Unemployment Benefits and Reemployment Wages: Evidence from Extended Benefits *

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Abstract

Understanding the effect of increasing unemployment benefit durations on reemployment wages is critical to calculating the welfare consequences of changes in unemployment insurance (UI). Combining data from the Survey of Income and Program Participation (SIPP) with a novel dataset of state unemployment insurance laws, I am able to estimate unemployment benefit eligibility. I then exploit variation in the potential benefit duration (PBD) arising from the phase-out of extended benefits to investigate the causal effect of additional weeks of UI on reemployment wages. Due to a limited sample size, I am unable to precisely estimate the effect of increased benefit durations on wages. Nevertheless, combining this identification strategy with a large administrative dataset could yield credible estimates of the effect of UI extensions on future earnings.

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1 Introduction

The United States offers some of the least generous UI benefits among OECD countries, especially when compared to nations in western and northern Europe (Schmieder and von Wachter, 2016). In the last decade, many states have reduced the generosity of their benefits even further. In 2000, every state offered at least 26 weeks of benefits for those with sufficient qualifying wages. In 2019, eight states offered benefits for fewer than 20 weeks, with some offering as few as 12 weeks in normal economic conditions.[1]

Determining the optimal level and duration of unemployment benefits is a difficult task. Theoretically, several contravening effects make the welfare implications of expanding unemployment benefits uncertain. Which of these effects dominates can differ across time and space. In this paper, I explore the impact of increasing individuals' potential benefit duration (PBD) on one salient outcome with important implications for welfare: reemployment wages. The magnitude and sign of changes in reemployment wages have important policy implications. The private welfare effect of individuals earning higher wages is clear, but an often overlooked fiscal externality of increased reemployment wages is increased future tax revenue. This additional revenue may partially offset or overwhelm the costs of longer non-employment durations and more generous benefits (Nekoei and Weber, 2017).

The existing literature on this topic has almost exclusively focused on wage effects in Europe, with mixed findings on the impact of increasing benefits on reemployment wages (Le Barbanchon, 2016; Schmieder et al., 2016; Nekoei and Weber, 2017). There are several reasons to believe that UI extensions may have different effects in the United States. For example, that maximum benefit durations are much lower in the US compared to Europe is good reason to believe that the effects of extending individuals' PBD on

[1]Source: US Department of Labor Archived Significant Provisions for January 2000 and January 2019.

reemployment wages may be different in the United States (Schmieder and von Wachter, 2016).

To identify the impact of increases in the PBD on reemployment wages, I exploit variation in eligibility for extended benefits (EB) arising from their phase-out. Extended benefits provide individuals with extra weeks of unemployment benefits in states with abnormally high unemployment rates. Individuals who exhaust their regular unemployment benefits x weeks before the end of an EB period may have their PBD increased by up to x weeks, while those exhausting shortly after the end of an EB period will receive no extra benefits.[2] Within a narrow time window around the end of an EB period, individuals are also likely to face similar labor market conditions, creating a natural experiment to analyze effects on reemployment wages.

The endings of EB periods provide an attractive natural experiment compared to alternative extensions for several reasons. Because EB periods end when the unemployment rate declines to more typical levels, estimates from EB expiration may have greater external validity outside of recessions when compared to estimates relying on the Temporary Extented Unemployment Compensation (TEUC) program of 2002 and Emergency Unemployment Compensation (EUC08) program of 2008. Additionally, by examining exhaustees within a small window of EB endings, I focus on much more local increases in the PBD of 1-8 weeks. Extensions of this magnitude are more likely to have general policy relevance than the massive extensions of up to 53 weeks provided by the 2008 EUC08.

This paper makes two main contributions to the literature on unemployment insurance. I update the unemployment insurance benefit calculator utilized by Chetty (2008), incorporating data on laws from 2000-2019. My version of the calculator also includes more states and territories and greater detail on benefit eligibility requirements.

[2]This is a slight oversimplification, as the extension in benefits is limited by a fraction of the regular benefit amount. More detail will be provided in Section 3.

This calculator can be used to estimate benefit eligibility, weekly benefit amount, and potential benefit duration given information on wage history and employment separations. It therefore has potential for use in settings where researchers observe wages but do not observe benefits or wish to analyze impacts of benefit eligibility rather than receipt.

I also contribute to the literature on the effects of unemployment insurance on reemployment wages. Although a limited sample size prevents me from estimating precise effects on future wages, a large administrative dataset could provide the necessary sample size to employ this identification strategy. This would allow for the credible identification of the effects of benefit extensions on reemployment wages in the United States. Although this limits analysis to partial equilibrium effects, it allows for weaker assumptions than prior work examining reemployment wages in the United States (Farooq et al., 2020).

The remainder of the paper is organized as follows. In Section 2, I review the existing literature on the effects of UI extensions on reemployment wages and discuss the theory that allows for indeterminate effects. Section 3 discusses the institutional details of extended benefits in the United States. Section 4 describes the data on state unemployment laws, EB triggers, and the construction of the core sample from the SIPP. In Section 5, I investigate the validity of the identifying assumption and describe the empirical strategy. Section 6 presents the core results, and Section 7 concludes.

2 Literature Review and Theoretical Framework

There are several theoretical channels through which unemployment insurance may increase reemployment wages. Acemoglu and Shimer (1999) develop a model that predicts risk-averse agents will seek higher-wage work with a higher risk of unemployment when they are insured. Since EB only provides a temporary extension, this is unlikely to be a channel by which reemployment wages are affected in this setting.

Conventional search models predict that UI extensions cause recipients to become more selective and increase reservation wages, while also decreasing search effort (Nekoei and Weber, 2017). Although a higher reservation wage has been seen as an example of moral hazard, Shimer and Werning (2007) develop a model that shows the after-tax reservation wage is a sufficient statistic for the individual welfare effects of unemployment insurance. The decrease in search effort, which arises from a distortion in the relative price of leisure, is the actual moral hazard cost of UI (Chetty, 2008). These countervailing effects may be the explanation for disparate findings on reemployment wage effects. It is a well established empirical finding that longer non-employment durations are associated with lower reemployment wages due to human capital depreciation and stigmatization (Schmieder et al., 2016). If increases in selectivity are small compared to decreases in search effort, the negative wage effects of non-employment duration may outweigh the positive wage effects of selectivity (Nekoei and Weber, 2017). This is consistent with findings in France and Germany that UI extensions had small negative effects on wages and that reservation wages were either unchanged or non-binding. (Schmieder et al., 2016; Le Barbanchon, 2016; Le Barbanchon et al., 2019).

