning records, we find that the bonus program in Taiwan provides unemployed workers strong incentives to accept reemployment—the bonus program is estimated to increase the reemployment hazard rate in the first four months of a nonemployment spell by more than 40 percent. Consistent with the declining bonus offer schedule, the bonus program increases the reemployment hazard by 20-30 percent in the following three months, and it gradually disappears afterwards.

This study connects to the four random experiment conducted in New Jersey, Illinois, Washington and Pennsylvania (Corson and Spielgelman, 2001). Although the designs of bonuses differ from each other, these experiments suggest bonuses significantly reduce insured duration of unemployment by about one-half week. For example, the Illinois bonus offered 500 dollars (about four weeks of unemployment benefits) if workers who found a job before the eleventh week of insured unemployment, and if they held the job for at least four months. Woodbury and Spiegelman (1987) found that offering such a bonus reduced insured duration by about 5% without the lowering reemployment wage. Davidson and Woodbury (1991)'s estimates also show the Illinois bonus reduces the insured duration by 0.75 weeks for workers eligible for 26 week of benefits (1.75 weeks for those eligible for 38 weeks).

Among bonus experiment, New Jersey's design is the most similar one to Taiwan's design. It provided 50% of remaining entitlement, and the amount declines 10% per week. Anderson (1992) finds that the New Jersey bonus increases the reemployment hazard early in the offer period and the effect diminishes over time. However, the bonus offers were made after seven weeks of insured unemployment, and participants did not know an offer would be made before that time. Hence, the New Jersey's experiment is not externally valid because in a real program, individuals would know that a bonus offer would be made in week seven.

The next section of this chapter describes the reemployment bonus program in Taiwan and explains what the program means for cohorts entering UI at various calendar times. Section 3.3 discuses the bonus impact on labor supply using a search model. Section 3.4 introduces the administrative UI data and the sample for empirical analysis. We provide descriptive evidence for how the hazard function evolves over time in Section 3.5 and specify our hazard model in Section 3.6. Section 3.7 contains our estimates for the effects of the bonus program on the reemployment hazard. Section 3.8 concludes.

3.2 Taiwanese Reemployment Bonus Program

Before 2000, the unemployment rate in Taiwan was rarely over 3%, but it began to rise in 2000 and reached over 5% by mid 2001, and 2002 (Figure E.3). The increasing unemployment rate in the beginning of 2000 is consistent with the dramatic increase in the UI caseload during the same period (Figure E.4). To encourage unemployed workers to return to work, the reemployment bonus program was announced in May 2002 and implemented in January 2003. It offers 50% of the remaining UI entitlement to a recipient who becomes reemployed before exhausting his/her six months of benefits, and if they remain employed for at least three months. The three months of reemployment does not have to be continuous, or with a single employer. A person who worked for multiple employers for three months after reemployment would also qualify for the bonus.[3]

In Taiwan, workers who have one year of work experience in the three years prior to job loss are eligible for six months of unemployment benefits. Unemployment benefits recipients were eligible for six months of benefits regardless of age before 2009. Since 2009, the potential benefit duration has been extended to nine months for workers aged 45 or older. Importantly, the one year of work experience cannot be repeatedly used to initiate a new UI claim. If a worker finds a job and becomes unemployed again, he is eligible for his remaining UI entitlement. For example, if a UI recipient finds a job in the end of the second month of his UI spell, works for one month, and is then laid off, he is eligible for the remaining four months of unemployment benefits, rather than six months of benefits. This worker is not eligible for a bonus because he did not remain employed for at least three months. However, if he is reemployed before he exhausts the four months of remaining benefits and works for at least three months or more, he will be eligible for a bonus.[4]

[3]The three months reemployment period does not include recalls (the work experience in the firm prior to layoff).

[4]In order to initiate a new claim for six months of benefits, the worker must accumulate an

The bonus program reached back to UI recipients who were receiving benefits when the program took effect in 2003. Figure E.5 illustrates the reach back provisions of the bonus program using three examples. Claimant A started his UI spell on June 1, 2002, and would not have been eligible for the reemployment bonus even if he had found a job before exhausting his benefits, because the bonus program started on January 1, 2003, after this claimant would have exhausted benefits. Claimant B started his UI spell on August 1, 2002 and would have been eligible for half a month of benefits as a bonus if he had found a job on January 1, 2003 and kept the job for three months. If this claimant had found a job before January 1, 2003, however, he would not have been eligible for the bonus because the program had not yet started. If this claimant had found a job after February 1, 2003, he would not have been eligible for a bonus either, because he would have exhausted his benefits by then. Claimant C started a UI spell on January 1, 2003 and would have been eligible for two months of benefits as a bonus if he had found a job on February 1, 2003. The end of his qualification period would have been July 1, 2003. If he had found a job after that date, he would not have been eligible for a bonus.

The introduction of the bonus program creates useful variation in bonus eligibility, allowing us to separate the effect of the bonus program from effects of the business cycle. In Figure E.6, I plot the bonus offer for three cohorts entering UI at different times. Cohorts entering UI before July 2002, which is six months away from January 2003 would not be entitled to a bonus because they would have exhausted their benefits when the bonus program took effect. Cohorts entering UI in October 2002 would be entitled to a bonus if they were reemployed between the 90th and 180th day of their UI spell because the bonus program took effect on the 90th day of their UI spell. Finally, cohorts entering UI after January 1, 2003 would be entitled to a bonus as long as they were reemployed before exhausting benefits and remained employed for at least three months.

In short, cohorts starting their UI spells before July 2002 were not exposed to the bonus program. Cohorts starting UI spells between July 1, 2002 and December 31, 2002 were partially exposed to the program due to the reach back provision. Cohorts starting UI spells after January

additional year of work experience.

2003 were fully exposed to the program. As we will further discuss in Section 3.7, the variation in bonus entitlement rules across inflow cohorts and within cohorts over calendar times will help us identify the effect of bonus program.

3.3 Theoretical Discussion

In this section, we adopt a discrete-time search model with borrowing constraints from Chetty (2008, 178-179). We make two adjustments to Chetty (2008)'s model. First, we incorporate the reemployment bonus into the model. Second, we assume workers control their reservation wages rather than search intensities to help understand the effects of extended benefits on reemployment wages.[5] We also consider Landais (2015b, 34-38)'s proposal that uses Euler conditions to simply model derivations. The model introduced below predicts that the bonus will reduce workers' selectivity for jobs, which is capture by a lower reservation wage, and increase job acceptance rate. We lay out the model and state the main results, leaving the theoretical derivations in Appendix C.

Consider an unemployed worker becomes unemployed at time t and holds an initial asset A_t. She lives for infinite periods of time and draws a wage offer, w, from a known and stationary wage distribution, $F(w)$, in each period of unemployment. If she rejects the offer, she receives an unemployment benefit, b_t, with a potential duration, P, that is

$$b_t = \begin{matrix} b, & \text{if } 0 \le t < P \\ 0, & \text{if } t \ge P \end{matrix}$$

If she accepts the offer, she earns a wage rate w_t, pays a tax rate, τ, and keeps the job forever.[6]

[5] The model from Chetty (2008) assumes that workers control their search effort, which determines the job finding rate. However, the model assumes a fixed wage rate, so workers' job search decision does not change their reemployment wages. For search models with reservation wages, see Burdett (1979) for a discrete-time model and Mortensen (1977) for a continuous-time model.

[6] The timing definition in Chetty (2008) is different from the conventional timing definition in the search model such as Burdett (1979). Burdett (1979) assumes the search intensity and reservation wage at time t determines the probability of reemployment at time $t+1$. Chetty (2008) follows Figure 1 of Lentz and Tranæs (2005, 471) and assumes the search intensity at time t determines the probability of finding a job at time t itself. The advantage of doing so is that it is easier to decompose the effect of UI into a liquidity and a moral hazard effect.

Moreover, if she is reemployed before running out of benefits, she receives a reemployment bonus, r_t, equal to θ percent of remaining benefits; otherwise, $r_t = 0$. Formally,

$$r_t = \theta \cdot \sum_{k=t}^{P-1} b_k, 0 < \theta < 1$$

The worker's consumption at time t equals the difference in income and saving. The income depends on her employment status, while the change in asset, $A_{t+1} - A_t$ reflects her saving. The flow utility when employed at time t equals $u(c_t^e) = u(A_t - A_{t+1} + w + r_t - \tau)$, where c_t^e indicates the consumption when employed at time t. Assuming the the time discount rate equals β, the value of being employed in period t is

$$V_t = \max_{A_{t+1}} u(A_t - A_{t+1} + w + r_t - \tau) + \beta V_{t+1}(A_{t+1})$$

If an unemployed worker cannot find a job in period t, her flow utility is equal to $u(c_t^u) = u(A_t - A_{t+1} + b_t)$. The value of being unemployed in period t is

$$U_t = \max_{A_{t+1}} u(A_t - A_{t+1} + b_t) + \beta J_{t+1}(A_{t+1}),$$

where $J_{t+1}(A_{t+1})$ is the value of entering period $t+1$ unemployed with asset A_{t+1}. The value in the beginning of period t without a job is

$$J_t(A_t) = \max_{R_t} [1 - F(R_t)]V_t(A_t) + F(R_t)U_t(A_t),$$

In the search model, the optimal reservation wage equates the value of employment, $V_t(A_t)$, and the value of unemployment, $U_t(A_t)$. That is,

$$V_t(A_t) = U_t(A_t)$$

The reemployment bonus, r_t, increases $V_t(A_t)$ but leaves $U_t(A_t)$ intact. As a result, workers are willing to lower their selectivity for jobs to receive the bonus. Formally,

$$\frac{\partial R_t}{\partial r_t} = -(1 - \beta) < 0.$$

A lower reservation wage implies a higher job acceptance rate, $1 - F(R_t)$. The search model hence has a sharp prediction that the bonus offer will increase the reemployment hazard. We examine the theoretical prediction in the empirical analysis.

3.4 Data and Sample

The data we use in this chapter are the administrative unemployment benefits files and the corresponding earnings records dating from January 1999 to December 2013. Each entry in the unemployment benefits file represents one beneficiary case and contains each UI recipient's date of birth, date of job loss, first and last date of benefit receipt, average previous insured earnings in the six months prior to layoff (hereafter, previous earnings), four-digit previous occupation, an individual identifier, and basic demographic characteristic, including gender, number of dependents, and place of birth. Using the unemployment benefits file alone, we can create a dataset in which each observation represents one UI spell, containing information on the insured duration of each period of unemployment, which is the total number of days for which a worker received unemployment benefits while unemployed.

We match the unemployment benefits records to the earnings records corresponding to each UI spell to construct data on spells of nonemployment. In the earnings records, each entry represents a change in the employment record, including any new enrollment in UI, job accession, or wage changes. Using the matched dataset, we define nonemployment duration as the total number of days from the start of receiving unemployment benefits to the next registered date of formal employment. UI recipients not observed to have been reemployed during our sample period were deemed to have failed to find a job, become self-employed, or dropped out of the labor force altogether.

Our estimation sample contains 228,095 UI spells initiated between January 1, 2001 and December 31, 2004. Table A.22 reports the summary statistics. We break the sample into three samples separated by two dates, July 1, 2002 and January 1, 2003. The insured duration and nonemployment duration both decrease over calendar time, which might be partly because the labor market conditions improved over time and partly because the bonus program was introduced in the beginning of 2003.

3.5 Descriptive Evidence

We follow each spell from the date of initial claim to the next registered date of formal employment. We group the data into 30-day intervals, indexed by d and define the hazard rate as the probability of being reemployed during the 30-day interval $(d-1,d]$, given that an individual has not been reemployed at the start of d. Table A.23 estimates the hazard rate using the full sample (the set up of this table follows Davidson and Woodbury (1991), Tables 4 and 5). In Column 1, I report the number of workers who are at risk of being reemployed in a given 30-day period. The estimated hazard in Column 2 is computed by dividing the number of individuals who are reemployed during the given period by the number of individuals at risk. The reemployment hazard declines from the beginning of a nonemployment spell and rises in the sixth month. It peaks at 1.3 percentage points in the seventh month and begins to decrease since then.

Figure E.7 plots the reemployment hazard over the nonemployment spell for cohorts entering UI at three different time periods. First, the blue line shows the hazard rate for cohort entering UI between January 1, 2001 and July 1, 2002. We call these workers "non-exposed" because they were not eligible for a bonus. The red line shows the hazard for the cohort entering UI between July 1, 2002 and December 31, 2002. These workers are partially exposed to the program because some of them were still receiving UI benefits in 2003. The green line plots the hazard for those starting their nonemployment spell after January 1, 2003. They were fully exposed to the bonus program.

Comparing the hazard rates for the fully exposed cohort with the non-exposed cohort, the difference between the hazard rates before exhaustion is a combination of the bonus and calendar time effects, while the difference in the hazard after exhaustion point is only the consequence of improving labor market conditions because workers were not be eligible for a bonus after exhausting benefits, whether workers had been exposed to the bonus program or not.

Because the cohort entering UI after January 2003 was eligible for a bonus, while the bonus after the exhaustion point is zero regardless of the date when workers entering UI, it is natural to use the non-exposed cohort as a control group and a difference-in-differences design to identify the effects of the reemployment bonus program. The partially exposed cohort is a mixture of the comparison and treatment groups. In the hazard model in the next section, we also include this partially exposed cohort in the estimation sample, because it will help identify the bonus impact.

3.6 Hazard Model

In this section, we introduce a discrete-time hazard model, which helps separately identify the calendar time effects, duration dependence, and the bonus effect. This hazard model is a discrete-time version of the hazard model in Van der Klaauw and Van Ours (2013).

Let D represents nonemployment duration. The hazard rate of reemployment, h_d, is defined as

$$h_d = P(D = d | D > d - 1)$$

Let $t = \tau_i + d$ be the calendar date at the end of the 30-day interval, where τ_i be the date when worker i enters UI. We specify the hazard rate in period d of the nonemployment spell for worker i as follows:

$$\begin{aligned} h_{id} &= \alpha_d + \lambda_t + \delta_k \cdot 1[t \geq Jan.1, 2003] \cdot \sum_{k=1}^{12} 1[d = k] \\ &+ \delta_k^{pre} \cdot 1[May\ 1, 2002 \leq t < Jan.1, 2003] \cdot \sum_{k=1}^{12} 1[d = k], \end{aligned} \tag{3.1}$$

where α_d is a set of indicators for a flexible baseline hazard function. We control for the calendar time effects nonparametrically by including a full set of calendar month dummies, λ_t. δ_k are the incentive effects, representing the effects of the bonus program on the reemployment hazard in month k following the start of unemployment. δ_k^{pre} captures the anticipatory effects, allowing the possibility that workers lower their job acceptance rate before the bonus program was adopted in order to be eligible for a bonus later when the program took effect. We estimate this linear probability model using ordinary least square and cluster the standard errors by nonemployment spells to account for serial correlation within a spell.

Since we assume the bonus program has no impact on the reemployment hazard before May 2002 and the reemployment hazard after the 360th day of an nonemployment spell, our empirical design is similar in the spirit of a difference-in-difference design. It implicitly makes two assumptions. First, the difference between the hazard rates after the 360th day of the initial claim before and after the adoption of the bonus program captures the calendar time effects. Second, it assumes the difference in the hazard rates before the 360th day after and prior to the adoption of the bonus program is a combination of the calendar time effects and a bonus effect.

There are two important concerns with the hazard specification. On one hand, the bonus program might induce a change in the composition of individuals in a particular period of a nonemployment spell. For example, if workers whose job finding rate is higher are more responsive to the bonus program, we will be more likely to observe these workers in the earlier periods of an unemployment spell and less likely later. The heterogeneous impact of the bonus program will lead us to overestimate the effect of the bonus program on the reemployment hazard. To mitigate this concern, we include covariates, including previous earnings and industry, age, gender, birth place, and number of UI spells prior to job loss into the model. The estimates are robust to the inclusion of these covariates.

On the other hand, the calendar time effects might vary over durations. In other words, the business cycle might have a differential impact on the hazard rate in different points in the nonemployment spell. We conduct a falsification exercise by altering the date the bonus program was adopted. If the estimated placebo effects are positive, it would suggest the estimates exaggerate the bonus impact on the timing of reemployment. The falsification test below offers evidence that the estimates are not confounded by the time-varying duration effects.

3.7 Estimation Results

In this section, we first estimate the effect of the reemployment bonus program on the timing of reemployment using the discrete-time hazard model. We then offer a falsification test to support our empirical design.

3.7.1 Effects on Reemployment Hazard

An important feature of the Taiwanese reemployment bonus program is its implicit declining bonus schedule. Since workers can receive a larger bonus earlier in a spell, the response to the bonus program should be larger earlier in the spell of unemployment. Column 2 of Table A.24 reports the estimates for equation 3.1. The estimates suggest the bonus program increases the hazard in the first and the second months of the spell by 6.9 and 6.6 percentage points. The estimated effect declines to 4.6, 3.4 and 2.1 percentage points in the following three months. It rises to 3.0 and 2.9 percentage points in the sixth and seventh month. It sharply declines to 0.9 percentage points in the eighth month and gradually dies out afterwards.

At first glance, the U-shaped response to the bonus program over the duration of nonemployment is puzzling because the bonus offer decreases over nonemployment. However, relative to the baseline hazard, the estimated effects based on Column 1 imply 68 (0.069/0.101) and 75 (0.066/0.088) percent increases in the first and second months, 69 and 58 percent increases in the third and the fourth months, and further declines to 43, 33, and 25 percent in the following three months. Therefore, the response to the bonus offer over the nonemployment spell is consistent with the declining bonus offer over the qualification period.

In addition, the small but significant association between the introduction of the bonus program and the hazard after six months of nonemployment suggests that workers did not claim monthly benefits continuously. Workers having interruptions in claim series were still eligible for bonuses after the six months of nonemployment as long as they had not exhausted their six months of benefits.

A pressing concern about the estimates in Columns 1 and 2 is whether they are driven by selection. If workers more able to find a job delayed filing for UI until the program took effect in order to be eligible for a bonus, the estimates will be biased up. In Figure E.8, we look at the number of new UI claimants over time. If workers initiated their claims until the program took effect, we should observe an extra mass of new UI claimants in a few months right after the adoption of the program. However, the number of new claimants appears to be smooth around

January 1, 2003, the date when the bonus program took effect. Therefore, Figure E.8 shows no evidence of workers' behaving strategically.

Although no evidence suggests workers delayed their initial claims, it is possible that workers might postpone making subsequent claims. This behavior will generate longer or more interruptions in the claim series and lower their reemployment rate before the program took effect. Column 1 of Table A.24 reports the anticipatory effects of the bonus program, δ_k^{pre}. If workers increase their selectivity for jobs prior to the reform to increases their chance to be eligible for the bonus, δ_k^{pre} will be negative. We find the estimated anticipatory effects are smaller in magnitude compared to the estimated incentive effects in Column 2, showing little evidence that workers decrease their job acceptance rate prior to the reform. Nevertheless, a few significant (but small) estimates in Column 1 perhaps suggest our model does not perfectly control for seasonal changes in the reemployment hazard.

3.7.2 A Falsification Test

We have shown there is significant positive association between the hazard rate in the first six months of nonemployment and the introduction of the bonus program conditional on duration dependence and calendar time effects. Further, this positive relationship is stronger earlier in the nonemployment spell. The positive association might not be the result of the bonus program if workers whose nonemployment duration are shorter are more sensitive to changing labor market conditions.

