The Effects of Unemployment Insurance Extensions on Labor Market Transitions

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Author Note

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Abstract

Did the extensions of the unemployment insurance program during the COVID-19 pandemic contribute to the current labor force shortage? In this paper, I estimate a multinomial logistic regression and found some yet limited evidence that as an individual stays longer in unemployment, the additional amount and duration of unemployment benefits reduce the individual's likelihood of exiting unemployment. This drop in exit hazard shall be attributed to people postponing re-employment, highlighting the economic inefficiencies resulting from these extensions.

Keywords: unemployment insurance, pandemic, duration dependence, labor market transition, multinomial logistic regression

JEL classification: J64; J65

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1. Introduction

During the COVID-19 global pandemic and the synchronous economic recession, many workers in the U.S. became unemployed. According to the U.S. Bureau of Labor Statistics (BLS), the unemployment rate rose to as high as 14.7% in April 2020[1] (figure 3). In response, the Federal Government rolled out several pandemic-relief policies, including the Pandemic Emergency Unemployment Compensation (PEUC) and Federal Pandemic Unemployment Compensation (FPUC), which extended the maximum potential duration and amount of unemployment insurance (UI) to support the people in unemployment. However, as the country recovers from the pandemic economically, it is widely reported that the U.S. is experiencing a labor shortage. Unemployment benefits subsidize continued unemployment. Thus, it seems likely that the vast UI extensions have contributed to some degree to the labor shortage.

Whether or not the large-scale extensions of unemployment insurance (UI) benefits during the pandemic have contributed to the observed labor shortage is an important policy question. There is abundant literature on the effects of UI benefits on workers' job search behavior; nonetheless, the magnitude and interpretation of this effect are still unclear. Namely, there exist two primary channels for UI to affect unemployment: on the one hand, UI benefits can lead recipients to reduce their search effort and raise their reservation wage, slowing the transition into employment. On the other hand, these benefits, available only to those engaged in

[1] https://www.bls.gov/charts/employment-situation/civilian-unemployment-rate.htm

active job search, provide an incentive for the continued search for those who might otherwise exit the labor force. This paper adds to the economic literature on the role of the Unemployment Insurance extensions during the Covid-19 pandemic in an unemployed individual's labor market decisions.

By estimating a multinomial logistic model, I found evidence that the UI extensions reinforce the duration dependence characteristic of unemployment spells. As unemployed individuals stay longer in their unemployment spell, they are more likely to remain unemployed when extra benefits are available. I also contribute to the growing literature that debates the magnitude of the two channels mentioned above by showing that the fall in exit hazard that I documented can be attributed to a reduced probability of re-employment. I also confirm that even with a maximum UI duration of around one year, a spike of re-employment hazard exists near benefit exhaustion, providing an argument for the storable offer model and the scheduled recall model.

The rest of the paper is organized as follows: section 2 summarizes the existing literature on unemployment insurance, with an emphasis on the effects of UI on unemployment duration and labor market transitions. In section 3, I introduce the data sources and present summary statistics as well as descriptive analysis. Section 4 presents the empirical specification. In section 5, I present and discuss the estimation results. Section 6 concludes the paper. Appendix A is a collection of figures; Appendix B provides a detailed description of the data cleaning process with the unemployment duration variable; Appendix C contains tables of the results of regression analysis.

2. Literature Review

There exists abundant literature on unemployment insurance. Economists share a consensus that unemployment benefits are necessary. Only in the static labor supply model is there no role for insurance (Moffitt and Nicholson, The effect of unemployment insurance on unemployment: the case of federal supplemental benefits. 1982), and such a model is far from being close to reality. Mortensen (1977) demonstrated that in a search model, where finding a job costs time and effort, UI could reduce the income loss that would otherwise occur and provide workers with insurance while they search for new jobs. Most subsequent theoretical literature follow Mortensen and model the job searching process of unemployed individuals. Different models measure UI differently; the three most common measures of UI are the number of benefit weeks payable (thereafter, maximum duration), maximum benefit amount, and the replacement rate, the ratio of an individual's weekly UI benefits to his pre-unemployment weekly wage.