The magnitudes of these effects, and therefore the sign of the average effect, depend on individual heterogeneity, general labor market conditions, and the policy environment (Nekoei and Weber, 2017). For those who are liquidity constrained, the moral hazard costs of UI are smaller (Chetty, 2008). Consistent with this effect, Farooq et al. (2020) find larger increases in reemployment wages for those who are more likely to face liquidity constraints. Worse job market conditions also decrease the moral hazard costs of UI. Multiple papers have shown that the moral hazard costs of unemployment insurance are significantly smaller during recessions (Schmieder et al., 2012; Kroft and Notowidigdo, 2016). In severe recessions, UI may even increase search effort by increasing labor force attachment due to job search requirements (Rothstein, 2011). At the same time, requirements to accept "suitable work" may decrease reservation wage effects.

Although an examination of heterogeneity is critical to understanding the effects on wages, I am unable to explore it in this paper due to sample size limitations. With a larger sample size, an exploration of heterogeneity in treatment affects across demographic groups and in different labor market conditions would be extremely valuable.

3 Institutional Context

3.1 Extended Benefits

Under federal law, states must provide extended benefits during periods of unusually high unemployment. Extended benefits are available to those who have exhausted their regular unemployment benefits and pay a weekly benefit amount equal to the amount paid by a state's regular unemployment insurance program. Eligibility generally follows basic state requirements, although some additional requirements are imposed. Depending on state law, an individual must have either 20 weeks of covered work, 40 times their weekly benefit amount in wages, or earned 1.5 times their high quarter wages during the base period.[3] Usually, individuals receive EB for the lesser of 13 weeks, 50% of an individual's regular benefit duration, and the number of weeks remaining in the period. If the state meets the requirements for the second tier of EB (discussed below), this is extended to the less of 20 weeks, 80% of a person's regular benefit duration, and the number of weeks remaining in the period.

There are several triggers states can use in order to determine the beginning of an EB period. All states are required to trigger on if the insured unemployment rate (IUR)

[3]The base period is the first 4 of the prior 5 completed calendar quarters at the time of unemployment. Some states allow for alternative or extended base periods if wages are insufficient in the regular base period.

for the prior 13 weeks exceeds 5% *and* is at least 120% of the average rate for the same 13 week period in the prior two years.[4] States also have the option of adopting two additional triggers into their unemployment laws. States may trigger on if the IUR for the prior 13 weeks reaches 6%, irrespective of unemployment conditions in past years.[5] Finally, states can also trigger on if the total unemployment rate (TUR) for the prior 13 weeks exceeds 6.5% *and* is 110% of the TUR for the same period in *either* of the past 2 years.[6] States that adopt the TUR trigger must also offer a second tier of EB, paying up to 20 weeks of benefits if the TUR exceeds 8% and the 110% "look-back" requirement is also satisfied.

While the triggers are necessary conditions for a state to begin or end an EB period, they are not sufficient. Once a state experiences a week in which the trigger conditions are satisfied, the EB period will generally begin the first day of the third calendar week thereafter. However, states cannot begin an EB period within 13 weeks of the end of another EB period. Thus, the start of the EB period will be delayed until the state has been "off" for 13 weeks. Similarly, a state will typically end an EB period on the last day of the third week after one of the trigger conditions is no longer satisfied. However, an EB period, once started, must last for at least 13 weeks. The end of the EB period will therefore be delayed until the state has been "on" for at least 13 weeks.

The nature of extended benefit periods makes it difficult to model an individual's anticipated PBD at any given point in their spell. Those who become unemployed before an EB trigger switches on will not know whether an EB period will be active during their spell. Furthermore, once a period begins, it is not clear how long it will last, meaning individuals do not know how many weeks of EB will be available (if any) after they

[4]During the Great Recession, states were allowed to modify their look-back provisions to cover the prior three years, as the high unemployment rates of the previous years could make reaching 120% of the average IUR for the past two years difficult.

[5]As of 2019, 30 states have adopted the 6% trigger.

[6]Only 11 states have adopted the TUR trigger as of 2019.

exhaust their regular benefits. Unlike in the case of the EUC of the Great Recession, there is no predetermined legislative end date of extended benefits. Thus, I cannot follow the solution used by Rothstein (2011), which takes advantage of EUC08 expiry dates at the time of the individual's interview to model anticipated benefit duration. As a result, I simply assign individuals' potential EB eligibility as treatment. Figure A3 in the Appendix illustrates how treatment is assigned based on the benefit exhaustion date.

This concern is somewhat assuaged by the fact that within my core sample (those exhausting regular benefits within eight weeks of the end of an EB period), individuals will have a better sense of their PBD. Those exhausting their regular benefits eight weeks prior to the end of an EB period experience at most 18 weeks of unemployment outside the EB period, with only 16 of those coming before knowledge than an EB period would soon begin.[7] Those who exhaust their regular benefits shortly after the end of EB will know for certain their PBD will not be extended, although this information comes only 2-10 weeks before the exhaustion of regular benefits. This lack of information probably has small impacts on take-up, as Anderson and Meyer (1997) show that the elasticity of take-up with respect to benefit duration is small. Nevertheless, bias in my estimates is likely insofar as individuals are unable to predict their PBD throughout their spell. It is unclear in which direction this type of measurement error biases my results (Rothstein, 2011).

3.2 Special Federal Extended Benefit Programs

Though not directly relevant for this analysis, a basic understanding of the 2002 TEUC and 2008 EUC08 is important given many EB periods coincide with these programs.

The TEUC was a two-tiered program. All states qualified for the first tier, which

[7]This is a worst-case scenario assuming an EB period of only 13 weeks. Longer EB periods, which most are, will leave the individual with more information. A period of 34 weeks is enough to ensure the job separation date occurred during an EB period.

provided 13 weeks of supplemental unemployment benefits. States with with a 3-month average unemployment rate above 6.5% qualified for the second tier, which offered an additional 13 weeks of benefits (Farooq et al., 2020).

The EUC08 evolved significantly over time, and a detailed explanation of its history goes beyond the scope of this paper. At its height, the EUC08 was a four-tiered program, with the first two tiers offering 34 weeks of benefits in all states. The third tier was triggered if the unemployment rate was above 6%, allowing for an additional 13 weeks of benefits. States entered the fourth tier when their unemployment rates exceeded 8.5%, which allowed individuals to claim an additional 6 weeks of benefits. Altogether, the program allowed for an additional 53 weeks of benefits. For greater detail on the evolution of the EUC08 benefit provisions, see Appendix C.

Since the tiers of the TEUC and EUC08 are tied to the unemployment rate in a similar manner to EB, a potential concern is that these programs are a source of omitted variable bias. Fortunately, this concern is alleviated by how I assign treatment and structure of these programs. I only look at spells in which individuals exhaust their regular benefits within a neighborhood of an EB end date, and individuals must be ineligible for EB before claiming from federal extended benefit programs. Thus, exhaustees will first draw from those weeks of benefits first. They will then be identically situated to those who exhaust shortly after the end of the EB period with respect to eligibility for TEUC or EUC08 benefits, meaning the variation in duration I exploit is unrelated to variation in the PBD arising from the TEUC and EUC08.