We check for this possibility by altering the date when the program was adopted. Specifically, we estimate the following model:

$$h_{id} = \alpha_d + \lambda_t + \delta_{1-6} \cdot 1[t \geq Y] \cdot 1[d \leq 6], \tag{3.3}$$

where we consider Y as January 1, 2001 and January 1, 2002, two years and 1 year before the adoption of the bonus program. Table A.25 estimates the model using all nonemployment spell starting between January 1, 2000 and January 1, 2003. Columns 1 and 2 assumes the program had been implemented on January 1, 2001, while Columns 3 and 4 assumes it has been adopted on

January 1, 2002. Compared to the estimates using the actual date when the program was adopted (3.7-4.2 percentage points), the estimates using mistimed bonus adoption dates are smaller and at best marginally significant, providing some assurance that the positive association between the reemployment hazard in the first six months and the adoption of the bonus program is not due to spurious factors.

The estimates in Table A.25 are consistent with Figure E.9, where we plot the hazard functions for three periods—January 1, 2000-July 1, 2001, July 1, 2001-December 31, 2001, and January 1, 2002-July 1, 2002. Note that workers initiating their claims during these three periods are not exposed to the bonus program. Since these hazard functions are closely matched to each other, it suggests the time-varying duration effects might not play an important role in my estimated effect of the bonus program.

3.8 Concluding Remarks

The reemployment bonus program in Taiwan has a unique structure—the bonus declines linearly over UI spell and reaches zero at the exhaustion point. While the labor supply effects of reemployment bonuses have been credibly evaluated using the random experiments in the U.S., most of the bonuses use fixed dollars amounts and a pre-specified qualification period. In this chapter, we estimate the labor supply effects of the time-varying reemployment bonus in Taiwan.

To identify the incentive effects of the bonus program, we exploit the variation in the bonus offer brought about by the introduction of the program. Since only incumbent claimants after the adoption of the program are eligible for bonuses if they were reemployed before exhaustion, there is a well-identified control group to identify the bonus effects. Using administrative UI claim and corresponding earning records, we estimate that the bonus program increases the hazard rate in the first four months of a nonemployment spell by more than 40 percent and by 20-30 percent in the following three months. Overall, consistent with the declining bonus offer schedule, the hazard response to the bonus program is smaller later in a nonemployment spell.

This chapter, however, has not answered whether the reduction of unemployment benefits payment is larger than the bonus payment. Such a study will rely on data on actual bonus receipt, which will allow us to calculate the cost-effectiveness of the bonus program for the UI system. From the perspective of the society, it is also crucial to estimate the effects of the bonus program on reemployment earnings. The random experiments in the U.S. show a shorter unemployment duration (caused by bonus offer) does not decrease reemployment earnings, but the estimates are generally imprecise. The national bonus program in Taiwan applying to all UI claimants and the administrative UI records covers the universe of the UI recipients. Future study on the effect of the bonus program on reemployment earnings may yield more precise estimates of the bonus impact on reemployment earnings for subgroups of workers over time.

APPENDICES

APPENDIX A

TABLES

Table A.1 Employment Rates of Teenagers and Adults

	Number of indexed states	Teenagers		Adult	
		Non-indexed	Indexed	Non-indexed	Indexed
1979	9	0.490	0.519	0.722	0.728
1986	12	0.443	0.488	0.745	0.763
1993	16	0.405	0.454	0.751	0.772
2000	15	0.449	0.479	0.784	0.796
2007	15	0.359	0.414	0.770	0.789
2014	17	0.275	0.330	0.737	0.755

Note: This table shows the teenage and adult employment rates for indexed and non-indexed states from 1979 to 2014. The sample is comprised of teenagers ages 16-19 and adults ages 20-60; data are from CPS MORG 1979-2014.

Table A.2 The UI Tax Base in 2014

State	Tax Base	State	Tax Base	State	Tax Base
AL	$8,000	AK	$35,800	AZ	$7,000
AR	$12,000	CO	$11,000	CT	$15,000
DE	$10,500	DC	$9,000	FL	$8,500
GA	$8,500	HI	$38,800	ID	$34,100
IL	$13,560	IN	$9,500	IA	$25,300
KS	$8,000	KY	$9,000	LA	$7,700
ME	$12,000	MD	$8,500	MA	$14,000
MI	$9,500	MN	$28,000	MS	$14,000
MO	$13,000	MT	$27,000	NE	$9,000
NV	$26,400	NH	$14,000	NJ	$30,300
NM	$22,400	NY	$8,500	NC	$20,400
ND	$27,900	OH	$9,000	OK	$19,100
OR	$33,000	PA	$8,000	RI	$19,600
SC	$12,000	SD	$12,000	TN	$9,000
TX	$9,000	UT	$29,500	VT	$16,000
VA	$8,000	VI	$23,700	WA	$38,200
WV	$12,000	WI	$13,000	WY	$23,000
CA	$7,000				

Note: Data are from ET Financial Data Handbook 394 Report.

Table A.3 Computations of the Taxable Wage Base for the Indexed States

State	% of Average Annual Wage	State	% of Average Annual Wage	Other
AK	75	NC	50	
HI	100	ND	70	
ID	100	OK	50	
IA	66	OR	80	
MN	60	RI	46.5	
MT	80	UT	75	
NV	66.66	VI	60	
NJ	50	WA	115% of previous year's base	
NM	60	WY	55	

Note: Data are from Comparison of State UI Laws.

Table A.4 Relationship between Average UI Tax Rates and the UI Tax Base

	$\log ATR^H$		$\log ATR^L$	
	(1)	(2)	(3)	(4)
ϕ_0	0.291***	0.645***	-0.418***	0.030
	(0.084)	(0.098)	(0.088)	(0.097)
Lags				
ϕ_1		0.169*		0.121
		(0.091)		(0.093)
ϕ_2		-0.165***		-0.181***
		(0.059)		(0.054)
ϕ_3		0.036		0.026
		(0.054)		(0.056)
ϕ_4		-0.140**		-0.145**
		(0.068)		(0.068)
ϕ_5		-0.034		-0.041
		(0.069)		(0.069)
ϕ_6		-0.248**		-0.282***
		(0.099)		(0.097)
$\sum_{k=0}^{6} \phi_k$		0.262**		-0.471**
		(0.077)		(0.085)
Observations	2244	1938	2244	1938

Note: This table explores the association between average UI tax rates and the UI tax base using data from all 50 U.S. states and Washington, D.C., dating from 1970 to 2013. Column 1 estimates the correlation between the log of the ratio of UI taxes to total payroll and the UI tax base, conditional on state and year fixed effects. Column 2 adds six lags of the UI tax base. The estimates in columns 3 and 4 are constructed in the same way, using the log of the ratio of UI taxes to taxable payroll as the dependent variable. Data are from ET Financial Data Handbook 394 Report. Data are from ET Financial Data Handbook 394 Report. Standard errors are clustered on the state level. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table A.5 Relationship Between Average UI Tax Rates and the UI Tax Base for Reserve-Ratio and Benefit-Ratio States

	(1)	(2)	(3)	(4)
	RR		*BR*	
	$\log ATR^H$	$\log ATR^L$	$\log ATR^H$	$\log ATR^L$
ϕ_0	0.642***	0.049	0.762***	0.093
	(0.114)	(0.112)	(0.198)	(0.234)
Lags				
ϕ_1	0.256***	0.206**	-0.146	-0.188**
	(0.085)	(0.090)	(0.086)	(0.083)
ϕ_2	-0.175**	-0.192***	-0.112	-0.118
	(0.072)	(0.065)	(0.149)	(0.147)
ϕ_3	0.006	-0.013	0.227	0.250
	(0.059)	(0.061)	(0.164)	(0.175)
ϕ_4	-0.179**	-0.187**	-0.073	-0.070
	(0.073)	(0.072)	(0.138)	(0.137)
ϕ_5	-0.043	-0.048	-0.053	-0.074
	(0.089)	(0.090)	(0.091)	(0.187)
ϕ_6	-0.220*	-0.252**	-0.391***	-0.431**
	(0.115)	(0.115)	(0.185)	(0.183)
$\sum_{k=0}^{6} \phi_k$	0.288**	-0.436***	0.215**	-0.538**
	(0.112)	(0.125)	(0.097)	(0.110)
Observations	1254	1254	684	684

Note: This table explores the association between average UI tax rates and the UI tax base in states using reserve-ratio and benefit-ratio systems, respectively. I use a sample comprised of data from all 50 U.S. states and Washington, D.C., representing the years from 1970 to 2013. Column 1 regresses the log of the ratio of UI taxes to total payroll to the present UI tax base and six lags, conditional on state and year fixed effects. Columns 3 and 4 are constructed in the same way, using the log of the ratio of UI taxes to taxable payroll as the dependent variable. Data are from ET Financial Data Handbook 394 Report. Standard errors are clustered on the state level. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table A.6 UI Tax Base and Annual Wages

	(1)	(2)	(3)	(4)
	Average UI Tax Base		Average Annual Wage	
	Indexed	Non-indexed	Teenagers	Adult
1979	7944	6333	5210	13345
1986	12125	7767	5615	19258
1993	15506	8325	6866	25622
2000	18966	8980	9623	33585
2007	24153	9569	11119	41472
2014	29764	10867	11944	46671

Note: This table shows the average UI tax bases for indexed and non-indexed states from 1979 to 2014, and the corresponding employment rates for teenagers and adults. I use data from teenagers ages 16-19 and adults ages 20-60 who reported positive weekly earnings in CPS MORG. I calculate annual earnings as 52 times weekly earnings.

Table A.7 Composition of Indexed States since 1979

1979	1980	1981	1982	1984	1985	1986
NJ	NJ	NJ	NJ	NJ	NJ	NJ
ID	ID	ID	ID	ID	ID	ID
NV	NV	NV	NV	NV	NV	NV
HI	HI	HI	HI	HI	HI	HI
WA	WA	WA	WA	WA	WA	WA
IA	IA	IA	IA	IA	IA	IA
ND	ND	ND	ND	ND	ND	ND
NM	NM	NM	NM	NM	NM	NM
MT	MT	MT	MT	MT	MT	MT
	RI	RI	RI	RI	RI	RI
		AK	AK	AK	AK	AK
			MN	MN	MN	MN
				NC	NC	NC
				WY	WY	WY
					UT	UT
						OK

Note: This table shows the composition of group of indexed states since 1979. In 1979, there were nine indexed states; seven states have moved to indexation since then: Rhode Island in 1980, Arkansas in 1981, Minnesota in 1982, North Carolina and Wyoming in 1984, Utah in 1985, and Oklahoma in 1986. There have been no other changes in this group since 1986.

Table A.8 The Effect of Indexing the Tax Base on Teenage Employment Rate

	(1)	(2)	(3)	(4)	(5)
β^{index}	0.009	0.016*	0.024**	0.024**	0.021**
	(0.014)	(0.009)	(0.010)	(0.010)	(0.009)
Census division x year dummies		$\surd$	$\surd$	$\surd$	$\surd$
State linear time trends			$\surd$	$\surd$	$\surd$
State minimum wage				$\surd$	$\surd$
Demographic controls					$\surd$
Observations	752,801	752,801	752,801	752,801	752,801

Note: This table shows the estimated employment effects of indexing the tax base. I estimate Equation 1.5 above using data on individuals aged 16-19 from CPS MORG 1979-2014, excluding observations from states that indexed their tax bases before 1979. Each specification controls for the log of state total employment and the log of state population. Demographic controls include age, race, gender, marital status and month of interview. Standard errors are clustered at state level. ** indicates significance at the 5 percent level and * significant at the 10 percent level.

Table A.9 The Effect of Indexing the Tax Base on Adult Employment Rate

	(1)	(2)	(3)	(4)
β^{index}	-0.000	0.002	0.003	0.002
	(0.007)	(0.004)	(0.005)	(0.005)
Census division x year dummies		$\surd$	$\surd$	$\surd$
State linear time trends			$\surd$	$\surd$
Demographic controls				$\surd$
Observations	7,037,778	7,037,778	7,037,778	7,037,778

Note: This table shows the estimated employment effects of indexing the tax base on teenage employment rate. I estimate Equation 1.5 using data on individuals aged 20-60 from CPS MORG 1979-2014, excluding observations from states that indexed their tax bases before 1979. Each specification controls for the log of state total employment and the log of state population. Demographic controls include age, race, gender, marital status and month of interview. Standard errors are clustered at state level. ** indicates significance at the 5 percent level and * significant at the 10 percent level.

Table A.10 The Dynamic Effects of Indexing the Tax Base on Teenage Employment Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β_0^{index}	-0.008	0.011	0.012	0.014	0.010	0.012	0.013
	(0.017)	(0.014)	(0.009)	(0.017)	(0.015)	(0.015)	(0.016)
Leads							
β_1^{index}	-0.023	-0.010	-0.007	-0.006	-0.008	-0.007	-0.007
	(0.020)	(0.014)	(0.016)	(0.016)	(0.013)	(0.013)	(0.013)
β_2^{index}	-0.016	-0.010	-0.008	-0.008	-0.005	-0.004	-0.004
	(0.010)	(0.010)	(0.010)	(0.010)	(0.008)	(0.009)	(0.009)
Lags							
β_{-1}^{index}	-0.004	0.016	0.020	0.020	0.018	0.020	0.021
	(0.023)	(0.013)	(0.017)	(0.017)	(0.014)	(0.014)	(0.015)
β_{-2}^{index}	-0.020	0.005	0.012	0.012	0.009	0.011	0.012
	(0.020)	(0.013)	(0.013)	(0.013)	(0.012)	(0.011)	(0.012)
β_{-3}^{index}	-0.019	-0.003	0.007	0.007	0.006	0.008	0.009
	(0.029)	(0.016)	(0.020)	(0.020)	(0.017)	(0.018)	(0.018)
β_{-4}^{index}	-0.013	0.010	0.024	0.024	0.023	0.025	0.026
	(0.021)	(0.013)	(0.018)	(0.018)	(0.015)	(0.016)	(0.016)
β_{-5}^{index}	0.004	0.018	0.035	0.035	0.030	0.032	0.033
	(0.026)	(0.016)	(0.023)	(0.023)	(0.022)	(0.022)	(0.022)
β_{-6}^{index}	0.002	0.015	0.048**	0.047**	0.038**	0.021	0.022
	(0.018)	(0.012)	(0.018)	(0.018)	(0.017)	(0.020)	(0.020)
β_{-7}^{index}						0.047**	0.039**
						(0.018)	(0.019)
β_{-8}^{index}							0.051***
							(0.018)
Census division x year dummies		√	√	√	√	√	√
State linear time trends			√	√	√	√	√
State minimum wage				√	√	√	√
Demographic controls					√	√	√
Observations	752,801	752,801	752,801	752,801	752,801	752,801	752,801

Note: This table estimates the employment effects of indexing the tax base on teenage employment rate before, during, and after moving to indexation. I estimate Equation 1.7 using data on teenagers aged 16-19 from CPS MORG 1979-2014, excluding observations from states that indexed their tax bases before 1979. Each specification controls for the log of state total employment and the log of state population. Demographic controls include age, race, gender, marital status and month of interview. Standard errors are clustered at state level. ** indicates significance at the 5 percent level and * significant at the 10 percent level.

Table A.11 The Effect of Increasing the Tax Base in Non-Indexed States on Teenage Employment Rate

	(1)	(2)	(3)	(4)	(5)
β^{non}	0.027*	0.028*	0.038*	0.040*	0.042*
	(0.016)	(0.016)	(0.022)	(0.022)	(0.022)
Census division x year dummies		√	√	√	√
State linear time trends			√	√	√
State minimum wage				√	√
Demographic controls					√
Observations	250,675	250,675	250,675	250,675	250,675

Note: This table shows the estimated employment effects of indexing the tax base. I estimate Equation 1.6 using data on teenagers aged 16-19 from CPS MORG 2000-2014, excluding observations from indexed states in this period. Each specification controls for the log of state total employment and the log of state population. Demographic controls include age, race, gender, marital status and month of interview. Standard errors are clustered at state level. ** indicates significance at the 5 percent level and * at the 10 percent level.

Table A.12 The Effect of Increasing the Tax Base in Non-Indexed States on Adult Employment Rate

	(1)	(2)	(3)	(4)
β^{non}	0.007	0.002	0.001	0.002
	(0.009)	(0.007)	(0.005)	(0.005)
Census division x year dummies		$\surd$	$\surd$	$\surd$
State linear time trends			$\surd$	$\surd$
Demographic controls				$\surd$
Observations	2,536,243	2,536,243	2,536,243	2,536,243

Note: This table shows the estimated employment effects of indexing the tax base. I estimate Equation 1.6 using data on adults aged 20-60 from CPS MORG 2000-2014, excluding observations from indexed states in this period. Each specification controls for the log of state total employment and the log of state population. Demographic controls include age, race, gender, marital status and month of interview. Standard errors are clustered at state level. ** indicates significance at the 5 percent level and * at the 10 percent level.

Table A.13 The Dynamic Effects of Increasing the Tax Base in Non-Indexed States on Teenage Employment Rate

	(1)	(2)	(3)	(4)	(5)
β_0^{non}	0.031*	0.030*	0.041**	0.041**	0.048**
	(0.018)	(0.017)	(0.019)	(0.019)	(0.019)
Leads					
β_1^{non}	-0.000	0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
β_2^{non}	0.000	-0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.003)	(0.003)
Lags					
β_{-1}^{non}	0.008	0.009	-0.001	-0.001	-0.010
	(0.034)	(0.033)	(0.031)	(0.032)	(0.030)
β_{-2}^{non}	-0.033	-0.019	-0.017	-0.015	-0.002
	(0.022)	(0.022)	(0.025)	(0.025)	(0.022)
β_{-3}^{non}	0.046	0.007	-0.009	-0.009	-0.007
	(0.034)	(0.030)	(0.031)	(0.031)	(0.028)
β_{-4}^{non}	0.007	0.018	0.019	0.019	0.009
	(0.017)	(0.021)	(0.018)	(0.019)	(0.017)
β_{-5}^{non}	-0.006	-0.003	-0.004	-0.004	0.001
	(0.019)	(0.016)	(0.017)	(0.017)	(0.014)
β_{-6}^{non}	0.003	-0.002	-0.009	-0.010	-0.016
	(0.023)	(0.021)	(0.019)	(0.019)	(0.018)
β_{-7}^{non}	-0.006	-0.002	-0.009	-0.010	-0.016
	(0.023)	(0.021)	(0.019)	(0.019)	(0.018)
β_{-8}^{non}	0.003	-0.002	-0.009	-0.010	-0.016
	(0.023)	(0.021)	(0.019)	(0.019)	(0.018)
Census division x year dummies		√	√	√	√
State linear time trends			√	√	√
State minimum wage				√	√
Demographic controls					√
Observations	250,675	250,675	250,675	250,675	250,675

Note: This table shows the estimated employment effects of indexing the tax base. I estimate Equation 1.6 using data on teenagers aged 16-19 from CPS MORG 2000-2014, excluding observations from indexed states in this period. Each specification controls for the log of state total employment and the log of state population. Demographic controls include age, race, gender, marital status and month of interview. Standard errors are clustered at state level. ** indicates significance at the 5 percent level and * at the 10 percent level.