This paper aligns closely to the set of literature that explore UI's effects on the unemployed. A UI program must provide adequate benefits to protect workers who lost their job involuntarily. Naturally, one would worry that generous benefits would induce unemployment. Consistently, empirical analysis has demonstrated that an extension in UI benefits is associated with a prolonged unemployment spell or a decreased probability of leaving unemployment, regardless of whether the authors applied a discrete-time framework (e.g., multinominal logistic regression) or a continuous-time framework (e.g., a survival model). Card and Levine (2000) showed that when Extended Benefits were available in New Jersey between 1995 and 1997, the

fraction of UI claimants who exhausted their regular benefits rose by 1–3 percentage points. Lalive (2007) studied Austria's REBP program and concluded that large benefit extensions increased the duration until a new job was taken. My estimates also affirm that the availability of extra UI benefits increased the probability of staying unemployed. What economists disagree with is whether these observed phenomena prove that UI generates an economically significant disincentive to work.

There exist two primary channels for UI to affect unemployment: on the one hand, UI benefits can lead recipients to reduce their search effort, substitute leisure for work, and raise their reservation wage, slowing the transition into employment. On the other hand, these benefits, available only to those engaged in active job search, provide an incentive for the continued search for those who might otherwise exit the labor force. The first channel is usually referred to as the “moral hazard” effect and constitutes a disincentive to work. The second channel does not significantly harm economic efficiency while it increases the labor force participation rate[2] (if anything, an incentive to work). Vodopivec (1995) discovered that the escape rate (from unemployment) of the recipients of UI to employment increased dramatically just before potential exhaustion of UI and decreased equally dramatically after benefits were exhausted. He concluded that the fact that exit hazard rises significantly just before benefit exhaustion proves the "waiting behavior" of UI recipients. Similarly, Katz and Meyer (1990) found that sharp increases in the escape rate from unemployment both through recalls and new job acceptances are apparent for UI recipients around the time of benefits exhaustion. Such

[2] Clark and Summers (1982) commented that this could be a reporting rather than a behavioral effect.

increases are not apparent at similar points of spell duration for nonrecipients, inferring a causal relationship between UI receipt and a delay in becoming re-employed. They demonstrated that a decrease in both the maximum benefit amount and the maximum duration of unemployment compensation could raise the exit hazard[3]. Most recent studies, however, pointed out that several earlier studies mixed the two channels and thus overestimated the disincentive to work. These latest studies separately estimate the exit to employment and out of the labor force, and I follow this approach. Farber et al. estimated a logistic model with competing risk and found that the UI extensions during the Global Financial Crisis have not had sizeable moral hazard effects on recipients' job-finding rate, thus no impact on labor market efficiency. Studying the same period, Rothstein (2011) infers that the UI extensions raised the unemployment rate in early 2011 by only about 0.1 to 0.5 percentage points, with at least half of this effect attributable to reduced labor force exit among the unemployed rather than to the changes in reemployment rates that are of more significant policy concern[4]. Chetty (2008) provided a unique perspective: 60 percent of the increase in unemployment durations caused by UI benefits is due to a "liquidity effect" rather than "moral hazard". As a result, increases in benefits have much larger effects on durations for liquidity-constrained households. I contribute to this literature by adding evidence that xxxxx.

My research also supplements a rapidly growing set of literature on the effects of the labor market interventions by the U.S. Federal government during the Covid-19 pandemic. The

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[3] Interestingly, Katz and Meyer found that lowering the replacement rate had no affect on the exit hazard.

[4] Fujita (2010) disagrees with Rothstein (2011) and Farber et al. (2015). His counterfactual calculations show that roughly 50-60 percent of the total increase is attributed to the effects on UE transition rates.

most relevant study is Marinescu, Skandalis and Zhao (2021); the authors used data from Glassdoor, a job search platform, and discovered that each 10% increase in benefits caused a 3.6% decline in job applications while it did not decrease job vacancy creation. The high benefits reduced competition for jobs, which was likely welfare-improving in a tight labor market. Another pivotal literature is Ganong, Noel and Vavra (2020), a paper that computed how the expansion of UI changed the expected income of a typical worker in a group during the pandemic. They reported that the median statutory replacement rate was 145% between April and July 2020. Another interesting observation in this paper is that low-earning workers had greater ex-ante increases in expected income than high-earning workers due to the $600 FPUC benefits. Bitler, Hoynes and Schanzenbach (2020) explored the rising food insecurity rates during the pandemic and argued that one factor was the substantial delay of relief payments due to the overwhelmed UI systems. The contribution of this paper is that it provides evidence to a question of significant policy interests: did the UI extensions during the pandemic influence the duration dependence of unemployment spells?