4 Data and Descriptive Statistics

4.1 State Unemployment Laws

I gather most of my data on state unemployment laws from the Employment and Training Administration's (ETA) Archived Significant Provisions of State UI Laws, which are published semiannually. These provide basic information on eligibility criterion, benefit formulas, duration formulas, maximum benefit amount, and maximum duration. When these are in any way inconsistent or unclear, I utilize the more detailed information contained in the ETA's Archived Comparisons of State Laws, which are published annually. In the event that the Archived Comparisons do not resolve the ambiguity or inconsistency, I use both historical and current state statutes. When possible, I take these from the websites of state legislatures. When states do not make their archived laws available, I use versions from Justia.com and Casetext.com. Combined with income histories from the SIPP, I am able to compute monetary eligibility, weekly benefits, and potential benefit duration for all job losers from 2000-2019. For a sample comparison of key provisions in state unemployment laws, see Appendix C.

4.2 Extended Benefit Triggers

I obtain information on EB trigger dates from ETA Form 539, which is publicly available.[8] The form provides weekly data on unemployment claims, the insured unemployment rate, whether a state is currently in an EB period, and the date the state changed to its current EB status. These triggers do not specify whether a state is paying 13 or 20 weeks of EB, but this is not a significant issue given I am looking at smaller increases the PBD.[9] I reduce this dataset to a unique list of state-periods, so that each observation

[8]The form can be downloaded from this site: https://oui.doleta.gov/unemploy/DataDownloads.asp

[9]Assuming a 13 week EB period only affects the extra benefits of four individuals in the core sample.

represents a single EB on/off period.

Figure 1 presents some basic descriptives for EB periods ending from 2000-2019. Panel (a) shows the distribution of EB period durations. A majority of periods are longer than the maximum, which alleviates some of the concern surrounding anticipation of EB receipt since most treated individuals will lose their jobs during the EB period. Panel (b) shows the distribution of years in which EB periods ended. Unsurprisingly, the vast majority of EB periods in the sample ended near the end of the Great Recession. Panel (c) shows the number of states with an EB period active by week from 2000-2019. This illustrates the rarity of EB periods outside of the Great Recession, with the smaller 2001 recession also causing a small bump in states offering EB.

4.3 SIPP

I obtain data on individual unemployment and earnings from the pooled 1996, 2001, 2004, 2008, 2014, and 2018 panels of the Survey of Income and Program Participation (SIPP), which I download from the NBER and the US Census Bureau websites.[10] The SIPP is useful due to its detailed weekly data on employment status, longitudinal structure, and relatively large sample size. I construct a dataset containing all unemployment spells from 2000-2019 in the SIPP following Chetty (2008) with a few adjustments.[11] Like Chetty (2008), I restrict to those of working age (18-65) with at least 3 consecutive months of work history in the panel so wages can be imputed. I also remove non-consecutive interviews, since these result in missing work and earnings history. Unlike Chetty (2008), I do not restrict my sample to men. I also do not drop individuals who do not report searching for a job or report a temporary layoff, primarily due to sample size constraints.

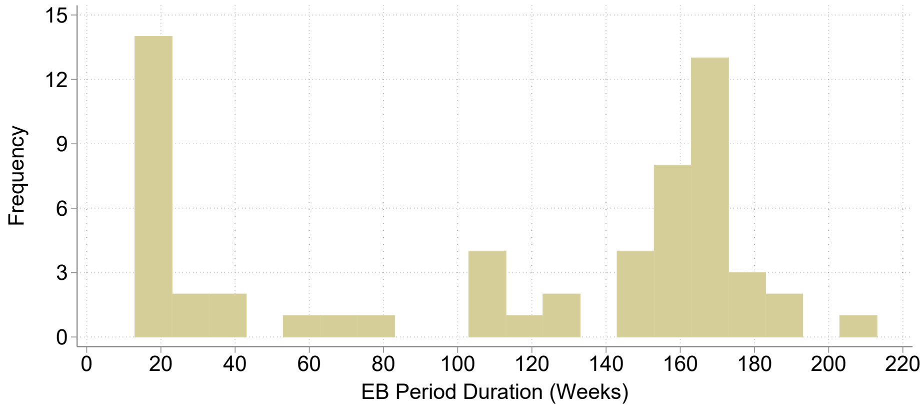
I then use my data on state unemployment insurance laws to estimate weekly benefit

[10] I modify extraction code from CEPR to access the 1996-2008 panels.

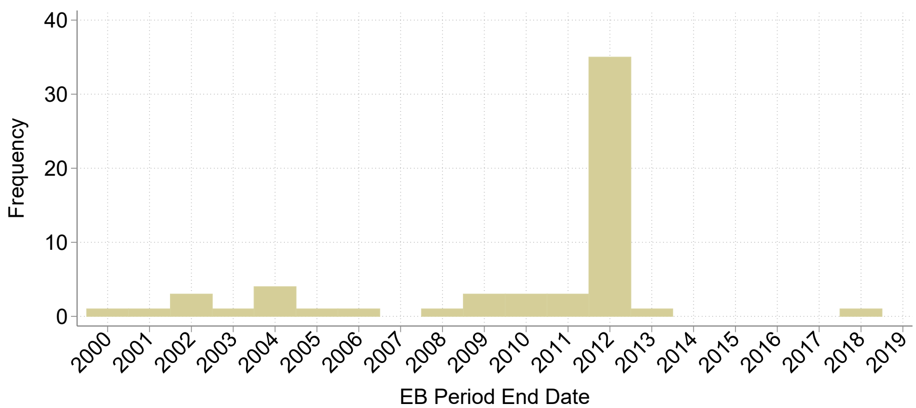
[11] In particular, the core of my data build is heavily inspired by the code in the replication package of Chetty (2008).