Table A.14 Descriptive Statistics

	25-65 (1)	43-46 (2)	43-44 (3)	45-46 (4)
age (years)	37.79	45.00	43.99	45.97
female	0.52	0.49	0.50	0.48
number of dependants	0.67	1.14	1.17	1.10
previous earnings (NTD)	29,810	30,853	30,757	30,947
worked in manufacturing	0.30	0.33	0.31	0.34
participate in training	0.07	0.08	0.08	0.08
duration of training (days)	6.45	7.94	7.98	7.91
insured duration (days)	145.74	175.04	144.22	204.90
nonemployment duration (days)	255.95	294.97	272.41	316.83
right censored at 730 days	0.12	0.15	0.13	0.15
exhaustion rate	0.53	0.65	0.61	0.69
recall rate	0.13	0.12	0.12	0.13
reemployment earnings (NTD)	25,554	25,907	25,703	26,104
observations	187,450	20,893	10,283	10,610

Note: This table shows the means of our main variables from the extended benefits sample. The sample in column 1 consists of all UI recipients starting UI spells between May, 1, 2009 and Jan. 1, 2012. Columns 2-6 report the results for UI recipients in the same sample period for workers from three different age groups. Nonemployment duration is censored at 730 days. We define exhaustion rate as the ratio of workers whose insured duration is 180 days or longer.

Table A.15 Estimates of Smoothness of Predetermined Covariates

	(1) Delay Days	(2) Female	(3) Manu. Sector	(4) # of Dependents	(5) Log Previous Earnings	(6) Predicted Nonemp. Dur.
β_{EB}	-0.70	-0.00	0.018*	0.01	0.013*	1.34
	(2.06)	(0.10)	(0.010)	(0.01)	(0.007)	(1.31)
Sample size	46,916	43,035	42,036	37,961	50,903	50,706
Poly. model	linear	linear	linear	linear	linear	linear
Bandwidth (days)	CCT	CCT	CCT	CCT	CCT	CCT

Note: This table checks for smoothness of mean predetermined variables by estimating a local linear regression using the optimal bandwidth by Calonico et al. (2014) and a triangular kernel. The sample are workers aged within the bandwidth and starting UI spells between May 1, 2009 and January 1, 2012. The predictors for nonemployment duration are previous wage, squared previous wage, previous industry, gender, place of birth, number of dependants, month/year at job loss and the number of days between job loss and initial claim. Standard errors in parentheses are clustered by age in days. *** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

Table A.16 The Effect of Extended Benefits on Insured Duration and Nonemployment Duration

	(1)	(2)	(3)	(4)	(5)	(6)
Insured Duration						
β_{EB}	57.96***	58.29***	56.55***	56.12***	57.09***	57.23***
	(1.97)	(1.95)	(1.50)	(1.74)	(2.25)	(2.25)
Baseline mean			147.32			
Sample size	20,906	20,893	40,507	40,507	37,785	50,680
Nonemployment Duration						
β_{EB}	41.14***	43.02***	36.23***	37.76***	40.41***	41.86***
	(6.90)	(6.90)	(5.18)	(6.01)	(7.96)	(7.95)
Baseline mean			276.39			
Sample size	20,906	20,893	40,987	40,987	36,589	48,906
Nonemployment Duration $<$ *180 days*						
β_{EB}	-0.06***	-0.06***	-0.06***	-0.06***	-0.06***	-0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Baseline mean			0.46			
Sample size	20,906	20,893	31,887	31,887	34,770	34,382
Bias-corrected	–	–	–	Yes	Yes	Yes
Covariates	–	Yes	–	–	–	Yes
Poly. model	linear	linear	linear	linear	quadratic	quadratic
Bandwidth (days)	730	730	CCT	CCT	CCT	CCT

Note: This table shows the estimates of the effect of increasing potential duration from 6 to 9 months on insured duration, nonemployment duration and the probability that nonemployment duration is less than 180 days. Column 1 estimates a linear regression on either side of the cutoff using sample from workers aged 43-46 at job loss for UI spells between May 1, 2009 and Jan. 1, 2012. Column 2 includes the following covariates: previous wage, squared previous wage, previous industry, gender, place of birth, number of dependants, month/year at job loss and number of UI spells prior to job loss. Columns 3 reports the estimates using optimal bandwidth algorithm from Calonico et al. (2014). The optimal bandwidths vary with the outcome variables, in the range of 3 to 6 years. The bias correction estimates and the corresponding robust standard errors are presented in the column 4. In column 5, we report the bias correction estimates and robust standard error using a local quadratic regression. Column 6 reports the bias correction estimates and robust standard error using a local quadratic regression with covariates (Calonico et al. (2016)). Standard errors in parentheses are all clustered by age in days. Columns 1 and 2 use a rectangular kernel. Columns 3-6 use a triangular kernel. *** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

Table A.17 The Effect of Extended Benefits on the Probability of Being Reemployed in k Days

	(1) $k=90$	(2) $k=180$	(3) $k=270$	(4) $k=360$	(5) $k=720$
β_{EB}	-0.013	-0.059***	-0.139***	-0.062***	-0.030***
	(0.010)	(0.010)	(0.011)	(0.008)	(0.008)
Baseline mean	0.309	0.478	0.652	0.724	0.824
Sample size	38,376	36,463	30,801	46,588	50,551
Poly. model	linear	linear	linear	linear	linear
Bandwidth (days)	CCT	CCT	CCT	CCT	CCT

Note: This table shows the estimates of the effect of increasing potential duration from 6 to 9 months on the probability of finding a job in k days. We estimate equation 2.3 using the optimal bandwidth by Calonico et al. (2014) and a triangular kernel. The sample are workers aged within the bandwidth, and starting UI spells between May 1, 2009 and January 1, 2012. Standard errors in parentheses are clustered by age in days. *** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

Table A.18 The Effect of Extended Benefits on Reemployment Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
Log Earnings with 1st Post-Claim Employer						
β_{EB}	0.014	0.006	0.013*	0.012	0.011	0.007
	(0.011)	(0.010)	(0.007)	(0.009)	(0.012)	(0.012)
Baseline mean			10.03			
Sample size	17,845	17,835	49,721	49,721	51,361	40,857
Panel B:						
Log Earnings 1 Year After Initial Claim						
β_{EB}	0.003	-0.006	0.004	0.003	0.003	-0.005
	(0.011)	(0.010)	(0.009)	(0.011)	(0.012)	(0.012)
Baseline mean			10.08			
Sample size	17,451	17,442	34,755	34,755	50,838	42,930
Panel C:						
Log Earnings 2 Years After Initial Claim						
β_{EB}	-0.003	-0.012	-0.002	-0.005	-0.008	-0.018
	(0.010)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)
Baseline mean			10.12			
Sample size	20,458	20,448	31,508	31,508	52,757	44,870
Bias-corrected	–	–	–	Yes	Yes	Yes
Covariates	–	Yes	–	–	–	Yes
Poly. model	linear	linear	linear	linear	quadratic	quadratic
Bandwidth (days)	730	730	CCT	CCT	CCT	CCT

Note: This table shows the estimates of the effect of increasing potential duration from 6 to 9 months on the reemployment earnings. Column 1 estimates a linear regression on either side of the cutoff using sample from workers aged 43-46 at job loss for UI spells starting between May 1, 2009 and January 1, 2012. Column 2 includes the following covariates: previous earnings, squared previous earnings, previous industry, gender, place of birth, number of dependants, month/year at job loss and number of UI spells prior to job loss. Columns 3 reports the estimates using optimal bandwidth algorithm from Calonico et al. (2014). The optimal bandwidths vary with the outcome variables, in the range of 3 to 6 years. The bias correction estimates and the corresponding robust standard errors are presented in column 4. In column 5, we report the bias correction estimates and robust standard error using a local quadratic regression. Standard errors in parentheses are all clustered by age in days. Column 6 reports the bias correction estimates and robust standard error using a local quadratic regression with covariates (Calonico et al. (2016)). Columns 1 and 2 use a rectangular kernel. Columns 3-6 use a triangular kernel. *** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

Table A.19 Smoothness of Predetermined Covariates Conditional on Employment

	(1) Delay Days	(2) Female	(3) Manu. Sector	(4) # of Dependents	(5) Log Previous Earnings	(6) Predicted Log Reemp. Earnings
Panel A:						
emp. before Dec. 2013						
β_{EB}	-2.18	-0.004	0.019*	0.034	0.013	0.004
	(1.76)	(0.011)	(0.010)	(0.020)	(0.009)	(0.005)
Sample size	43,861	40,891	40,377	31,331	35,164	36,585
Panel B:						
emp. 1 year after initial claim						
β_{EB}	0.125	-0.011	0.024**	0.031	0.013	0.006
	(1.71)	(0.011)	(0.011)	(0.020)	(0.009)	(0.005)
Sample size	43,861	42,536	36,769	29,176	32,333	34,345
Panel C:						
emp. 2 years after initial claim						
β_{EB}	1.18	-0.008	0.022**	0.032*	0.011	0.005
	(1.51)	(0.010)	(0.010)	(0.019)	(0.008)	(0.004)
Sample size	62,150	40,891	45,128	33,960	36,558	38,318
Poly. model	linear	linear	linear	linear	linear	linear
Bandwidth (days)	CCT	CCT	CCT	CCT	CCT	CCT

Note: This table checks for smoothness of mean predetermined variables conditional on employment by estimating a local linear regression using the optimal bandwidth by Calonico et al. (2014) and triangular kernel. The estimates in the first row use sample conditional on reemployment before December 2013. The sample used in the second and the third rows are conditional on one year and two years after initial claim. The predictors for reemployment earnings are previous earnings, squared previous earnings, previous industry, gender, place of birth, number of dependants, month/year at job loss and the number of days between job loss and initial claim. Standard errors in parentheses are clustered by age in days. *** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

Table A.20 The Effect of Extended Benefits on Unemployment Duration and Reemployment Earnings: By Quartiles of Predicted Probaility of Exhausting Benefits

	(1) 1st	(2) 2nd	(3) 3rd	(4) 4th
Insured Duration				
β_{EB}	41.61***	52.79***	60.45***	64.04***
	(3.85)	(2.97)	(2.96)	(2.44)
Baseline mean	139.94	140.93	147.28	156.93
Sample size	7,777	12,512	11,049	14,757
Nonemployment Duration				
β_{EB}	28.56**	24.94***	33.54***	50.81***
	(11.04)	(9.22)	(9.88)	(10.54)
Baseline mean	211.63	239.33	276.43	346.25
Sample size	7,773	11,412	12,120	13,155
Log Earnings with 1st Post-Claim Employer				
β_{EB}	0.001	0.019	0.002	0.028*
	(0.020)	(0.015)	(0.017)	(0.016)
Baseline mean	10.07	10.06	10.01	10.00
Sample size	8,395	12,952	10,913	11,887
Poly. model	linear	linear	linear	linear
Bandwidth (days)	CCT	CCT	CCT	CCT

Note: This table shows the estimates of the effect of increasing potential duration from 6 to 9 months on insured duration, nonemployment duration, and the difference in log monthly earnings between post- and pre-unemployment for four different groups divided by quartiles of predicted probability of exhausting benefits. The predictors are previous earnings, square of previous earnings, previous industry, gender, birthplace, number of UI spells prior to job loss, month when unemployed, number of days between job loss and initial claim, and whether a worker was recalled to work by a previous employer. We estimate a local linear regression using the optimal bandwidth by Calonico et al. (2014) and a triangular kernel. The sample are workers aged within the optimal bandwidth, and starting UI spells between May 1, 2009 and January 1, 2012. Standard errors in parentheses are clustered by age in days. *** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

Table A.21 The Effect of Extended Benefits on Durations and Reemployment Earnings: By Quartiles of Predicted Probaility of Exhausting Benefits and Previous Earnings

	(1)	(2)	(3)	(4)
	4th Quartile of Exhaustion Rate		1-3 Quartiles of Exhaustion Rate	
	Below 30,300	Above 30,300	Below 30,300	Above 30,300
Insured Duration				
β_{EB}	61.70***	66.24***	53.11***	52.23**
	(3.34)	(2.77)	(2.68)	(2.77)
Baseline mean	155.02	158.33	147.24	139.32
Sample size	8,039	9,545	14,383	13,995
Nonemployment Duration				
β_{EB}	46.54***	44.41***	19.76**	37.14***
	(14.35)	(13.24)	(8.26)	(8.69)
Baseline mean	339.90	350.74	256.75	237.83
Sample size	6,841	8,478	16,091	14,469
Log Earnings with 1st Employer				
β_{EB}	0.035**	0.023	-0.001	-0.003
	(0.017)	(0.024)	(0.011)	(0.016)
Baseline mean	9.84	10.14	9.86	10.22
Sample size	3,580	7,054	10,991	14,488
Poly. model	linear	linear	linear	linear
Bandwidth (days)	CCT	CCT	CCT	CCT

Note: This table shows the estimates of the effect of increasing potential duration from 6 to 9 months on insured duration, nonemployment duration, and reemployment earnings by quartiles of predicted probability of exhausting benefits and average previous wage. We estimate a local linear regression using the optimal bandwidth by Calonico et al. (2014) and a triangular kernel. The sample are workers aged within the bandwidth, and starting UI spells between May 1, 2009 and January 1, 2012. Standard errors in parentheses are clustered by age in days. *** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

Table A.22 Summary Statistics

	01/2001-06/2002	07/2002-12/2002	01/2003-12/2004
	(1)	(2)	(3)
age (years)	36.47	36.56	37.68
female	0.56	0.57	0.57
previous earnings (NTD)	26,565	26,901	27,906
insured duration	155.04	146.19	134.67
nonemployment duration	369.83	335.49	298.74
right censored	0.10	0.10	0.10
exhaustion rate	0.74	0.64	0.56
reemployment earnings (NTD)	22,739	23,089	23,473
Number of Spells	114,126	29,790	84,179

Note: This table shows the summary statistics of the estimation sample. The sample in Column (1) consists of all UI recipients starting UI spells between Jan, 1, 2001 and Jun. 30, 2002. Column (2) uses all UI recipients starting UI spells between Jul, 1, 2002 and Dec. 31, 2002. Column (3) contains those entering UI between Jan. 1, 2003 and Dec. 31, 2004.

Table A.23 Discrete Reemployment Hazard in the First 720 Days of Nonemployment

Days since Initial Claim	Risk Set (Number of Nonemployed) (1)	Reemployment Hazard (2)
0-30	228,095	0.090
30-60	207,463	0.083
60-90	190,247	0.068
90-120	177,226	0.063
120-150	166,132	0.057
150-180	156,637	0.094
180-210	141,977	0.130
210-240	123,582	0.097
240-270	111,557	0.077
270-300	102,968	0.063
300-330	96,432	0.057
330-360	90,943	0.051
360-390	86,257	0.046
390-420	82,323	0.041
420-450	78,935	0.038
450-480	75,954	0.035
480-510	73,321	0.032
510-540	70,982	0.031
540-570	68,760	0.029
570-600	66,795	0.028
600-630	64,945	0.025
630-660	63,289	0.024
660-690	61,763	0.024
690-720	60,255	0.024

Note: The risk set is the number of individuals nonemployed in the beginning of the 30-day period. The reemployment hazard is the proportion of individuals reemployed within the 30-day period.

Table A.24 Effects of Bonus Program on Reemployment Hazard over Nonemployment Spell

	Baseline Mean	(1) Pre-bonus period	(2) Bonus period
δ_1	0.101	0.012***	0.069***
		(0.003)	(0.002)
δ_2	0.088	0.003	0.066***
		(0.003)	(0.002)
δ_3	0.066	0.002	0.046***
		(0.003)	(0.002)
δ_4	0.058	0.007**	0.034***
		(0.003)	(0.003)
δ_5	0.049	0.001	0.021***
		(0.003)	(0.003)
δ_6	0.091	0.004	0.030***
		(0.003)	(0.003)
δ_7	0.114	-0.002	0.029***
		(0.003)	(0.003)
δ_8	0.087	-0.005	0.009***
		(0.004)	(0.003)
δ_9	0.070	-0.012***	-0.002
		(0.004)	(0.004)
δ_{10}	0.057	-0.008**	-0.000
		(0.004)	(0.003)
δ_{11}	0.053	-0.003	-0.002
		(0.004)	(0.003)
δ_{12}	0.048	-0.006*	-0.002
		(0.004)	(0.003)

Note: This table shows the estimated effects of the bonus program on the reemployment hazard in the kth month of a nonemployment spell. δ_k in column 1 denote the the anticipatory effects of the bonus program, while column 2 shows the incentive effects of the bonus program. The model includes the following covariates: previous wage, squared previous wage, previous industry, gender, place of birth, and number of UI spells prior to job loss. The sample comprises all nonemployment spells starting between January 1, 2001 and December 31, 2004.

*** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

Table A.25 A Falsification Test

	(1)	(2)	(3)	(4)
δ_{1-6}	0.008**	0.005	-0.001	0.002*
	(0.004)	(0.004)	(0.001)	(0.001)
Covariates	–	Yes	–	Yes

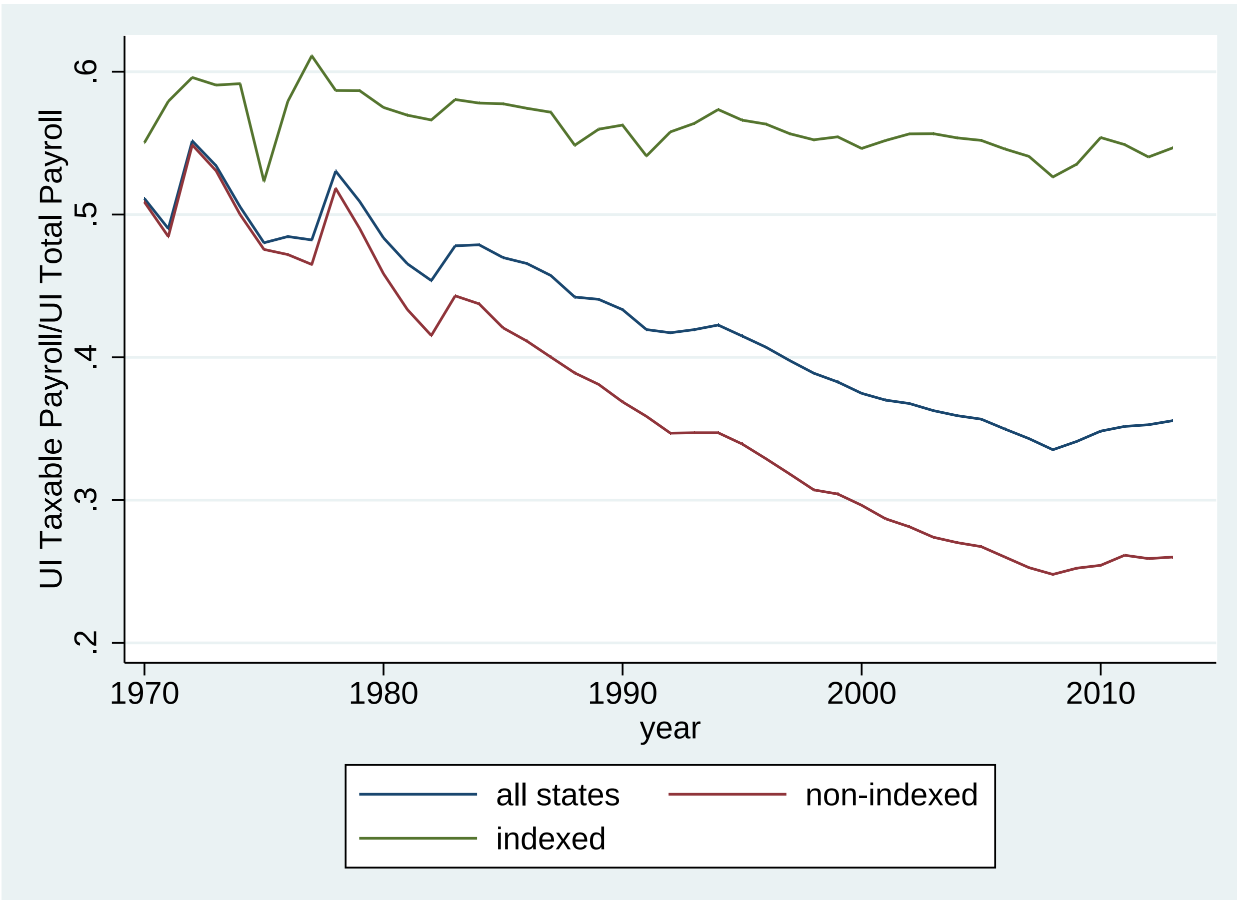
Note: This table conducts a falsification test by altering the date when the bonus program was implemented. We estimate equation 3.3 assuming the program took effect on January 1 2001 in Columns 1 and 2, January 1, 2002 in Columns 3 and 4. The sample comprises 100,052 nonemployment spells starting between January 1, 2000 and January 1, 2003. Columns 2 and 4 include the following covariates: previous wage, squared previous wage, previous industry, gender, place of birth, and number of UI spells prior to job loss.

*** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

APPENDIX B

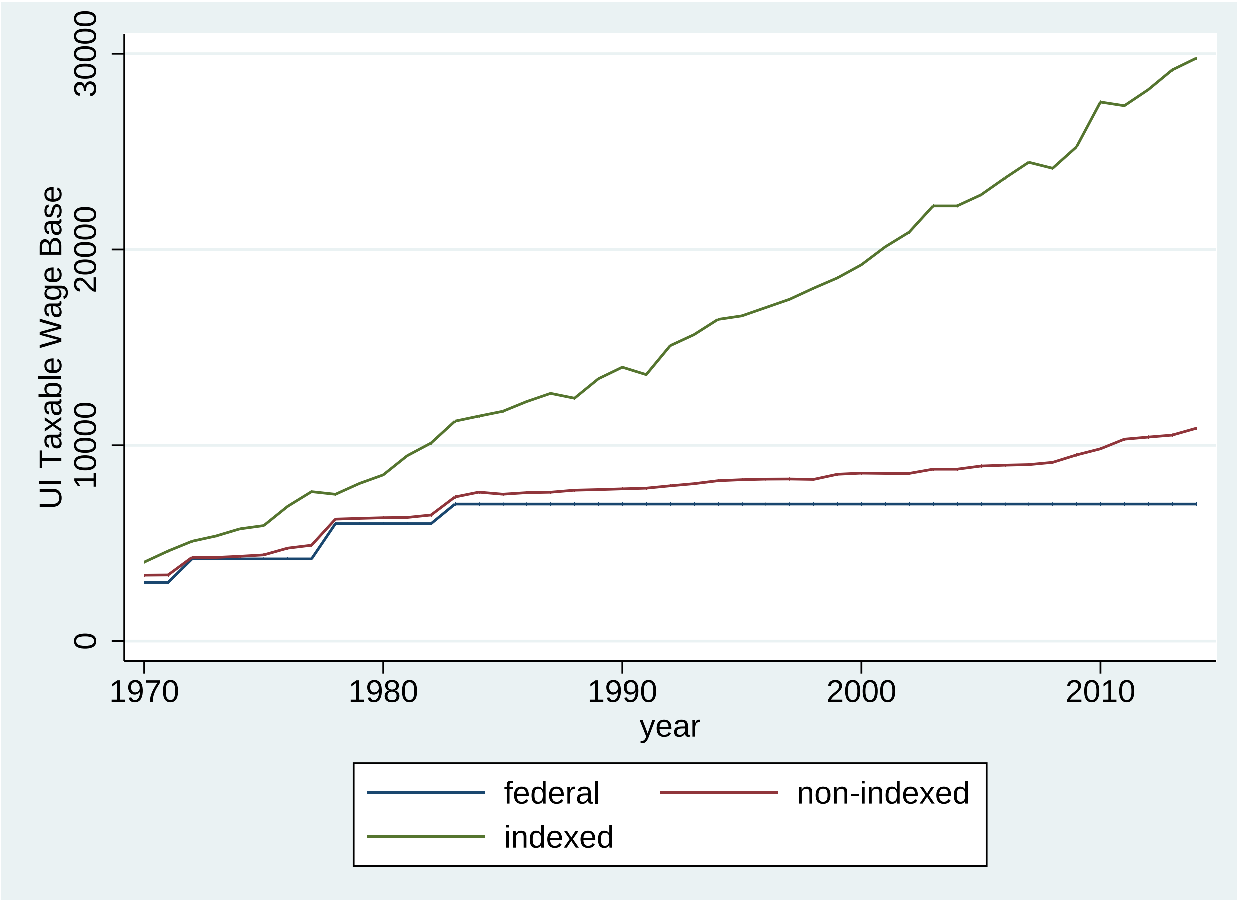
FIGURES

Figure B.1 Ratio of UI Taxable Payroll to Total Payroll



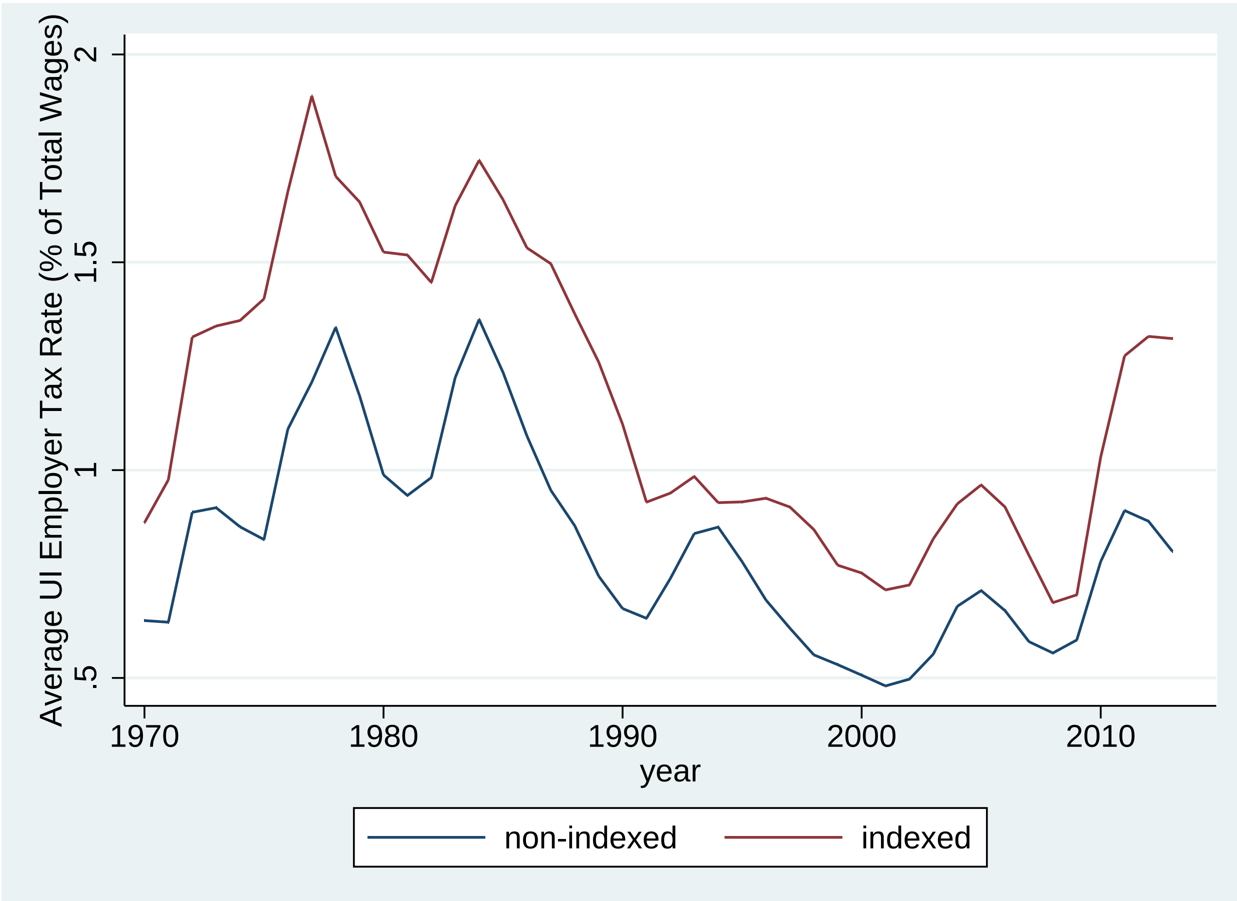
Notes: This figure presents the ratio of UI taxable payroll to total payroll from 1979 to 2013. The green line shows the average ratio for indexed states. The blue line indicates the average ratio for all 50 U.S. states and the District of Columbia. The red line plots the average ratio for non-indexed states. Data are from ET Financial Data Handbook 394 Report.

Figure B.2 UI Taxable Wage Base



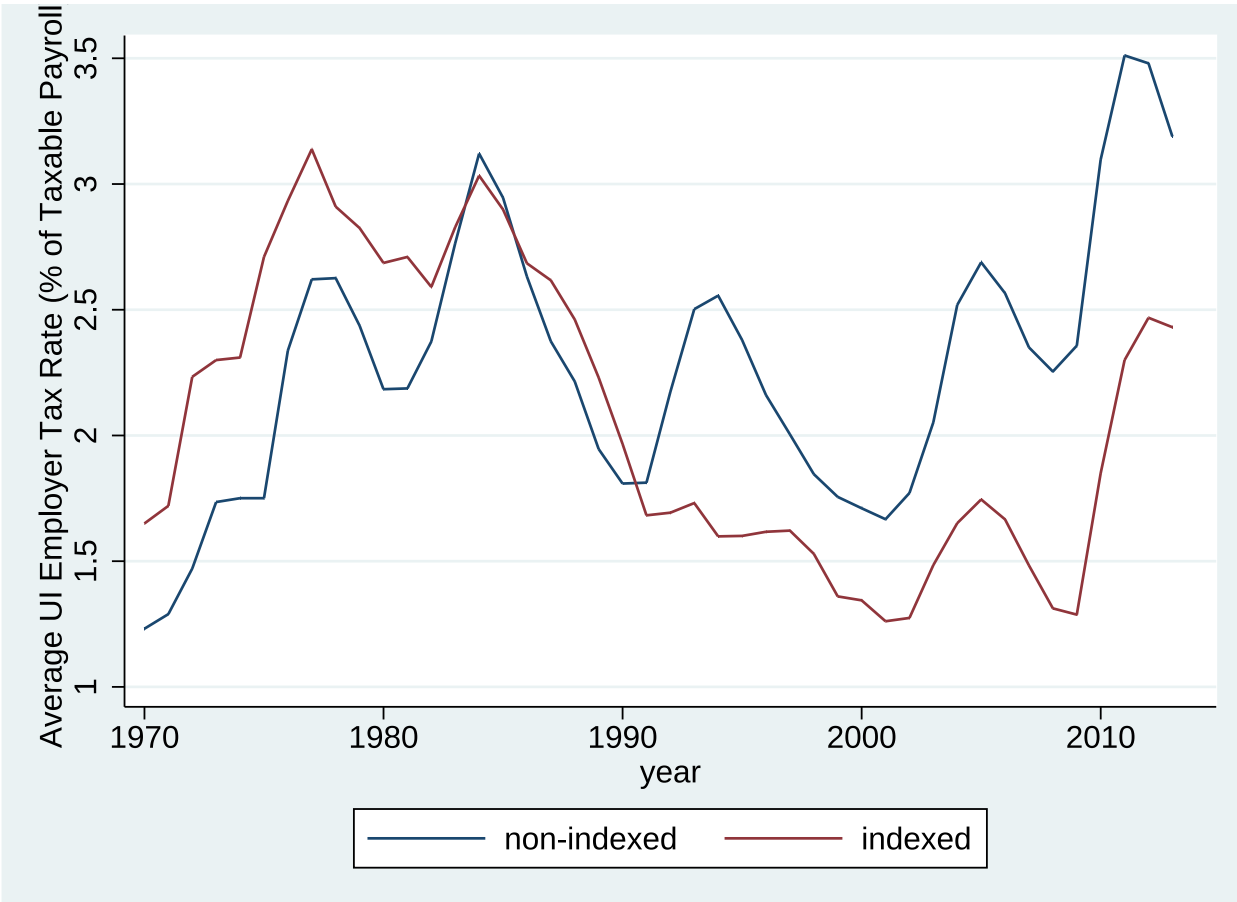
Notes: This figure presents the UI tax base from 1979 to 2013. The green line shows the average tax base for indexed states. The blue line indicates the federal tax base. The red line plots the average tax base for non-indexed states. Data are from ET Financial Data Handbook 394 Report.

Figure B.3 Average UI Employer Tax Rate (% of Total Wages)



Notes: This figure presents the ratio of UI taxes to total payroll from 1979 to 2013. The red (top) line shows the average ratio for indexed states. The blue (bottom) line indicates the average ratio for non-indexed states. Data are from ET Financial Data Handbook 394 Report.

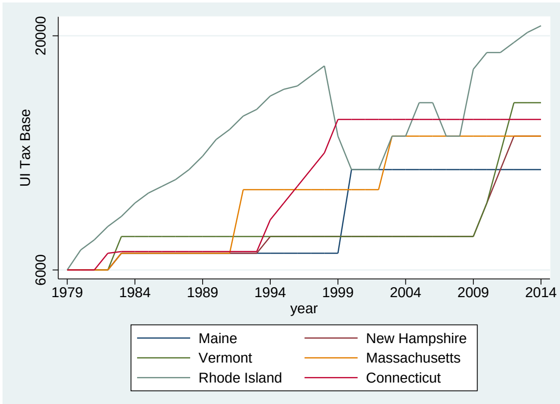
Figure B.4 Average UI Employer Tax Rate (% of Taxable Payroll)



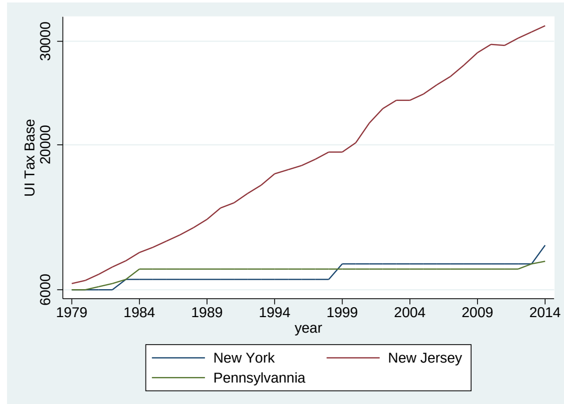
Notes: This figure plots the ratio of UI taxes payroll to taxable payroll from 1979 to 2013. The red line shows the average ratio for indexed states. The blue line indicates the average ratio for non-indexed states. Data are from ET Financial Data Handbook 394 Report.

Figure B.5 UI Tax Base in New England, Middle Atlantic, East North Central, West North Central, South Atlantic, and East South Census Disvision

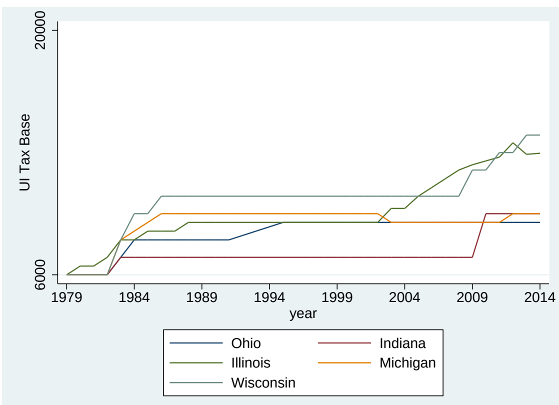
(a) New England



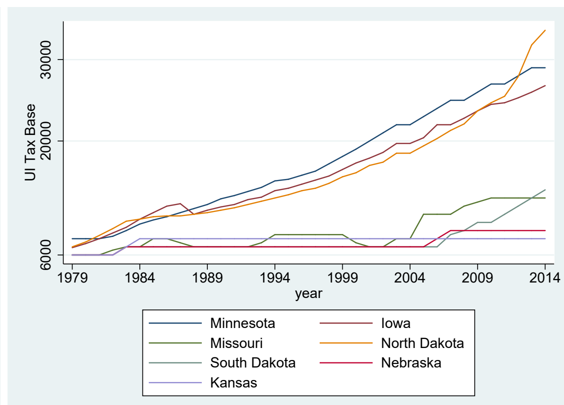
(b) Middle Atlantic



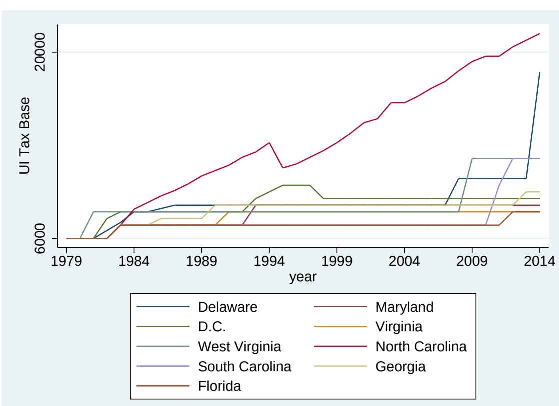
(c) East North Central



(d) West North Central



(e) South Atlantic



(f) East South

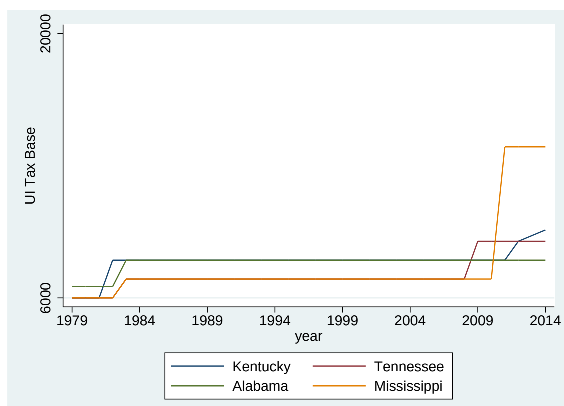
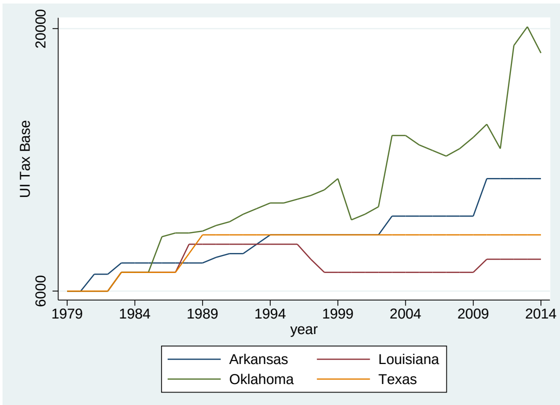
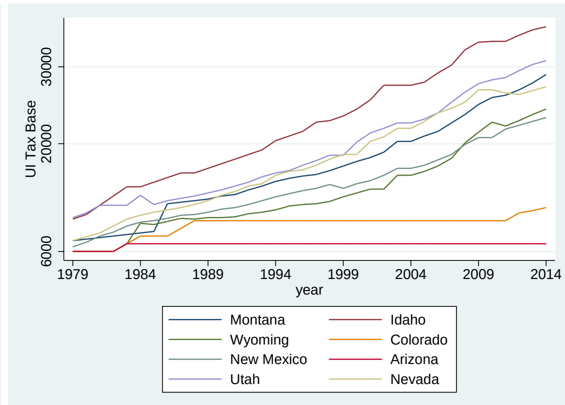