Figure 1: EB Period Descriptives

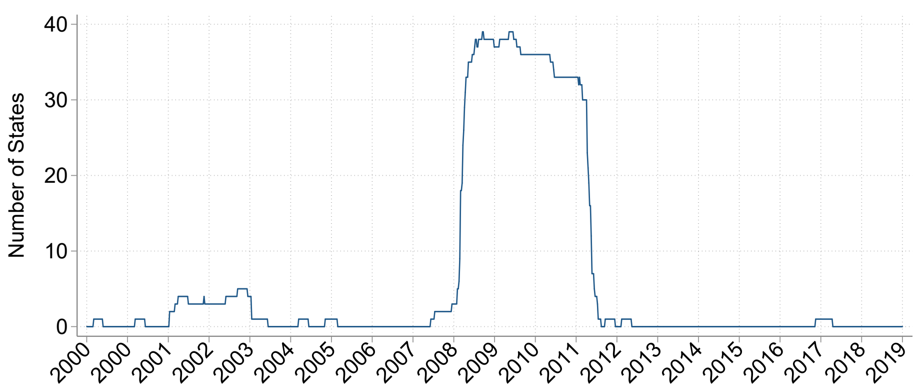
(a) Distribution Extended Benefit Period Length



(b) Distribution of EB Period End Dates



c: Number of States offering EB



Source: Author's calculations from ETA Form 539. The above figures describe some basic characteristics of EB periods ending between 2000-2019. All are generated using data from EB periods in the 50 states and District of Columbia.

amounts, eligibility for regular benefits, and eligibility for EB. I drop all individuals who are ineligible to receive EB based on their earnings history. Since I am interested in the impact of potential benefit duration rather than actual benefit take-up, I do not restrict to a sample of those who take-up benefits. Thus, in a similar vein to Rothstein (2011), I assume every individual is eligible for the benefits I calculate.

Finally, I assemble the core sample. Using the information on EB triggers, each spell is assigned to the EB on/off period in which the predicted regular benefit exhaustion date falls. To limit to those who face plausibly similar labor market conditions, I restrict to those who exhaust their benefits within eight weeks of the end of an EB period. This decreases the sample size from 80,514 spells to just 1,645. Finally, I limit my analysis to those whose with observed reemployment hourly wages, leaving 873 spells and 873 individuals in the core sample. Individuals who report wages below the federal minimum wage are winsorized to the federal minimum wage.[12]

Those without information on wages are overwhelmingly individuals facing significantly longer non-employment durations, and thus likely have lower reemployment wages.[13] Rates of missing wages due to right-censored unemployment spells are similar across the treatment and control groups.[14] As a result, this is unlikely to result in any bias; however, it does mean that my ability to speak to wage impacts on the long-term unemployed is limited.

The SIPP data has other important limitations to consider. I cannot observe whether individuals' earnings come from covered employment or potential causes of non-monetary ineligibility such as misconduct. Additionally, a significant amount of imputation on prior wages is necessary for spells occurring in the first few quarters of each panel. This likely causes me to overestimate eligibility for benefits, which in turn

[12]This affects only 15 individuals in the core sample.

[13]Those excluded due only to missing earnings information have an average non-employment duration of 35.8 weeks in the panel, compared to just 15.2 weeks for those in the core sample.

[14]47.1% of the treatment group is lost due to missing wages, compared to 46.8% in the control group.

will bias my estimates towards zero.

Restricting to spells with multiple months of employment history after they end is infeasible, as many spells do not have many observations following reemployment. Thus, I only analyze an individual's hourly wage in the month after their unemployment spell ends. As a result, I am unable to make conclusions about long term effects on wages.

5 Empirical Strategy

To identify the effect of additional weeks of unemployment benefits on reemployment wages, I rely on variation in potential benefit duration induced by extended benefit periods. The identifying assumption is that the number of weeks of EB an individual is eligible for is "as good as random". In other words, the treatment must be independent from potential outcomes, conditional on covariates (Angrist and Pischke, 2013). In this context, this assumption demands that treated and untreated individuals be similar in relevant characteristics and face comparable labor market conditions, conditional on covariates. Choosing the window around EB end dates to restrict the sample to is a balancing act between preserving the validity of conditional independence and power: a larger window will increase the sample size, but also increase the variation in labor market conditions faced by exhaustees. Due to the significant sample size issues I face, I opt for a relatively large bandwidth of eight weeks. Despite this large bandwidth, the sample size is still significantly limited and my results are under powered. As a result, I do not test smaller bandwidths.

To test the assumption of conditional independence in the core sample, I compare the distribution of exogenous and pre-treatment observables between the treatment and control groups. I employ two strategies to do so. First, I compare the summary statistics of exogenous variables between the treatment and control groups. Table 1 below presents summary statistics for the core sample. The averages and medians of key variables prior to job loss are very similar across the treatment and control groups. Individuals in both groups are also entitled to similar weekly benefit amounts and regular benefit durations, and face similar unemployment rates at benefit exhaustion. While this does not guarantee there is no unobserved heterogeneity in characteristics or labor market conditions across the different groups, it is evidence that treatment is exogenous.

As a second check on the identifying assumption, I create binned scatter plots for several covariates. These plots calculate means for each distance from exhaustion to the EB end date, ranging from 8 weeks before to 8 weeks after the EB end date. Binned scatter plots are commonly utilized in RDD settings to test whether relevant covariates jump at the point of discontinuity. Although I do not employ an RD design, the number of weeks from exhaustion to the EB end date functions similarly to a running variable, with treatment beginning with those who exhaust their regular benefits 1 week before an EB period ends. As is standard in the literature, I fit a quadratic function of the "running variable" and allow it to vary on each side of the "discontinuity". Moderate jumps are to be expected given the number of weeks before an EB period ends is discrete, so there is no data close to the discontinuity. Additionally, each value will only have around 50 observations. However, large jumps at the point of benefits or large differences in covariate means across different values of the "running variable" would suggest that the identifying assumption is invalid.

Figure 2 below presents the most relevant binned scatter plots. Panels (a)-(d) are the plots for nominal base period wages, years of education, real weekly benefit amount, and the unemployment rate at the time of regular benefit exhaustion respectively. None of the panels exhibit large discontinuities at the point of treatment. Panel (b) displays very stable trends over time. There is a starker difference in the fitted quadratics for nominal base period wages and real weekly benefit amount. However, examining

Table 1: Summary Statistics by Treatment Status for Core Sample

	Pooled	Treatment Status	
		Treatment (1)	Control (2)
Prior to or at Job Loss			
Mean Base Period Wage (Nominal)	\$22,525	\$23,026	\$22,111
Median Base Period Wage (Nominal)	\$17,632	\$18,438	\$17,128
Tenure in Base Period (Weeks)	46.4	46.3	46.6
Age	36.3	36.2	36.4
Years of education	12.4	12.5	12.3
Percent White	83%	85%	81%
Percent Female	43%	48%	40%
After Layoff			
Weekly Benefit Amount (2015 USD)	\$253	\$262	\$246
Maximum Regular Duration (Weeks)	24.0	23.9	24.1
Mean Unemployment Duration (Weeks)	15.2	15.7	14.8
Median unemployment duration (Weeks)	9.0	9.0	9.0
Unemployment Rate at Exhaustion	8.7	8.6	8.7
Reemployment Wages (Hourly, 2015 USD)	\$14.20	\$14.27	\$14.13
Number of Observations	873	397	476