Figure B.6 UI Tax Base in West South, Mountain and Pacific Census Division

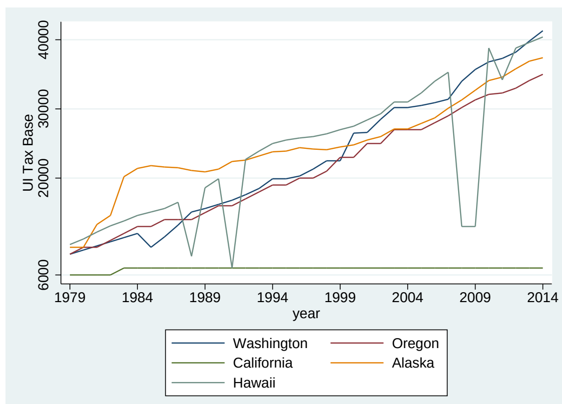
(a) West South



(b) Mountain

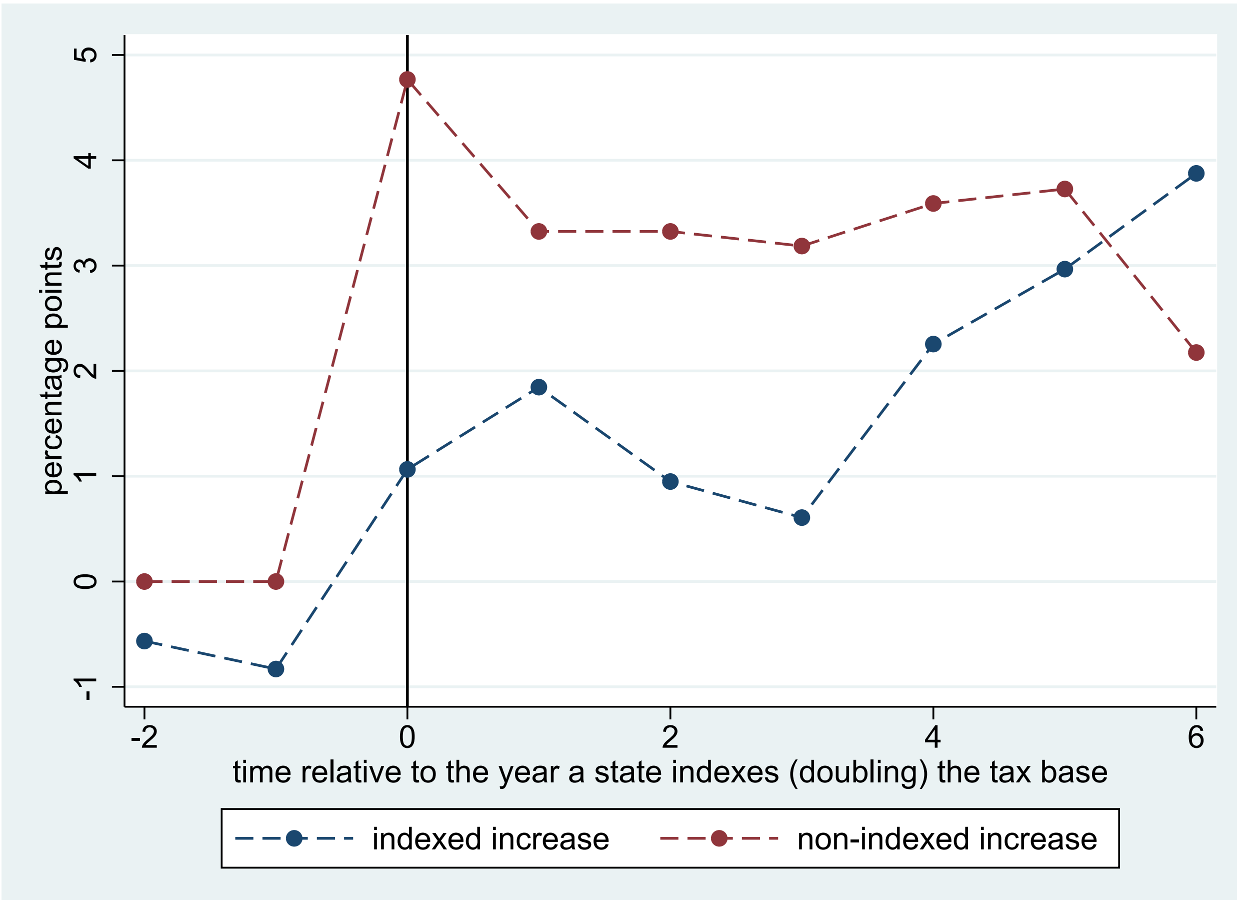


(c) Pacific



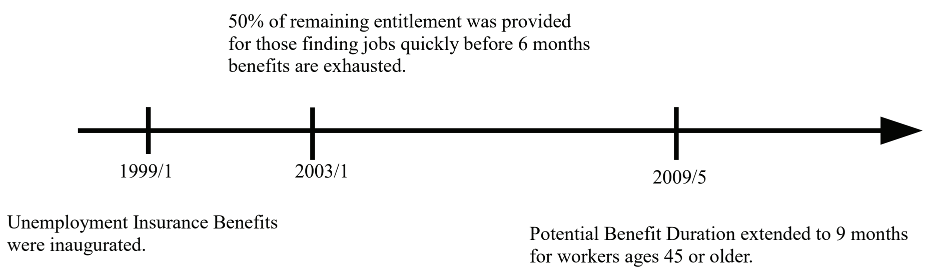
Notes: These figures plot the UI tax bases for all 50 U.S. states and the District of Columbia from 1979 to 2014. I divide the states into nine groups according to the Census Bureau's regional divisions. Data are from ET Financial Data Handbook 394 Report.

Figure B.7 Comparison between the Effects of Indexing the Tax Base and Doubling the Tax Base on Teenage Employment Rates



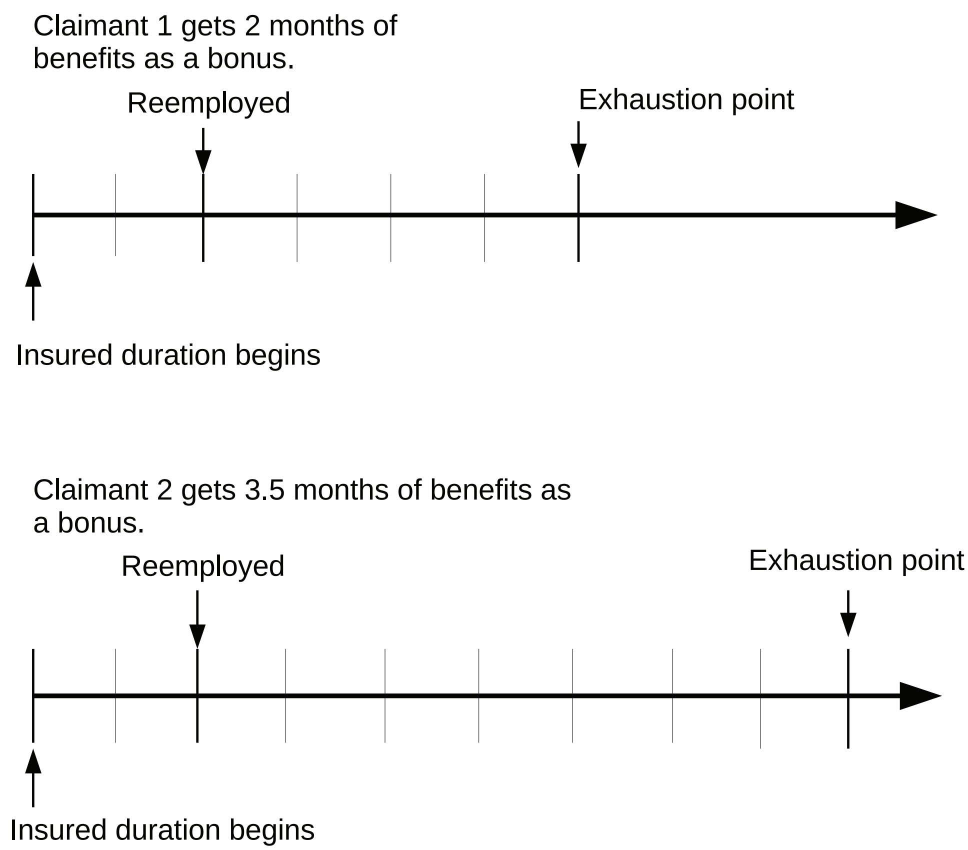
Note: This figure considers the effects of two different increases in the tax base on teenage employment rates. The blue dashed line plots the estimated effects of indexing the tax base on teenage employment; the red line plots the estimated effects of doubling the tax base on teenage employment. Both estimates begin two years before indexing, and extend to the sixth year afterward. The blue line was created using column 5 of Table A.10, the red column 5 of Table A.13.

Figure B.8 Timeline of UI Reforms



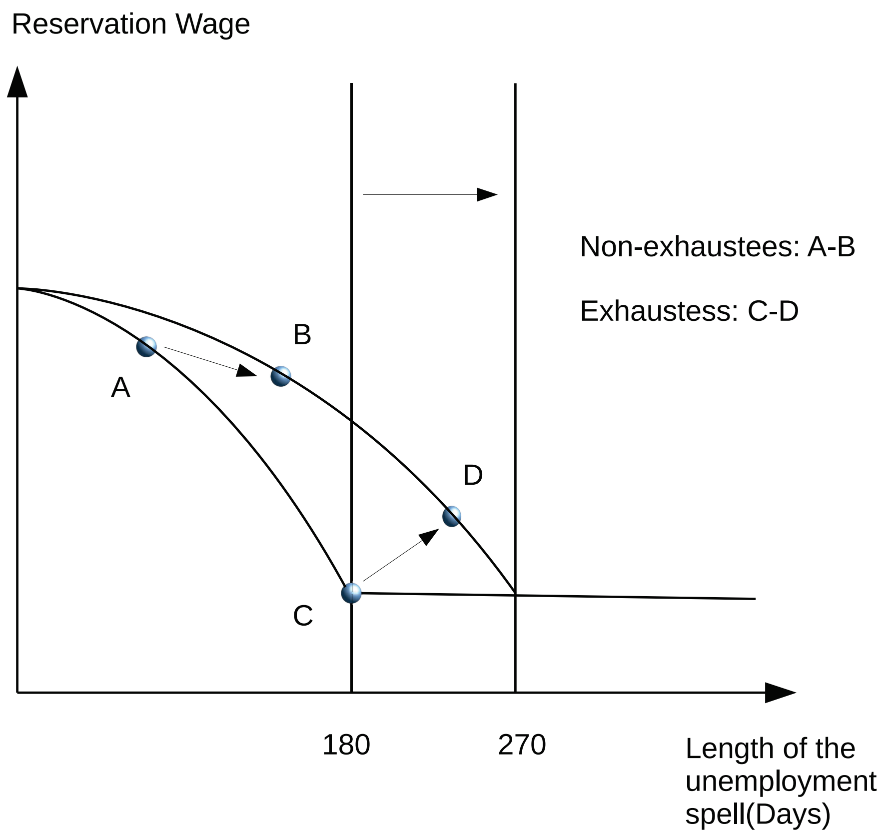
Notes: This figure summarizes the evolution of Taiwan's UI. UI in Taiwan was inaugurated in Jan 1999. On May 15, 2002, the reemployment bonus program was announced. On January 1, 2003, a bonus, equal to 50% of remaining benefits, began to offer for UI recipients who find jobs before exhausting benefits. The potential duration for the worker aged 45 or older has extended from 6 months to 9 months since May 1, 2009.

Figure B.9 Examples of Benefit Extension in Taiwan



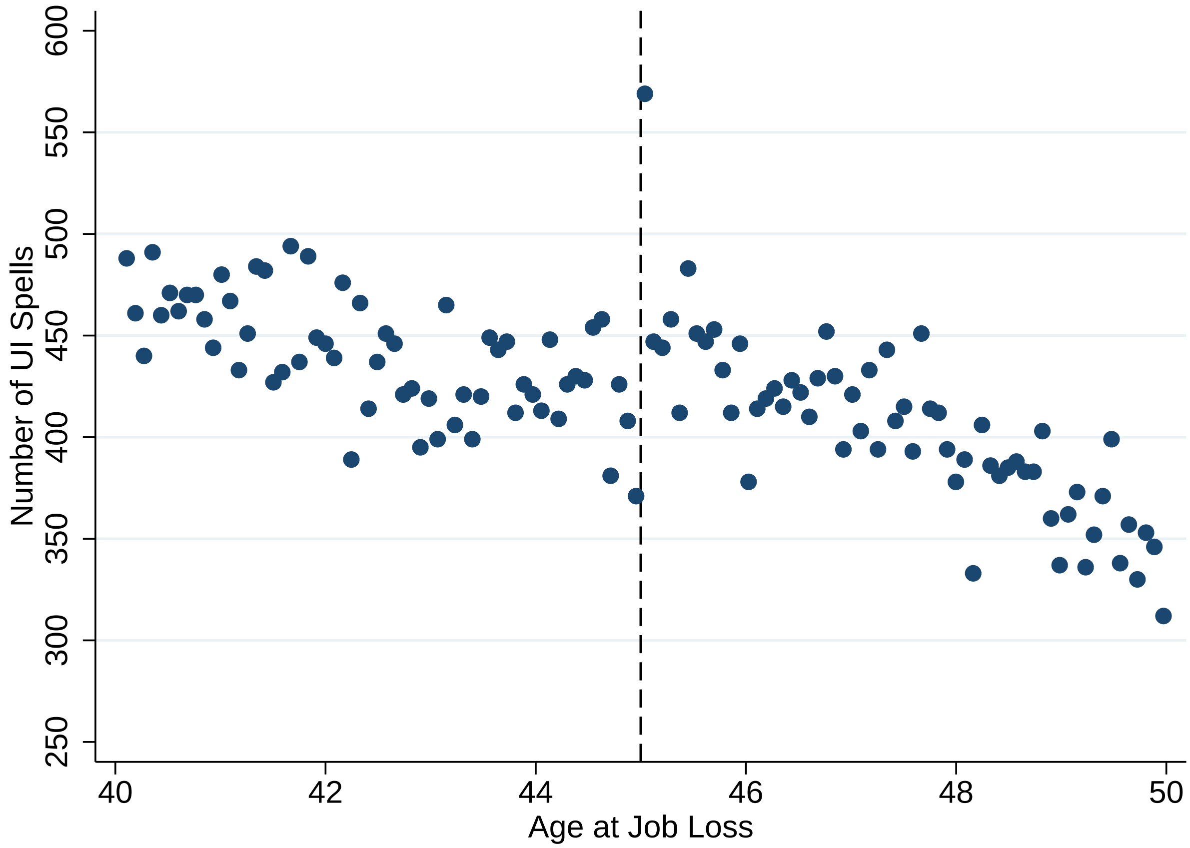
Notes: The figure provides two examples for Taiwan's version of benefit extension. Claimant 1 age below 45 at job loss is eligible for 6 months of benefits. If he/she finds a job in the end of the second month of the UI spell, he/she will be eligible additional two months of benefits as a bonus. Claimant 2 aged above 45 at job loss is eligible for 9 months of benefits. He/she also finds a job in the end of second month of the UI spell, but he/she can receive additional 3.5 months of benefits as a bonus after reemployment.

Figure B.10 Effect of UI Extension on Reservation Wage



Notes: This figure plots a typical reservation wage curve over an unemployment spell. The reservation wage becomes lower closer to the exhaustion point and stays constant afterwards. An increase in potential duration from 180 days to 270 days shifts the reservation wage curve outward. For a non-exhaustee, extended benefits lower the post reemployment wage if the declining reservation wage dominates the outward shift of the reservation wage. For an exhaustee, extended benefits weakly increase the reservation wage in the period of reemployment because the reservation wage stays constant after the exhaustion point.

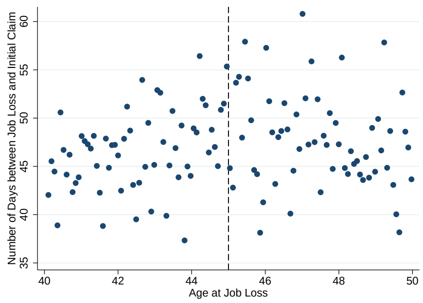
Figure B.11 Validity of RD Design: Density Test



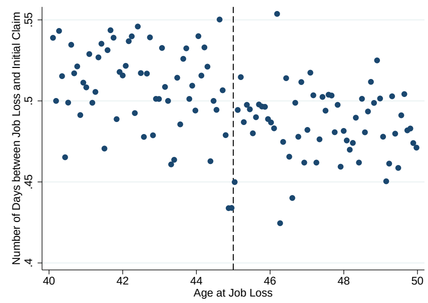
Notes: This figure plots the number of workers starting UI spells between May 1, 2009 and January 1, 2012, conditional on age at job loss. Each bin corresponds to the total number of workers starting UI spells within a 30-day interval.