Source: These statistics are generated using SIPP data and calculated UI benefits for the core sample. The unemployment rate is the seasonally adjusted state unemployment rate in the month of regular benefit exhaustion taken from the Bureau of Labor Statistics. All values are calculated using individual monthly sample weights and are means unless otherwise stated. Aside from gender, the exogenous pre-treatment covariates are similar across the treatment and control groups. Base period wages are presented using nominal dollars because they are calculated using earnings over multiple years. There are more individuals in the treatment group because who exhaust the same week the EB period ends are in the control group. This table is heavily inspired by Table 1 in Chetty (2008).

the points on the scatter plot rather than the fitted values suggests that there isn't a particularly strong trend in these variables across different levels of treatment. Panel (d) demonstrates that the unemployment rate at exhaustion is higher earlier in the benefit period, which is unsurprising. This illustrates the importance of controlling for differences in labor market conditions as much as possible. Ideally, I could incorporate other state-level macroeconomic indicators such as gross state product, but these tend to be reported at the quarterly level and thus aren't informative when looking at spells within a short period. Binned scatter plots for additional variables, including outcomes, can be found in Appendix A.

Having done my best to validate the assumption of conditional independence, I turn to the actual estimation. All the models that I estimate utilize the core sample. As a quick sanity check, I examine the relationship between treatment on non-employment duration, as well as the association between non-employment duration and reemployment wages. In line with the existing literature, I find a positive association between treatment and non-employment duration, as well as a negative association between non-employment duration and wages. Details on this estimation can be found in Appendix B.

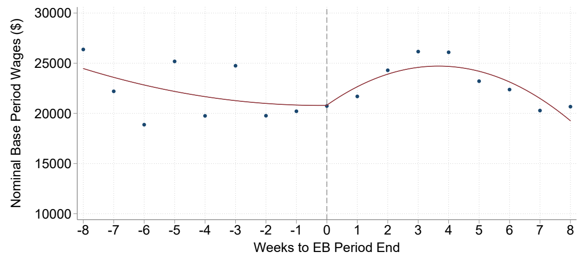
I estimate the effects of increases to the PBD on reemployment wages in two ways. In the first, I consider treatment to be a binary variable, taking on a value of 1 when an individual is eligible for extended benefits and 0 when an individual is not. My preferred specification for this model is as follows:

$$log(y_{ipst}) = \alpha + \beta 1(x_i = 1) + \delta_1 UR_{st} + \delta_2 (UR_{st})^2 + \gamma_p + \epsilon \tag{1}$$

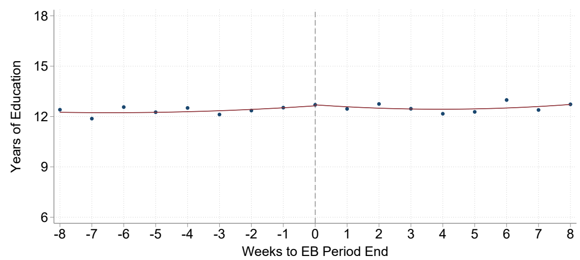
where y_{ipt} are real hourly wages in the first complete month of reemployment for individual i living in state s who exhausts their regular benefits in month t within an eight week window around the end of EB period p. $1(x_i = 1)$ is an indicator for whether

Figure 2: Binned Scatter Plots of Relevant Covariates

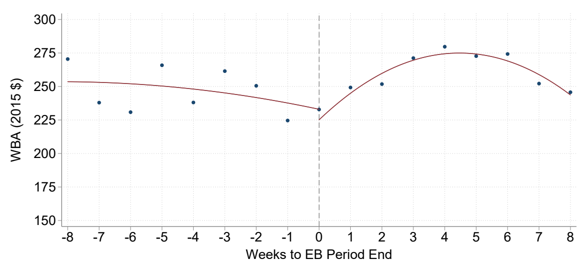
(a) Nominal Base Period Wages



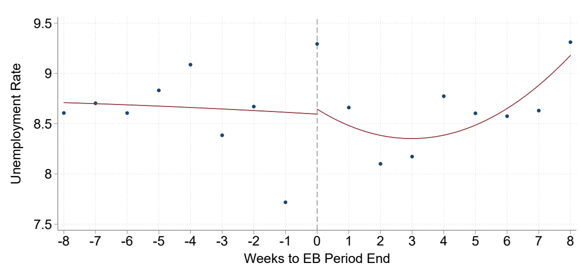
(b) Years of Education



(c) Real WBA (2015 USD)



(d) Maximum Regular Benefit Duration



Source: This data is generated using SIPP data and calculated benefit amounts for the core sample. Binned averages are weighted using individual monthly sample weights. These plots demonstrate that there are no strong trends between treatment and nominal base period wages, years of education, or real weekly benefit amount. The unemployment rate is relatively constant near the end of EB periods, but is much higher earlier in the EB period. This indicates the importance of controlling for state labor market conditions.

person i is eligible for any EB. γ controls for fixed effects associated with a particular benefit period such as the trigger that activated it and labor market conditions that are constant 8 weeks on either side of the EB end date. UR is the monthly unemployment rate, of which I include a quadratic to control for evolving labor market conditions. Finally, ϵ is the error term.

In the above model, the coefficient of interest β can be roughly interpreted as 1/100 of the average percentage change in real hourly reemployment wages as a result of potential eligibility for any amount of EB.

In the second class of models, I consider treatment to be continuous, taking on values from $0-8$ depending on the number of weeks of EB a person is eligible for. The model then becomes

$$log(y_{ipst}) = \alpha + \beta Weeks\_EB_i + \delta_1 UR_{st} + \delta_2 (UR_{st})^2 + \gamma_p + \epsilon \tag{2}$$

where $Weeks\_EB$ is the number of weeks of EB weeks person i is eligible for (ranging from $0-8$) and all other variables have already been defined.

In the above model, the coefficient of interest β can be roughly interpreted as 1/100 of the average percentage change in real hourly reemployment wages as a result of a 1 week increase in the PBD arising from EB eligibility.

6 Results

Table 2 below presents my estimates for the core analysis. Panel A shows the results from an OLS regression of reemployment hourly wages on binary treatment with period fixed effects, and Panel B provides estimates with continuous treatment. Columns 1-3 present estimates based on logged hourly wages, while columns 4-6 present estimates based on levels of hourly wages for the sake of thoroughness. In particular, column 3 shows the estimates for my preferred specifications which include a quadratic control for the unemployment rate.

In all of the specifications the estimates are imprecise, with standard errors several times larger than the coefficients. As a result, the confidence intervals cover the full range of economically plausible treatment effects.[15] I therefore cannot make conclusions regarding the sign or magnitude of the effect of treatment on hourly reemployment wages, and I cannot rule out any economically interesting values of the marginal effect of treatment overall or additional weeks on reemployment wages.

7 Conclusion

In this paper, I investigate the causal effect of unemployment insurance extensions on reemployment wages in the United States. I do so by exploiting plausibly exogenous variation in potential benefit duration arising from the phase-out of extended benefits (EB). Variation induced by EB phaseout is attractive compared to alternative benefit extensions since individuals are compared to others exhausting benefits at similar times in the same state. As a result they face much more comparable labor market conditions and assumptions equating unobserved labor market conditions are more likely to hold. Unfortunately, sample size limitations prevent me from estimating the sign or magnitude of the wage effect. Despite the SIPP's relatively large sample size when compared to other surveys, the data demands of a design restricting to individuals exhausting benefits within a 2-4 month window in a particular state are high. Even with a large sample size, the nature of the SIPP leads to many right-censored non-employment spells without reemployment wages. This limits the applicability of results to the long-term unemployed.

[15] Based on other estimates in the literature, wage effects should be fractions of a percent (Schmieder et al., 2016; Nekoei and Weber, 2017).

Table 2: Effects of EB on Reemployment Wages

Panel A: Effects on Reemployment Wages: Binary Treatment

	Log Hourly Wages			Levels of Hourly Wages		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0811	0.122	0.0818	0.504	1.456	0.789
	(0.290)	(0.294)	(0.306)	(4.274)	(4.305)	(4.507)
Unemployment Rate		0.0264	-0.0840		0.618	-1.225
		(0.0400)	(0.224)		(0.621)	(3.462)
(Unemployment Rate)^2			0.00614			0.102
			(0.0124)			(0.192)

Panel B: Effects on Reemployment Wages: Continuous Treatment

	Log Hourly Wages			Levels of Hourly Wages		
	(1)	(2)	(3)	(4)	(5)	(6)
Weeks of EB	-0.00220	-0.00221	-0.00271	-0.0665	-0.0666	-0.0753
	(0.0106)	(0.0105)	(0.0106)	(0.180)	(0.179)	(0.180)
Unemployment Rate		0.0264	-0.0907		0.619	-1.413
		(0.0400)	(0.224)		(0.620)	(3.462)
(Unemployment Rate)^2			0.00651			0.113
			(0.0124)			(0.192)
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	873	873	873	873	873	873

Standard errors in parentheses

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

Notes: The above table presents results from OLS regressions of hourly reemployment wages on both binary and continuous treatment using the core sample. Observations are weighted using individual monthly survey weights. Coefficients for period fixed effects and the intercept are not included. Standard errors are robust to White heteroskedasticity. Panel A utilizes a binary treatment variable, while Panel B considers treatment to be continuous. Columns 1-3 present estimates of effects on logged hourly wages, while columns 4-6 present estimates of effects on levels of hourly wages. Estimates for my preferred specification (as described in Section 4) can be found in column 3.

Nevertheless, this paper creates several opportunities for future research. With access to a large administrative dataset, I could potentially obtain the necessary sample size to precisely estimate impacts on reemployment wages using this identification strategy. Although this would likely come at the cost of the SIPP's detailed weekly data on employment status, monthly employment status is granular enough to determine binary treatment status for most spells. However, this strategy would still be limited by the fact that individuals cannot usually predict the precise end date of an EB period, meaning their "actual" PBD may not align with their anticipated PBD. This information gap provides the opportunity for theoretical or experimental study of how individuals dynamically update their anticipated PBD based on available information throughout their unemployment spells.

Finally, the unemployment insurance calculator I develop can assist researchers studying UI but who do not observe UI receipt or are interested in eligibility rather than take-up. This has applications to the study of kinks in benefit schedules, changes in maximum durations, and other important UI parameters.

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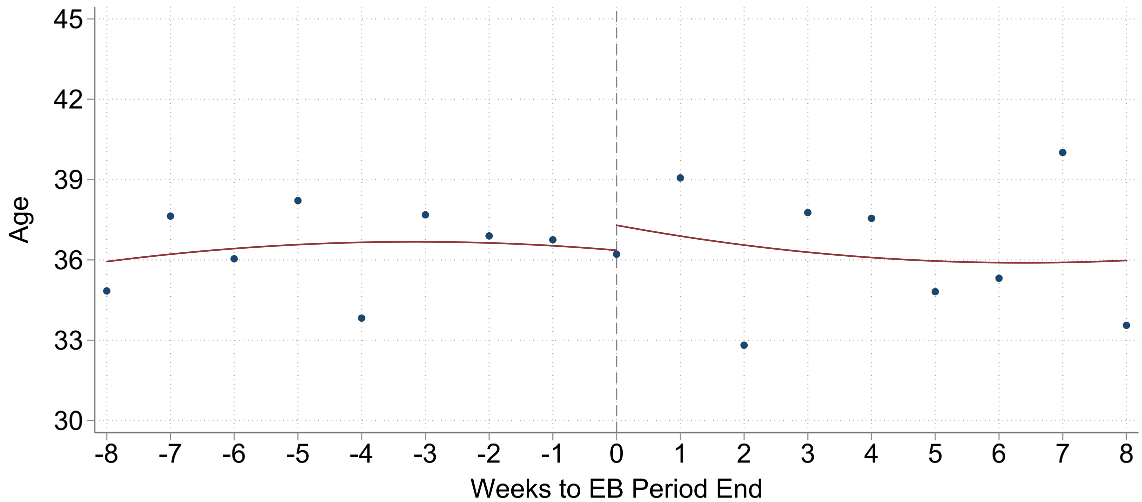
Appendix Materials