Figure B.12 RD: Smoothness of Covariates

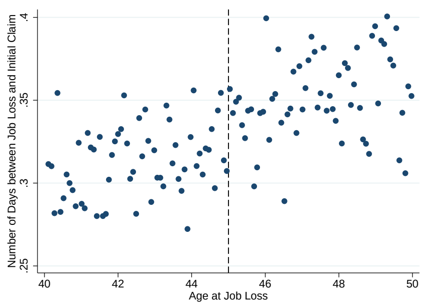
(a) Number of Days Between Job Loss and Initial Claim



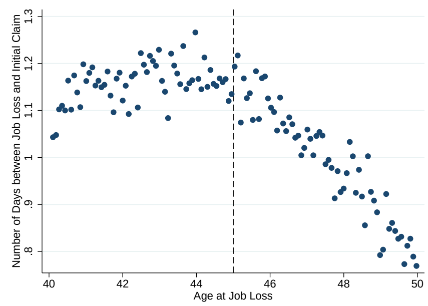
(b) Female



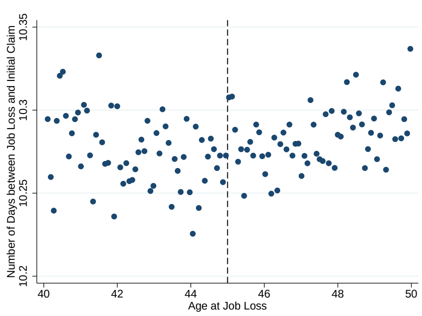
(c) Worked in Manufacturing Sector (Last Job)



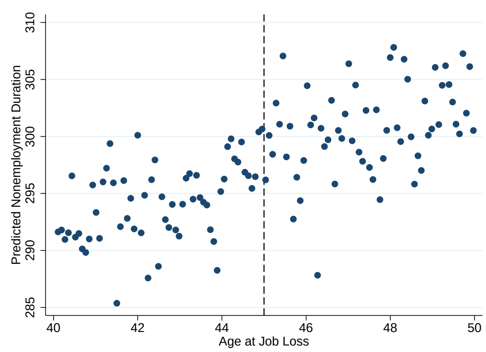
(d) Number of Dependents



(e) Log Average Monthly Earnings Prior to Layoff



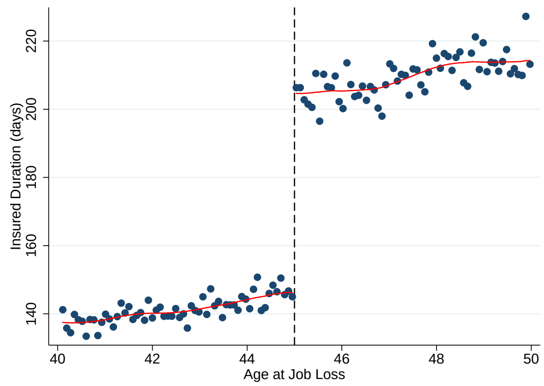
(f) Predicted Nonemployment Duration



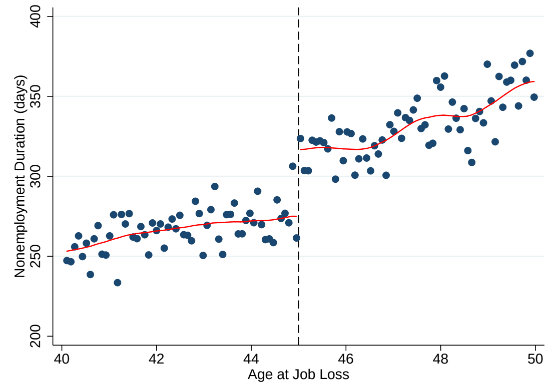
Notes: These figures plot the averages workers' characteristics for UI spells starting between May 1, 2009 and January 1, 2012. The predictors for nonemployment duration are previous earnings, squared previous earnings, previous industry, gender, place of birth, number of dependants, month/year at job loss and the number of days between job loss and claiming benefits. Each bin represents the average number of UI recipients within a 30-day interval.

Figure B.13 Effects of Extended UI Benefits

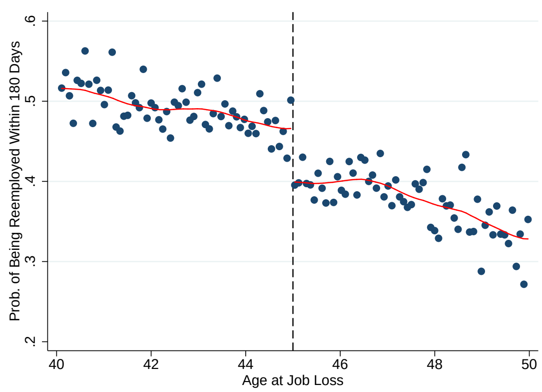
(a) Insured Duration



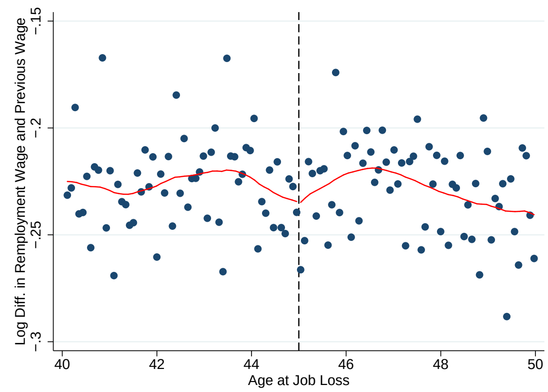
(b) Nonemployment Duration



(c) Probability of Being Reemployed in 180 Days

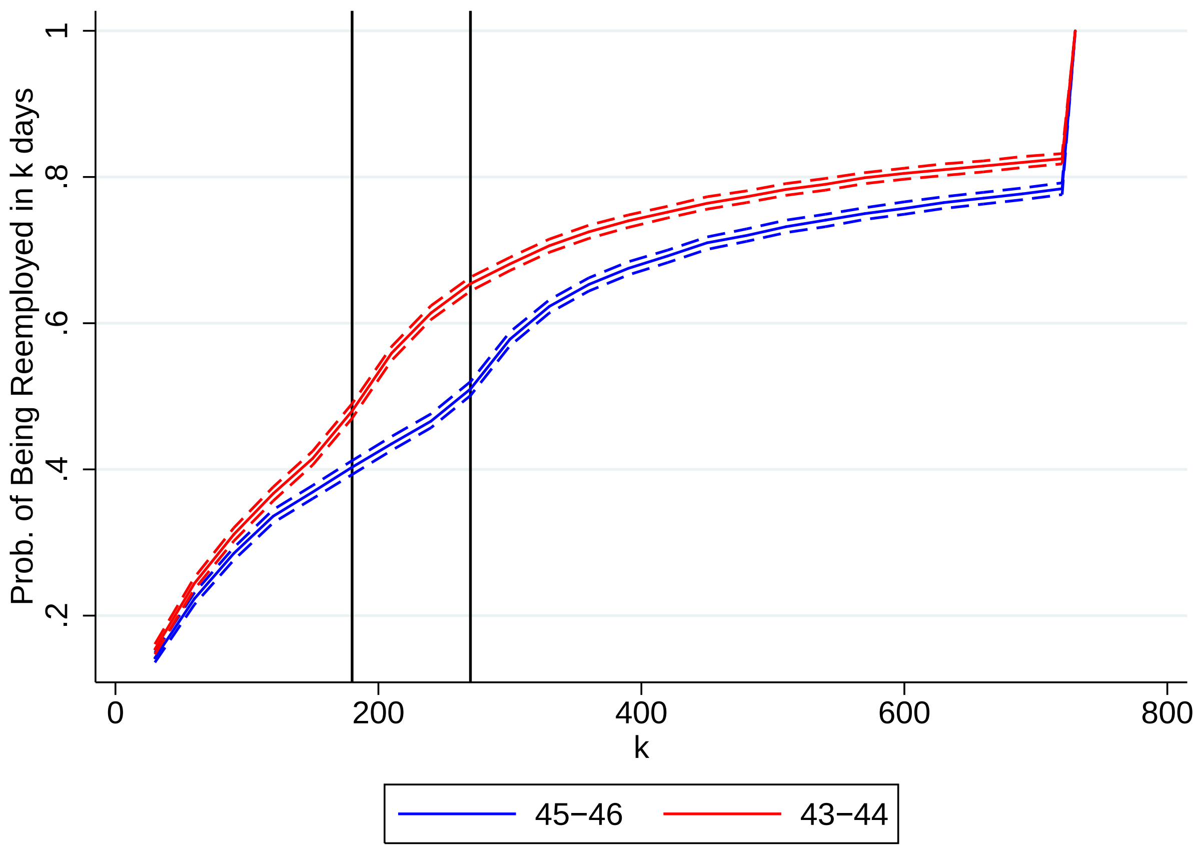


(d) Difference in Log Earnings Between Post- and Pre- Unemployment Jobs



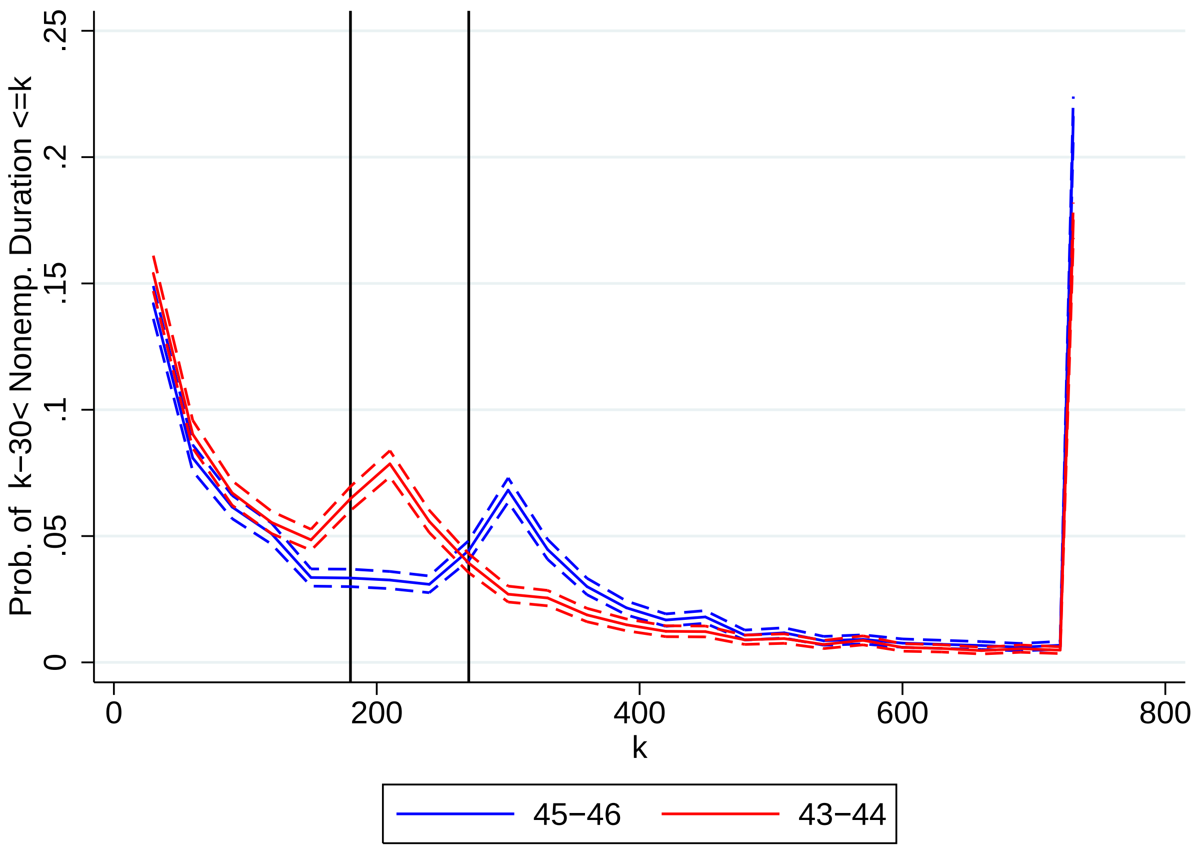
Notes: These figures plot the average outcomes for UI recipients aged 40 to 50 at job loss for UI spells starting between May 1, 2009 and January 1, 2012. Each bin represents the average number of UI recipients within a 30-day interval. The solid lines are fitted values from a local linear regression on either side of the cutoff using an edge kernel, with a bandwidth of one year.

Figure B.14 Cumulative Distribution of Nonemployment Duration



Notes: This figure plots the probability of being reemployed in k days for workers aged 45-46 and 43-44 at job loss, respectively. We plot the probability from $k = 30$, $k = 60$, .., to $k = 720$, and $k = 730$. The dashed lines are 95% confidence interval. The solid vertical lines indicate 180th day and 270th day of the nonemployment spell.

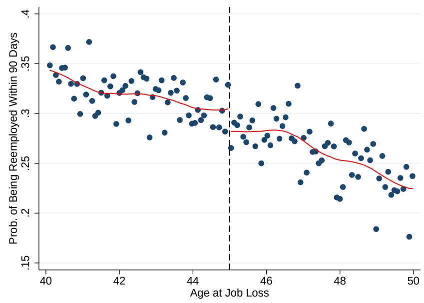
Figure B.15 Density Distribution of Nonemployment Duration



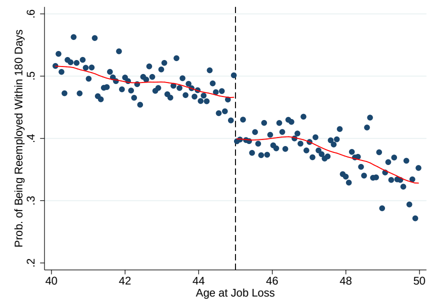
Notes: This figure plots $P(k-30 < \text{nonemployment duration} \leq k)$ for workers aged 45-46 and 43-44 at job loss, respectively. We plot the probability from $k = 30$, $k = 60$, .., to $k = 720$, and $k = 730$. The dashed lines are 95% confidence interval. The solid vertical lines indicate 180th day and 270th day of the nonemployment spell.

Figure B.16 RD: Probability of Being Reemployed Within k Days

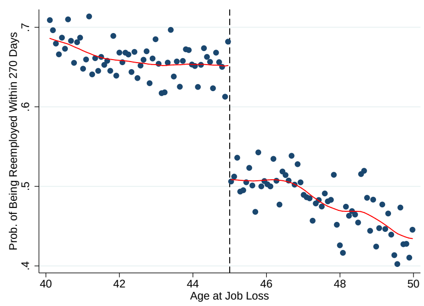
(a) Probability of Being Reemployed Within 90 Days



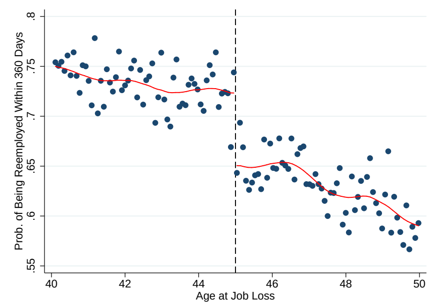
(b) Probability of Being Reemployed Within 180 Days



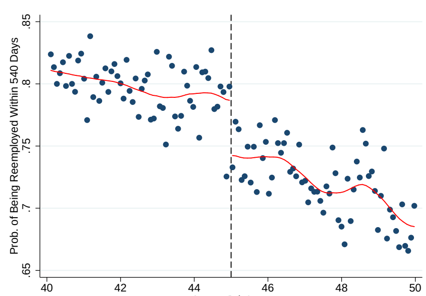
(c) Probability of Being Reemployed Within 270 Days



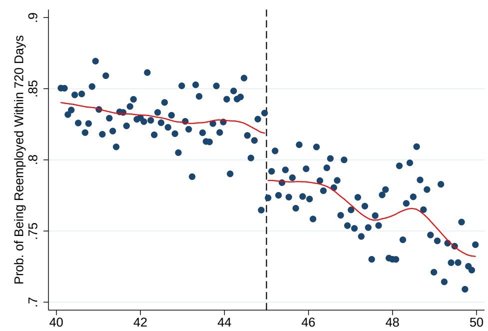
(d) Probability of Being Reemployed Within 360 Days



(e) Probability of Being Reemployed Within 540 Days



(f) Probability of Being Reemployed Within 720 Days



Notes: These figures plot the probability of being reemployed before k days for UI spells starting between May 1, 2009 and January 1, 2012. Each bin represents the average within a 30-day interval. The solid lines are fitted values from a local linear regression on either side of the cutoff using an edge kernel, with a bandwidth of one year.

APPENDIX C

THEORETICAL DERIVATIONS

The discrete-time search model with reservation wages we discussed in section 2.4 of chapter 2 and section 3.3 of chapter 3 is based on the model in Chetty (2008, 178-179). Our model differs from the model from Chetty (2008) in two ways. First, we include the reemployment bonus into the model. Second, we assume workers control their reservation wages instead of search intensities.[1] When deriving the effects of reemployment bonus and extended benefits on reservation wages, we apply Euler conditions as Landais (2015b, 34-38).

Consider an unemployed worker becomes unemployed at time t and holds an initial asset A_t. She lives for infinite periods of time and draws a wage offer, w, from a known and stationary wage distribution, $F(w)$, in each period of unemployment. If she rejects the offer, she receives an unemployment benefit, b_t, with a potential duration, P, that is

$$b_t = \begin{cases} b, & \text{if } 0 \leq t < P \\ 0, & \text{if } t \geq P \end{cases}$$

If she accepts the offer, she earns a wage rate w_t, pays a tax rate, τ, and keeps the job forever. Moreover, if she is reemployed before running out of benefits, she receives a reemployment bonus, r_t, equal to θ percent of remaining benefits; otherwise, $r_t = 0$. Formally,

$$r_t = \theta \cdot \sum_{k=t}^{P-1} b_k, 0 < \theta < 1$$

The worker's consumption at time t equals the difference in income and saving. The income depends on her employment status, while the change in asset, $A_{t+1} - A_t$ reflects her saving. When employed, she earns wage rate, w_t, bonus, r_t, and pays a tax, τ. The flow utility when employed at time t equals $u(c_t^e) = u(A_t - A_{t+1} + w + r_t - \tau)$, where c_t^e indicates the consumption when employed at time t. Assuming the the time discount rate equals β, the value of being employed in period t is

$$V_t = \max_{A_{t+1}} u(A_t - A_{t+1} + w + r_t - \tau) + \beta V_{t+1}(A_{t+1})$$

[1] For search models with reservation wages, see Burdett (1979) and Mortensen (1977).