Contents

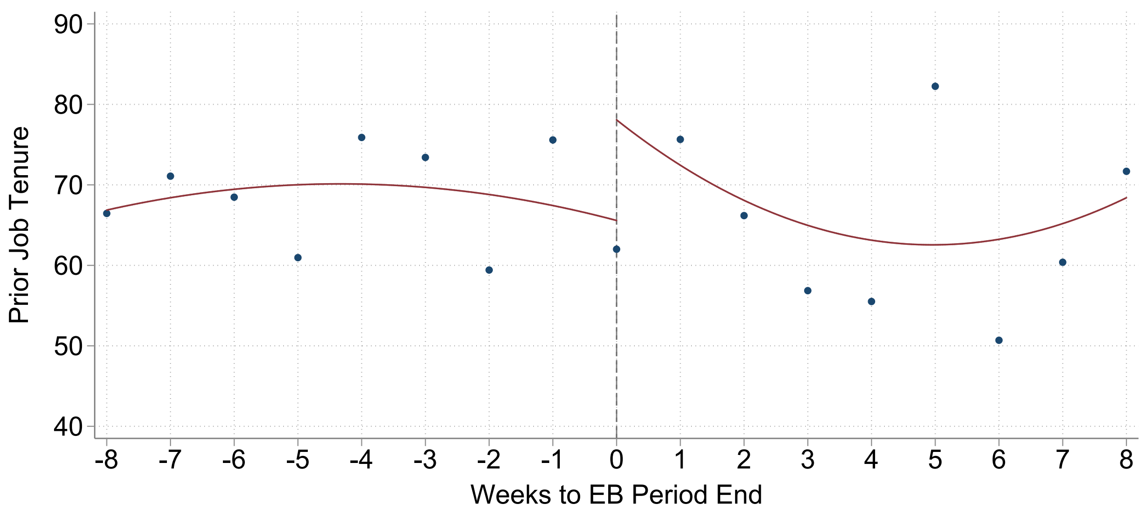
A Additional Binscatters

Figure A1: Additional Binned Scatters: Covariates

(a) Age



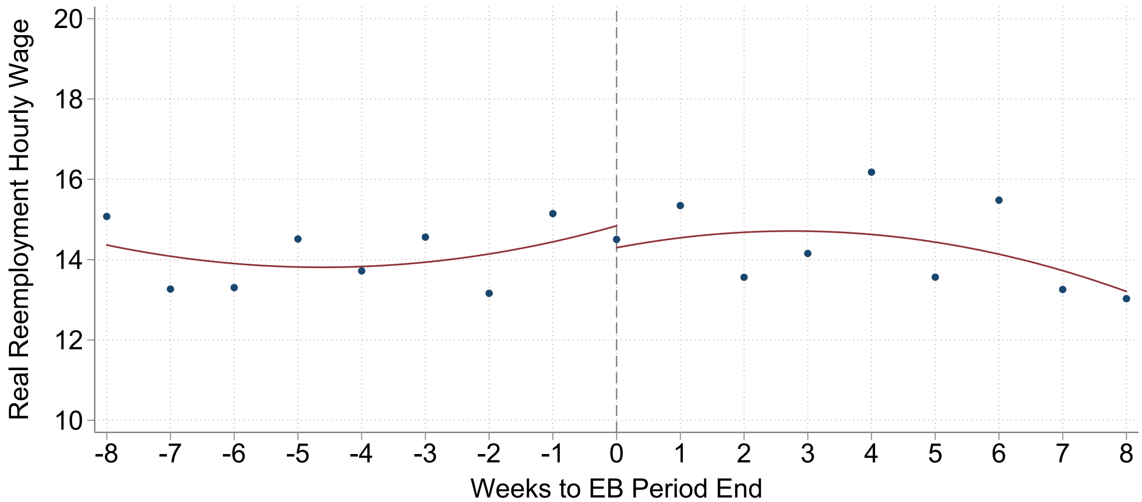
(b) Prior Job Tenure



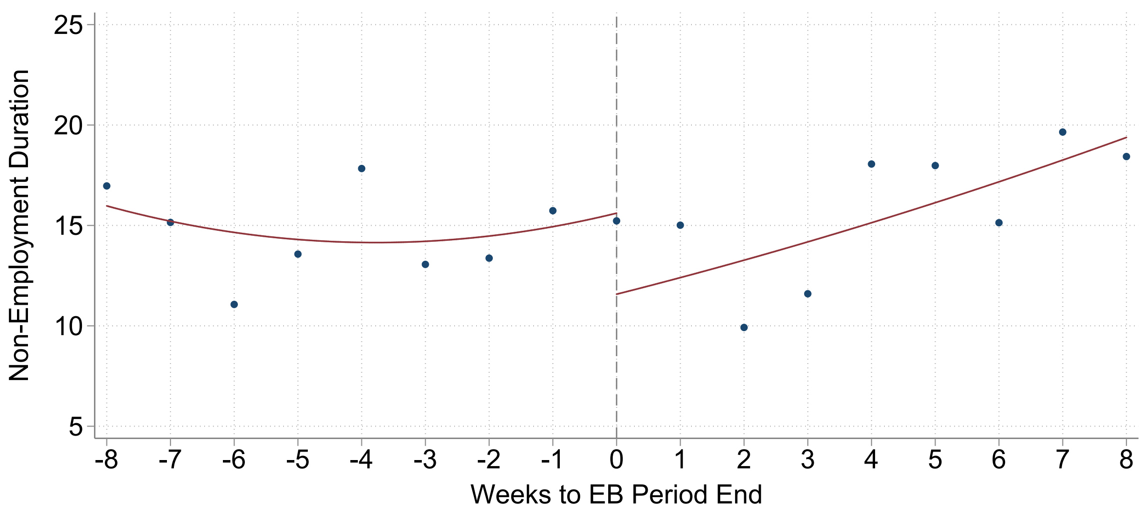
Source: This data is generated using SIPP data and calculated benefit amounts for the core sample. Binned averages are weighted using individual monthly sample weights. Age is relatively constant across different levels of the running variable. Job tenure is more noisy, though there is not a trend of uniformly longer/shorter tenures between the treatment and control groups.

Figure A2: Additional Binned Scatters: Outcomes

(a) Reemployment Wages



(b) Non-Employment Duration



Source: This data is generated using SIPP data and calculated benefit amounts for the core sample. Binned averages are weighted using individual monthly sample weights. The above figures are binscatters for two notable outcomes: (a) reemployment wages and (b) non-employment duration. Note that these figures do not control for period fixed effects or labor market conditions, and are therefore unreliable for surmising causal effects.

B Additional Analysis: Treatment, Duration, and Reemployment Wages

Table A1 below reports the results from OLS regressions of non-employment duration (in weeks) on treatment with period fixed effects. Panel A presents results for binary treatment, while Panel B presents results for continuous treatment. Column 3 contains results for my preferred specification, which includes a quadratic of the unemployment rate to control for evolving labor market conditions. As expected, I find a positive association between extended UI duration and non-employment duration, with a statistically significant positive association in my preferred specification with a continuous treatment. However, the point estimate is not precisely estimated.[16] Although the identification strategy may be valid for inference on duration, I do not posit these to be estimates to be causal.

Table A2 reports the results from OLS regressions of hourly reemployment wages on non-employment duration with period fixed effects. Columns 1-3 present results using logged wages, while columns 4-6 present results using levels of hourly wages. Again, column 3 utilizes my preferred specification. In all specifications, I find small, precisely estimated, negative associations between non-employment duration and reemployment. In my preferred specification, I find that an additional week of non-employment duration is associated with an approximate .2% decrease in hourly reemployment wages. This negative association is consistent with the existing literature (Schmieder and von Wachter, 2016). These estimates should not be viewed as causal since non-employment duration is endogenous.