If an unemployed worker cannot find a job in period t, her flow utility is equal to $u(c_t^u) = u(A_t - A_{t+1} + b_t)$. The value of being unemployed in period t is

$$U_t = \max_{A_{t+1}} u(A_t - A_{t+1} + b_t) + \beta J_{t+1}(A_{t+1}),$$

where $J_{t+1}(A_{t+1})$ is the value of entering period $t+1$ unemployed with asset A_{t+1}. The value in the beginning of period t without a job is

$$J_t(A_t) = \max_{R_t} [1 - F(R_t)]V_t(A_t) + F(R_t)U_t(A_t),$$

The intratemporal first order condition balances the value of being employed and the value of being unemployed in period t.

$$V_t(A_t) = U_t(A_t)$$

The effect of an increase in P on the reservation wage in period t is dependent on the effect on the value of employment in period t and the value of unemployment in period t. For $t \leq P$, an increase in P increases the value of being employed.

$$\frac{\partial V_t(A_t)}{\partial P} = b\theta u'(c_t^e) + \frac{\partial R_t}{\partial P}[u'(c_t^e) + \beta u'(c_{t+1}^e) + ...] \tag{C.1}$$

An increase in P increases the value of unemployment in period t through two channels. On one hand, it increases the value of unemployment in period t because it increases the utility of being unemployed in period P. On the other hand, it also increases the utility of finding a job in any period before exhaustion point. That is,

$$\begin{aligned} \frac{\partial U_t(A_t)}{\partial P} &= b[\beta^{P-t}(1-p_{t+1})..(1-p_P)u'(c_P^u) \qquad \text{(C.2)} \\ &+ p_{t+1}\beta\theta u'(c_{t+1}^e) + .. + (1-p_{t+1})..p_P\beta^{P-t}\theta u'(c_P^e)] \end{aligned}$$

As Landais (2015b, 34-38) demonstrates, we can apply Euler conditions to simplify Equations C.1, C.2. Since workers face no uncertainty after they are employed, the marginal utility of consumption when workers are employed at time t equals the marginal utility of consumption at time $t+1$ when employed if the liquidity constraint does not bind. Otherwise, workers set consumption at time t

equal to after tax wage rate. Formally, we can write the intertemporal first order condition when employed as follows:

$$u'(c_t^e) = \begin{array}{l} \beta u'(c_{t+1}^e);\ \text{if } A_t > L \\ u'(w-\tau);\ \text{if } A_t = L \end{array}$$

Similarly, if workers are unemployed at time t, they smooth consumption such that the marginal utility of consumption when unemployed at time t equals the expected marginal utility of consumption at time $t+1$. That is, the intertemporal first order condition when unemployed is

$$u'(c_t^u) = \begin{array}{c} \beta[p_{t+1}u'(c_{t+1}^e) + (1-p_{t+1})u'(c_{t+1}^u)]; \text{if } A_t > L \\ u'(b_t);\ \text{if } A_t = L, \end{array}$$

where $p_t = P(w > R_t)$. If liquidity constraint is not binding yet at exhaustion point, $P-1$,

$$u'(c_t^e) = \beta_{P-t}u'(c_P^e);$$

$$u'(c_t^u) = \beta^{P-t}[1-S_{t+1}(P)]u'(c_P^e) + S_{t+1}(P)u'(c_P^u).$$

Using Equations C.1, C.2 and the intertemporal first order conditions, we can write

$$\begin{aligned} \frac{\partial R_t}{\partial P} &= b(1-\beta)[\frac{u'(c_t^u)-u'(c_t^e)}{u'(c_t^e)} + S_{t+1}(P)(1-\theta)] \\ &> 0 \end{aligned}$$

Further, to express the effect of an increases in P on the reservation wage as the sum of a liquidity and a moral hazard effect, we use

$$\frac{\partial R_t}{\partial A_t} = (1-\beta)\frac{u'(c_t^e)-u'(c_t^u)}{u'(c_t^e)};$$

$$\frac{\partial R_t}{\partial r_t} = -(1-\beta).$$

Therefore, we get

$$\frac{\partial R_t}{\partial P} = \frac{\partial R_t}{\partial A_t} - (1-\theta)S_{t+1}(P)\frac{\partial R_t}{\partial r_t}.$$

APPENDIX D

ADDITIONAL TABLES

Table A1 Estimates of the Effects of Extended Benefits on Durations – Excluding Observations Within k Days of the Cutoff

	Insured duration (1)	Nonemployment duration (2)
$k = 30$	55.83***	32.07***
	(1.60)	(5.21)
Sample Size	42,591	49,329
$k = 60$	55.51***	36.69***
	(1.72)	(5.57)
Sample Size	40,444	46,199
$k = 90$	55.27***	38.43***
	(1.81)	(6.01)
Sample Size	39,947	42,416
$k = 120$	56.00***	36.69***
	(1.82)	(6.16)
Sample Size	42,867	43,203
$k = 150$	56.68***	36.20***
	(1.89)	(6.28)
Sample Size	42,822	43,131
$k = 180$	56.26***	33.24***
	(1.90)	(5.68)
Sample Size	42,524	51,829
Poly. model	linear	linear
Bandwidth (days)	CCT	CCT

Note: This table shows the estimates of the effect of increasing potential duration from 6 months to 9 months on insured duration and nonemployment duration. We estimate a local linear regression using the optimal bandwidth by Calonico et al. (2014) and a triangular kernel. The sample are workers starting UI spells between May 1, 2009 and January 1, 2012, and aged within the bandwidth excluding those aged within k days of the cutoff. Standard errors in parentheses are clustered by age in days.

Table A2 Estimates of the Effects of Extended Benefits on Wages – Excluding Observations Within k Days of the Cutoff

Reemployment Earnings	with 1st Employer (1)	1 year after Initial Claim (2)	2 years after Initial Claim (3)
$k = 30$	0.014*	0.003	0.005
	(0.008)	(0.010)	(0.009)
Sample Size	48,988	31,876	33,136
$k = 60$	0.013	-0.000	0.002
	(0.008)	(0.011)	(0.010)
Sample Size	46,670	30,370	31,826
$k = 90$	0.010	-0.000	0.007
	(0.011)	(0.012)	(0.010)
Sample Size	35,872	25,958	31,376
$k = 120$	0.010	-0.007	0.002
	(0.011)	(0.013)	(0.012)
Sample Size	35,872	24,297	28,094
$k = 150$	0.010	-0.001	0.008
	(0.010)	(0.012)	(0.011)
Sample Size	41,463	29,217	32,399
$k = 180$	0.017*	0.013	0.016
	(0.010)	(0.011)	(0.010)
Sample Size	44,193	33,417	35,502
Poly. model	linear	linear	linear
Bandwidth (days)	CCT	CCT	CCT

Note: This table shows the estimates of the effect of increasing potential duration from 6 months to 9 months on reemployment earnings for the first employer, one year, and two years after initial claim. We estimate a local linear regression using the optimal bandwidth by Calonico et al. (2014) and a triangular kernel. The sample are workers starting UI spells between May 1, 2009 and January 1, 2012, and aged within the bandwidth excluding those aged within k days of the cutoff. Standard errors in parentheses are clustered by age in days.

Table A3 Placebo Test for RD Design

	(1)	(2)	(3)	(4)	(5)
Panel A: *Insured Duration*					
β_{EB}	1.49	1.34	2.09	1.48	1.19
	(1.88)	(1.87)	(1.68)	(1.93)	(2.07)
Sample size	14,023	14,023	18,531	18,531	26,690
Panel B: *Nonemployment Duration*					
β_{EB}	2.28	2.18	4.50	2.63	0.08
	(9.18)	(9.15)	(7.85)	(9.30)	(9.40)
Sample size	14,023	14,023	18,487	18,487	32,081
Panel C: *Nonemployment Duration* $<$ *180 days*					
β_{EB}	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Sample size	14,023	14,023	28,935	28,935	27,002
Bias-corrected	–	–	–	Yes	Yes
Covariates	–	Yes	–	–	–
Poly. model	linear	linear	linear	linear	quadratic
Bandwidth (days)	730	730	CCT	CCT	CCT

Note: This table conducts a placebo test using sample before UI extension. Column 1 estimate a linear regression on either side of the cutoff using sample from workers age 43-46 at job loss for UI spells starting before November 1, 2008. Column 2 includes the following covariates: previous earnings, squared previous earnings, previous industry, gender, place of birth, number of dependants, month/year at job loss, number of job loss and number of days between job loss and initial claim. Columns 3 reports the estimates using optimal bandwidth algorithm from Calonico et al. (2014). The optimal bandwidths vary with the outcome variables, in the range of 4 to 6 years. The bias correction estimates and the corresponding robust standard errors are presented in column 4. In column 5, we report the bias correction estimates and robust standard error using local quadratic regression. Standard errors in parentheses are all clustered by age in days. Columns 1 and 2 use a rectangular kernel. Columns 3-5 use triangular kernel. *** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

Table A4 The Effect of Extended Benefits on Particpation in Vocational Training

	(1) Participation in Training	(2) # of Days Receiving Training Subsidies
β_{EB}	-0.003	-0.105
	(0.005)	(0.668)
Baseline Mean	0.080	8.282
Sample size	39,996	40,352
Poly. model	linear	linear
Bandwidth (days)	CCT	CCT

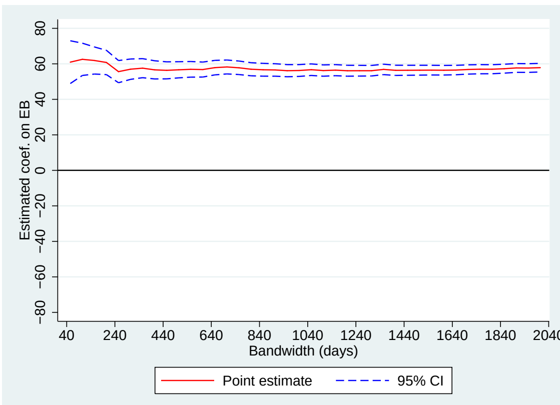
Note: This table examines whether an increase in potential duration form 6 to 9 months on the participation in vocational training and duration of training. We estimate a local linear regression using the optimal bandwidth by Calonico et al. (2014) and a triangular kernel. The sample are workers aged within the bandwidth and starting UI spells between May 1, 2009 and January 1, 2012. Standard errors in parentheses are clustered by age in days. *** significant at the 1 percent level, ** significant at the 5 percent level, and * significant at the 10 percent level.

APPENDIX E

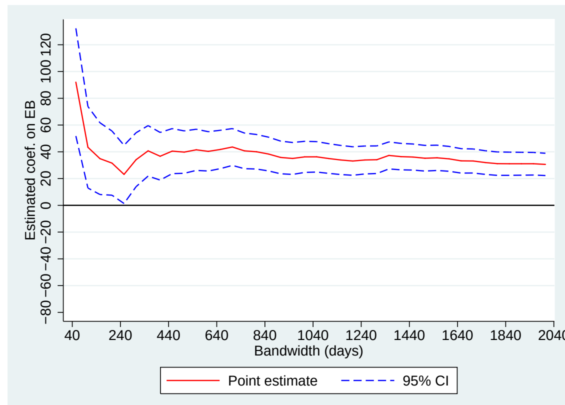
ADDITIONAL FIGURES

Figure A1 RD Estimates with Varying bandwidths

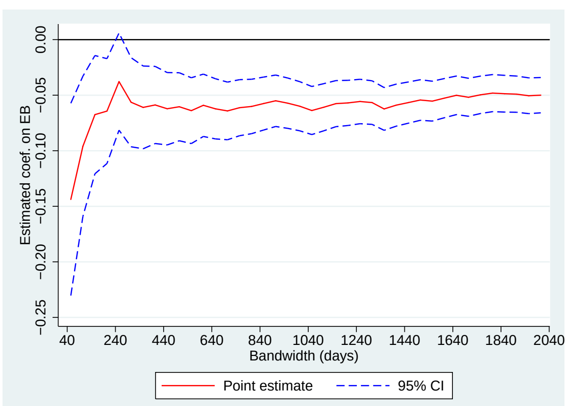
(a) Insured Duration



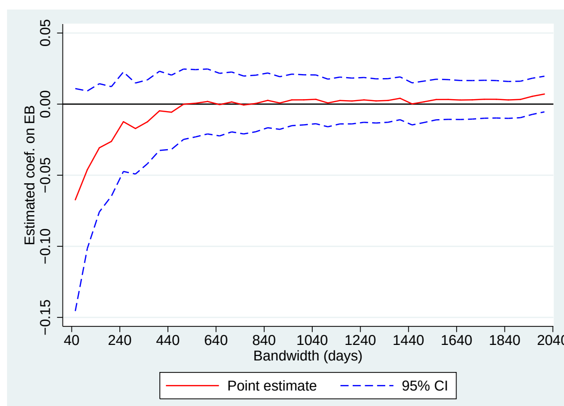
(b) Nonemployment Duration



(c) Probability of Being Reemployed in 180 Days



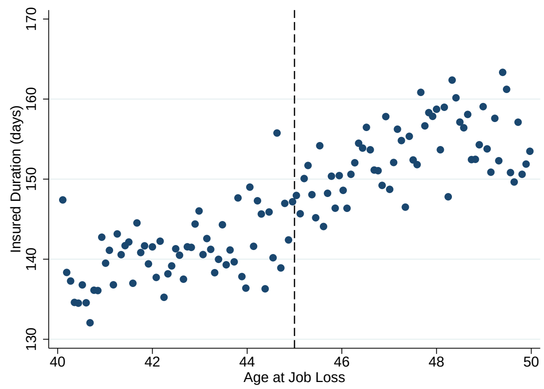
(d) Difference in Log Reemployment Earnings and Log Previous Earnings



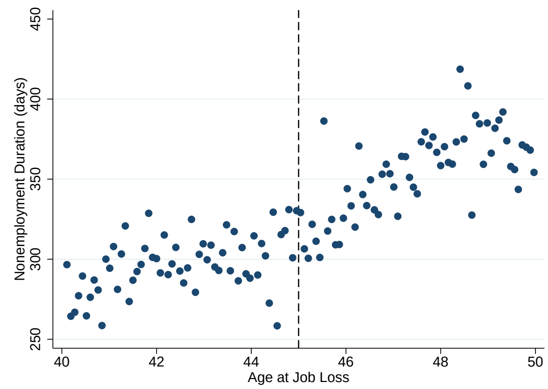
Notes: These figures test for the sensitivity to bandwidth choice for our RD estimates. We estimate a local linear regression using a bandwidth ranging from 40 to 2000 days. The solid line indicates the point estimates, and the dashed lines are corresponding confidence intervals.

Figure A2 Placebo Test for RD Design

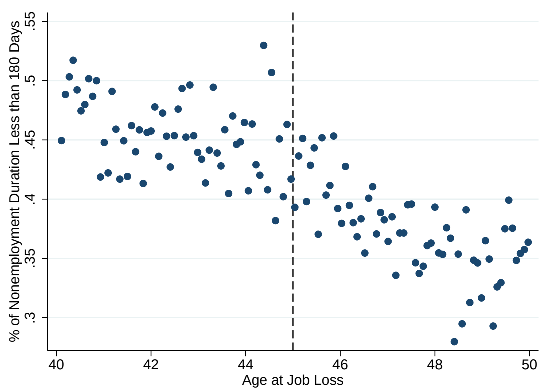
(a) Insured Duration Prior to UI Extension



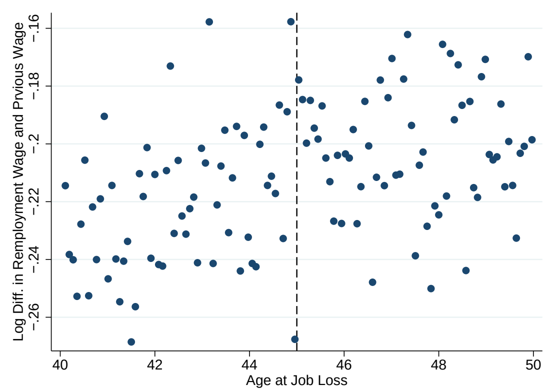
(b) Nonemployment Duration Prior to UI Extension



(c) Probability of Being Reemployed in 180 Days

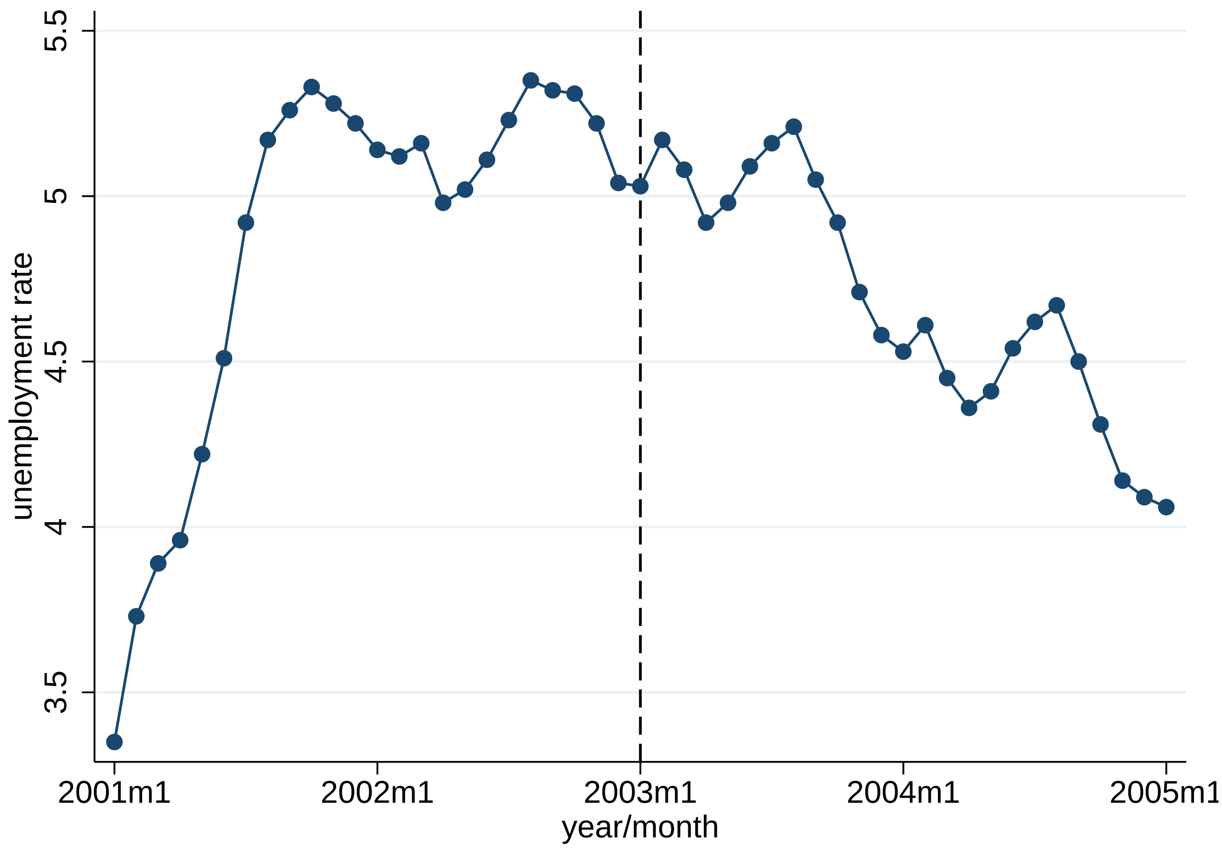


(d) Difference in Log Reemployment Earnings and Log Previous Earnings



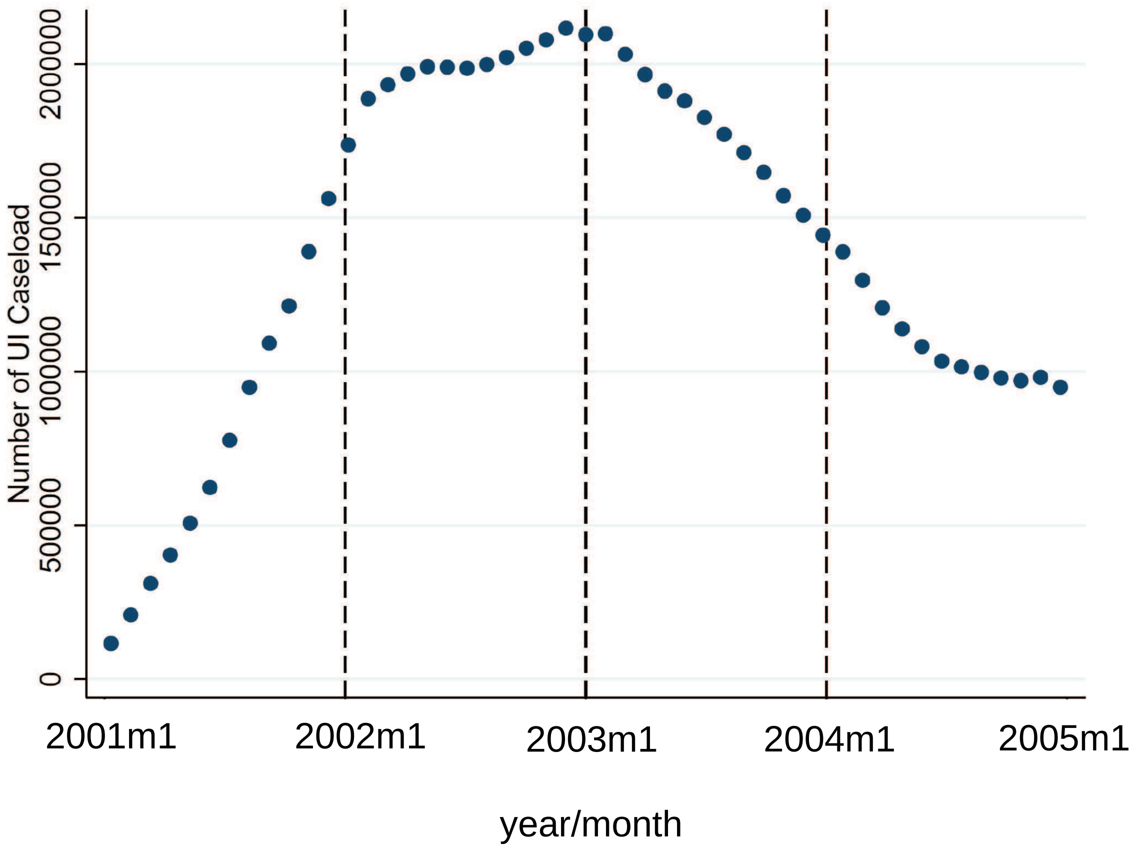
Notes: These figures plot the average outcomes for UI recipients age 40 to 50 at job loss for UI spells starting before November 1, 2008. Each bin represents the average number of UI recipients within a 30-day interval.

Figure E.3 Unemployment Rate in Taiwan Between 2001 to 2005



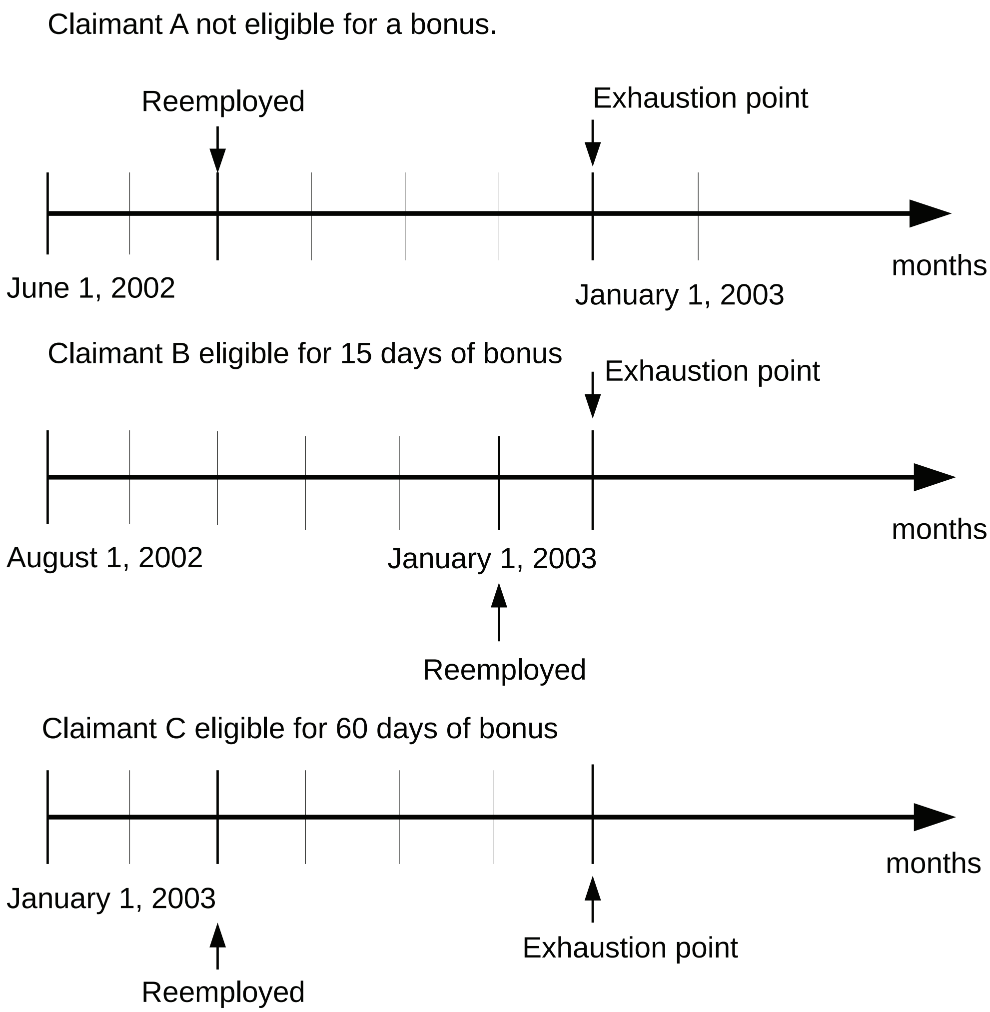
Notes: This figure plots the monthly unemployment rate in Taiwan from January 2001 to January 2005 using data from Directorate-General of Budget, Accounting and Statistics.

Figure E.4 Number of UI Caseload



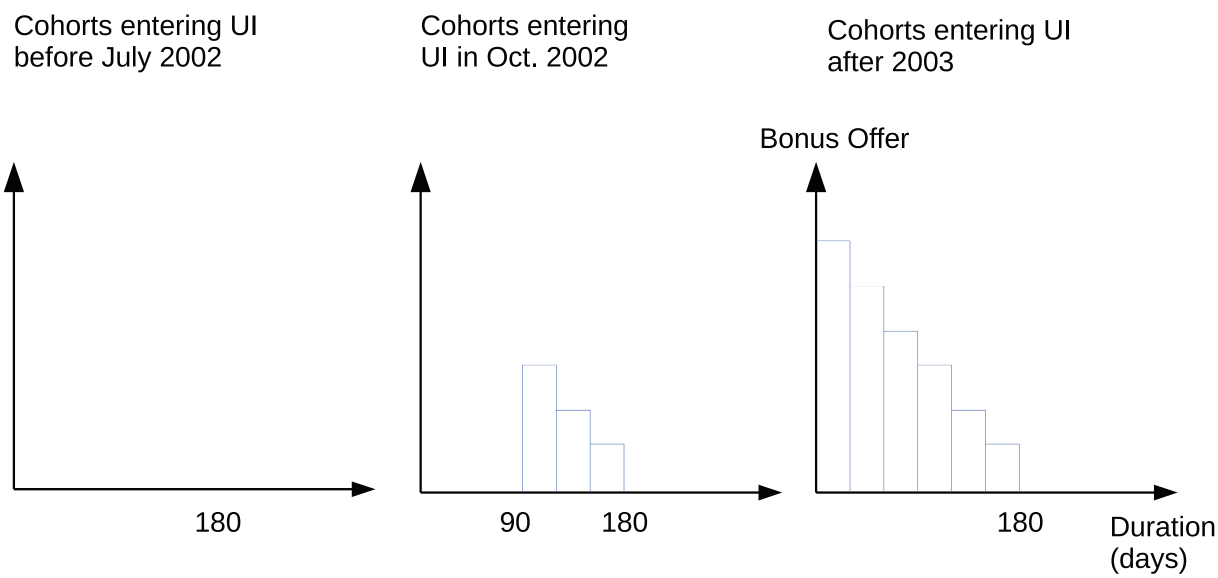
Notes: This graph plots the number of UI caseloads between January 1, 2001 and January 1, 2005. The sample includes every UI spell started within 730 days from Jan 1, 2003. Each bin represents the total number of UI recipients within 30 days interval.

Figure E.5 Reach Back Provison of Reemployment Bonus Program



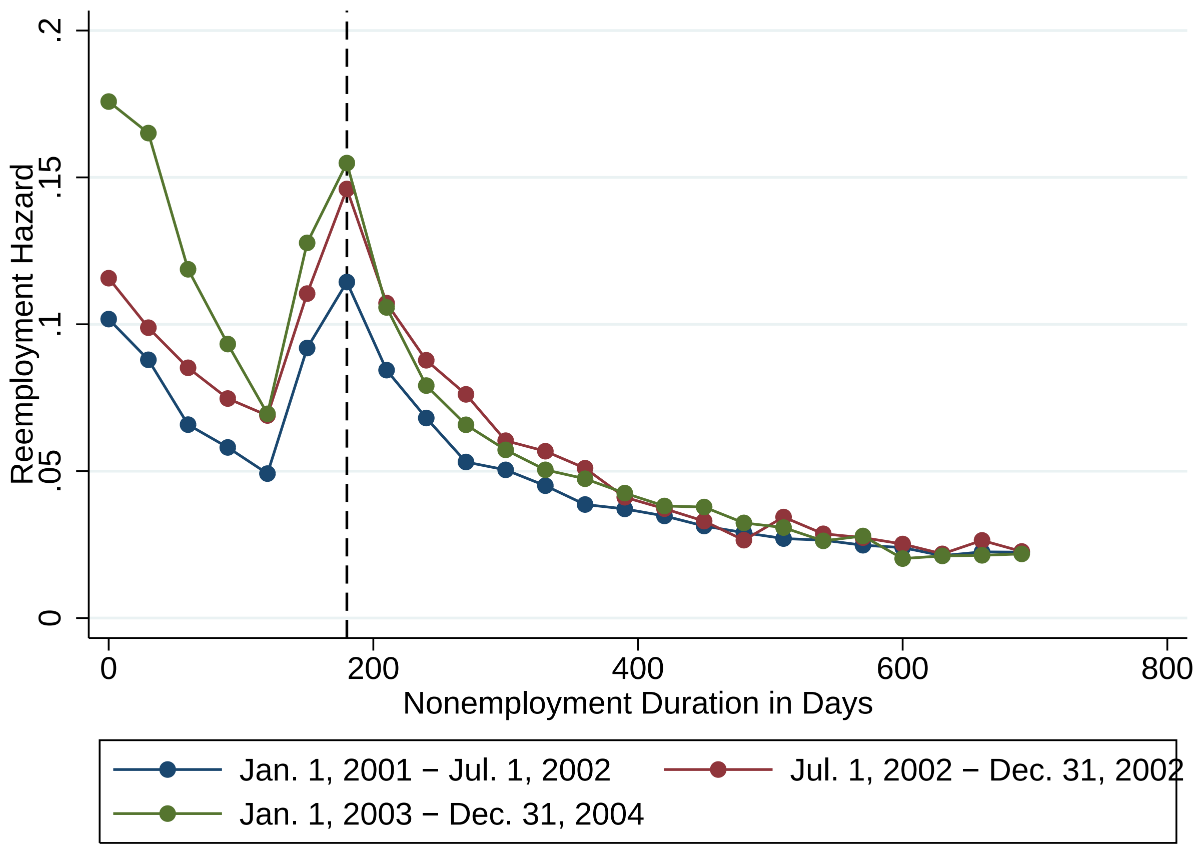
Notes: This figure provides three cases to explain how the reach back provision affects workers' bonus offer. Claimant A's insured duration began on June 1, 2002. He would not be eligible for bonus even he found a job before exhausting benefits, because the program took effect after his exhaustion point. Claimant B began the spell on August 1, 2002. He would be eligible for 15 days of bonus if he found a job on January 1, 2003, but he would not be eligible for bonus if reemployed before 2003. Claimant C started the insured duration on January 1, 2003. He is eligible for 60 days of bonus if reemployed on March 1, 2003.

Figure E.6 Bonus Offer over Unemployment Spell for Cohorts not Exposed, Partially Exposed, and Fully Exposed to the Bonus Program



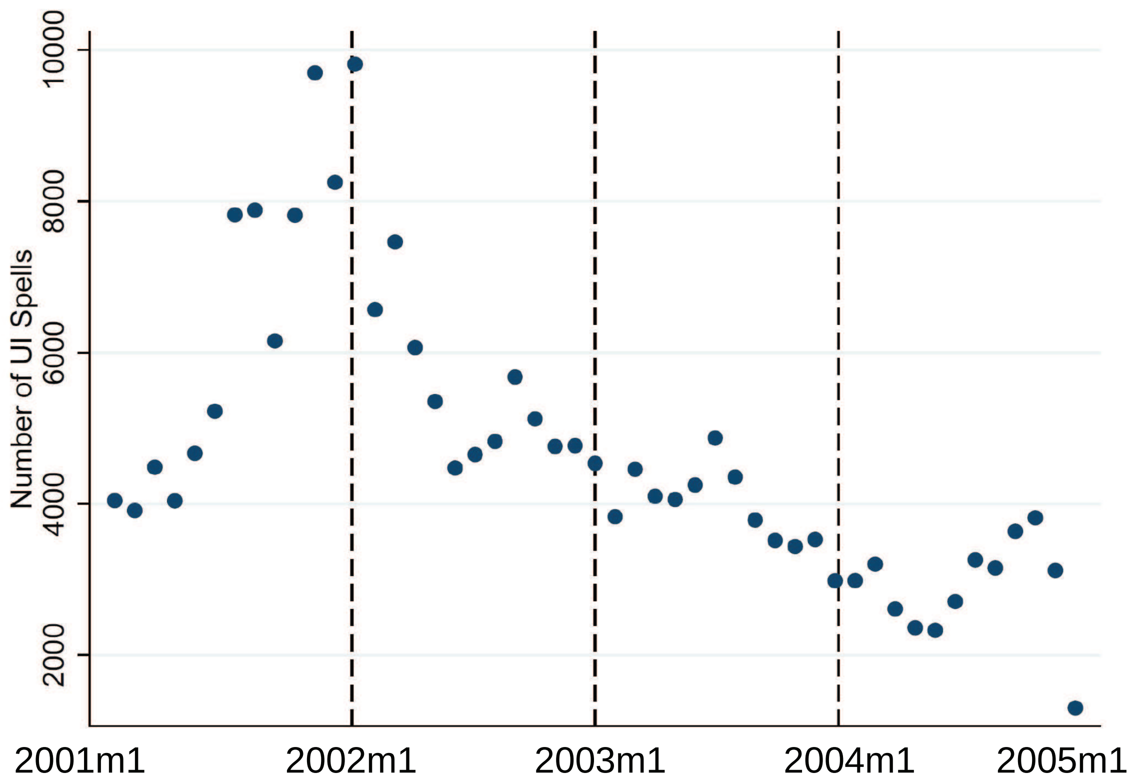
Notes: This figure plots the bonus schedule over the nonemployment spell for cohorts entering UI at three different times. The graph on the left indicates the bonus schedule for workers workers entering UI before July 1, 2002. The graph in the middle indicates the bonus schedule for workers entering UI in October 1, 2002. The graph on the right indicates the bonus schedule for those entering UI after 2003.

Figure E.7 Reemployment Hazard over the Nonemployment Spell for Cohorts not Exposed, Partially Exposed, and Fully Exposed to the Bonus Program



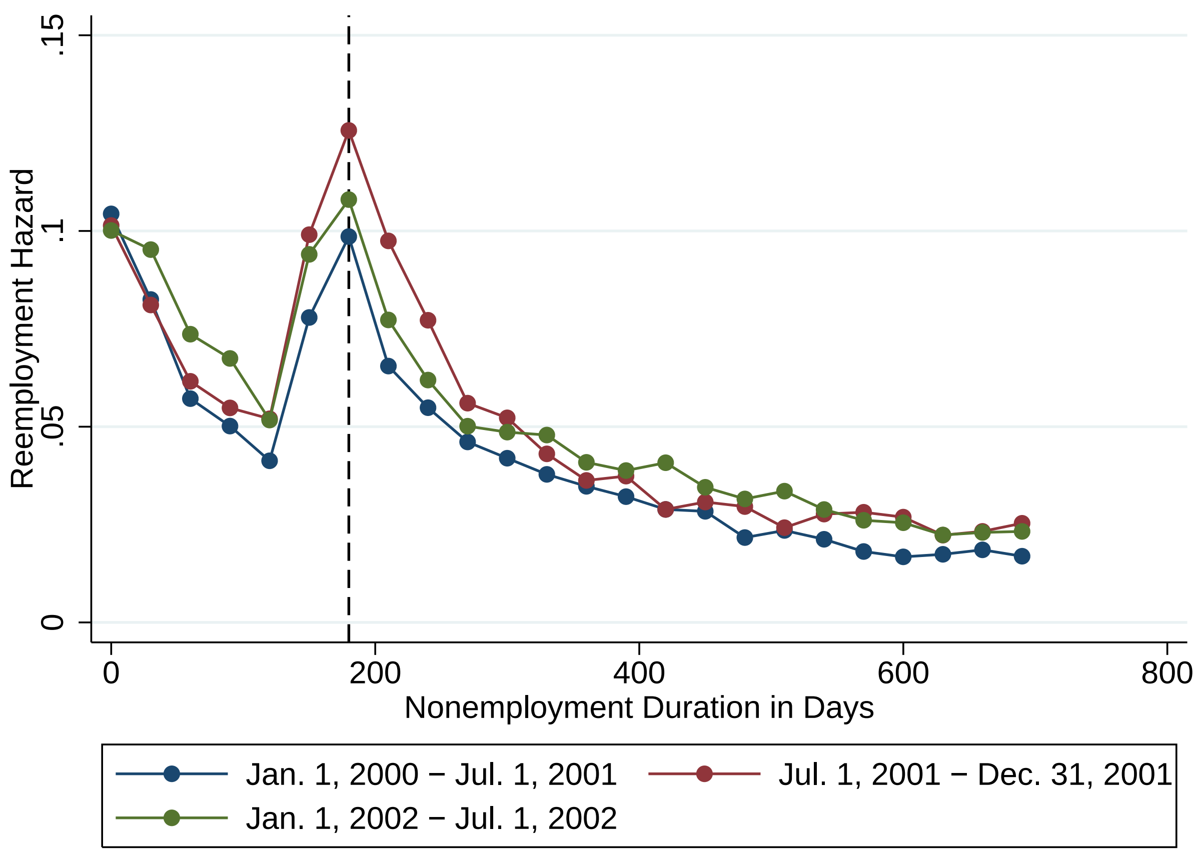
Notes: This figure plots the reemployment hazard over the nonemployment spell for cohorts entering UI during three different time periods. The blue line is the hazard function for workers entering UI between January 1, 2001 and July 1, 2002. The red line is the hazard function for workers entering UI between July 1, 2002 and January 1, 2003. The green line uses is the hazard function for workers entering UI between January 1, 2003 and December 31, 2004. The dashed line indicates the 180th day of the nonemployment spell.

Figure E.8 Number of New UI Claimants over Time



Notes: This graph plots the number of new UI claimants between January 1, 2001 and January 1, 2005. The sample includes every UI spell started within 360 days from January 1, 2003. Each bin represents the total number of UI recipients within a 30-day interval.

Figure E.9 Reemployment Hazard over the Nonemployment Spell—A Falsification Exercise



Notes: This figure plots the reemployment hazard over the nonemployment spell for cohorts entering UI during three different time periods. The blue line is the hazard function for workers entering UI between January 1, 2001 and July 1, 2002. The red line is the hazard function for workers entering UI between July 1, 2002 and January 1, 2003. The green line uses is the hazard function for workers entering UI between January 1, 2003 and December 31, 2004. The dashed line indicates the 180th day of the nonemployment spell.

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