[16] The 95% confidence interval for the association between an additional week of EB eligibility and non-employment duration in weeks ranges from .07 to 1.9.

Table A1: Relationship Between EB and Non-Employment Duration

Panel A: Binary Treatment

	(1)	(2)	(3)
Treatment	2.374	3.897	12.17
	(13.30)	(13.52)	(13.85)
Unemployment Rate		0.990	23.85*
		(1.822)	(12.38)
(Unemployment Rate)^2			-1.271*
			(0.695)

Panel B: Continuous Treatment

	(1)	(2)	(3)
Weeks of EB	0.897*	0.897*	1.006**
	(0.473)	(0.473)	(0.476)
Unemployment Rate		0.987	26.36**
		(1.834)	(12.45)
(Unemployment Rate)^2			-1.410**
			(0.700)
Period FE	Yes	Yes	Yes
Observations	873	873	873

Standard errors in parentheses

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

Notes: The above table presents results from OLS regressions of non-employment duration on EB eligibility using the core sample. Observations are weighted using individual monthly survey weights. Coefficients for period fixed effects and the intercept are suppressed. Standard errors are robust to White heteroskedasticity. Panel A presents results treating treatment as binary, while Panel B presents estimates with continuous treatment. Estimates for my preferred specification, which includes a quadratic control for the unemployment rate, are in column 3. I find an insignificant, positive association between treatment and non-employment duration, with a statistically significant, albeit imprecise estimate for my preferred specification.

Table A2: Relationship Between Reemployment Wage and Non-Employment Duration

	Log Hourly Wages			Levels of Hourly Wages		
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Employment Duration (Weeks)	-0.00274***	-0.00276***	-0.00274***	-0.0468***	-0.0471***	-0.0469***
	(0.000788)	(0.000790)	(0.000794)	(0.0123)	(0.0124)	(0.0124)
Unemployment Rate		0.0291	-0.0186		0.665	-0.106
		(0.0400)	(0.223)		(0.620)	(3.434)
(Unemployment Rate)^2			0.00265			0.0429
			(0.0124)			(0.190)
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	873	873	873	873	873	873

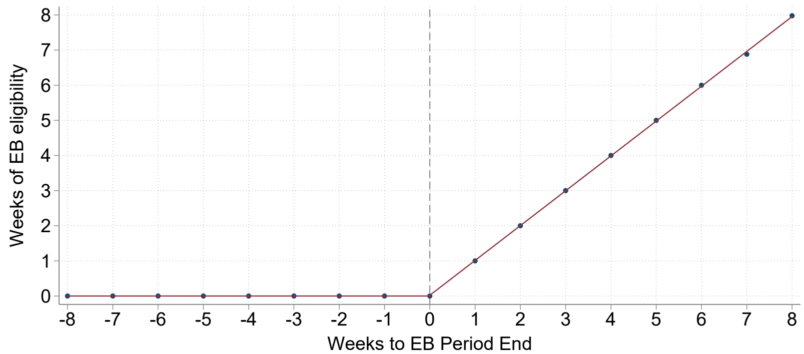
Standard errors in parentheses

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

Notes: The above table presents results from OLS regressions of hourly reemployment wages on non-employment duration using the core sample. Observations are weighted using individual monthly survey weights. Coefficients for period fixed effects and the intercept are suppressed. Standard errors are robust to White heteroskedasticity. Columns 1-3 present estimates for logged hourly wages, while columns 4-6 present estimates for levels of hourly wages. I find a statistically significant, negative relationship between non-employment duration and reemployment wages in all specifications. Estimates for my preferred specification are in column 3.

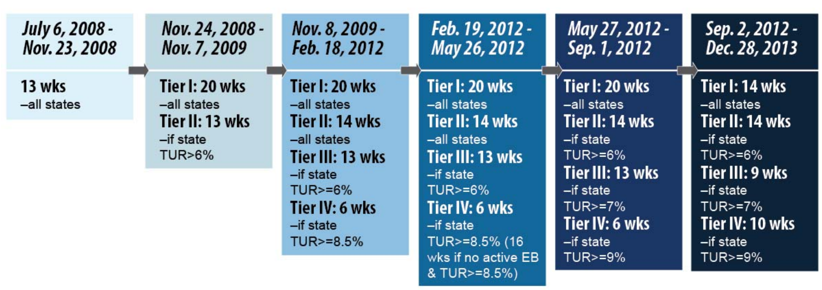
C UI Law Details

Figure A3: Assignment of Treatment



Source: Author's calculations. This figure demonstrates how the number of weeks remaining in an EB period at the time of regular benefit exhaustion translates into weeks of EB eligibility. The fitted line is not perfectly linear because some individuals' EB is capped by 50% of their regular benefit duration.

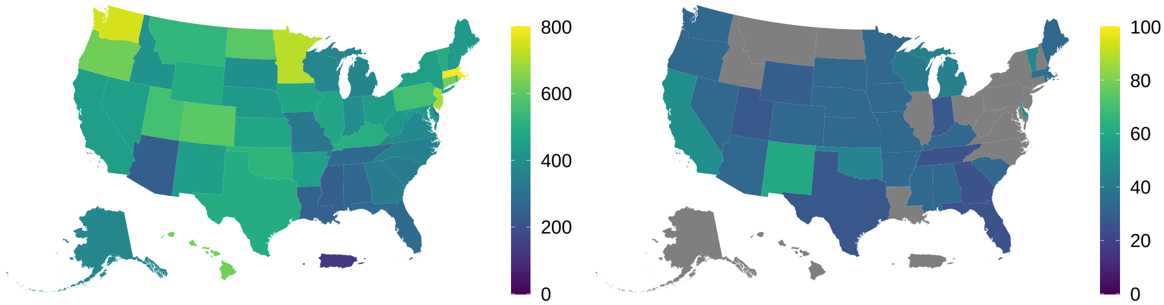
Figure A4: Details of EUC08 Benefit Extensions



Source: This figure is taken from the Congressional Research Service (Isaacs and Whittaker, 2020). It describes the evolution of additional benefits offered by the EUC08 program.

Figure A5: Significant Provisions of State UI Laws in 2019

(a) Maximum Weekly Benefit Amounts by State (b) Maximum Total Benefit Rates by State



Source: ETA's Archived Significant Provisions of State UI Laws for January 2019.
Panel (a) of the above figure shows the maximum weekly benefit amount (WBA) available to individuals as of January 1, 2019, excluding any dependent allowances. Panel (b) compares maximum potential benefits as a percentage of the base period wage. Gray states do not determine maximum total benefits as a percentage of base period wages. Some states rely on the ratio of base period to high quarter wages, while other states uniformly offer a maximum number of weeks. Values are presented for each of the 50 states, the District of Columbia, and Puerto Rico.