Lastly, empirical evidence also suggests that the effects of a change in UI depends on how “generous" the legislative change was, as well as how the change was introduced – did it change the maximum benefit amount, the maximum duration, or the replacement rate? Anderson & Meyer (1997) documented that a 10 percent increase in the WBA would increase UI take-up rate by 2 to 2.5 percentage points, while a similar increase in PD would increase take-up by 0.5 to 1 percentage point. Lalive (2007) highlighted the heterogeneity in UI’s effects by the maximum duration extended by Austria’s REBP program. These sets of literatures underscore the potential usefulness of studying the effect of a drastic change in the amount and duration of the

unemployment insurance program, such as that generated by EB, FPUC, PEUC, IWA, and IBA during the Covid-19 pandemic. I therefore turn to a detailed discussion of these programs and the relevant data.

3. Data

For this study, I combine individual-level survey data from the U.S. Census Bureau's Current Population Survey (CPS) with a state-level UI policy tracker. I also use state-level monthly unemployment data from the Local Area Unemployment Statistics (LAUS) by the Bureau of Labor Statistics (BLS), non-farm job opening data from the Job Openings and Labor Turnover (JOLTS) by the BLS, and the Oxford Stringency Index by the Oxford Covid-19 Government Response Tracker (OxCGRT). Figure 4 and Figure 5 in Appendix A visualize the ratio of non-farm job openings to unemployed and the Oxford Stringency Index.

3.1 UI Tracker

The UI Tracker is a state-level dataset collected by Prof. Peter that keeps track of the duration, the amount, and the eligibility requirements of UI benefits. It also contains detailed information about every extension to the regular UI benefits during the Covid-19 pandemic, including the starting dates, the end dates, the amount or duration extended, and whether there was a delay in handing out the benefits. I contributed to the UI tracker by appending information regarding the regular UI benefits in 2018 to the dataset. Specifically, I collected the maximum amount and duration of regular state UI benefits and the eligibility criteria from the series of "Significant Provisions of State UI Laws," published by the U.S. Department of Labor. I used the January 2018 Issue for the first half of 2018, and the July 2018 Issue for the rest of the year. The

UI tracker enables me to construct my key explanatory variables, which will be introduced in section 4.

During the Covid-19 pandemic, there were several extensions to the regular UI benefits. First, the Pandemic Emergency Unemployment Compensation (PEUC) under the CARES Act extended UI duration by up to 13 weeks. In addition to the PEUC duration extension, every state except for South Dakota extended benefits according to the Extended Benefits (EB) federal provisions and added 13 weeks or 50 percent of the regular maximum duration, whichever is smaller (when triggered[5]). States may implement a second trigger allowing up to 20 weeks of EB (but not exceeding 80 percent of the state maximum) during high unemployment periods. Figure 1 and Figure 2 presents the maximum UI duration in each state and its distribution, before and during the pandemic. The maximum of the regular state UI benefits in 2019 was 30 weeks, and most states offered 26 weeks. In January 2021, the mode increased to 39 weeks, with some states offering as many as 59 weeks. In terms of supplement UI benefits, there were the Federal Pandemic Unemployment Compensation (FPUC), Lost Wages Assistance (LWA), and Increased benefit assistance (IBA). The pandemic-related additional weekly benefit amount, when applicable, is usually $600 (FPUC 1) or $300 (FPUC 2). The maximum additional weekly benefit was $1,800, as a result of the governments of Alaska, Iowa, and New Hemisphere handed out LWA benefits in a lump sum.

[5] States are required to have an automatic EB trigger that is an Insured Unemployment Rate (IUR) of at least 5%, with its IUR exceeding a threshold of 120 percent of its IUR for the corresponding period in each of the prior 2 calendar years.

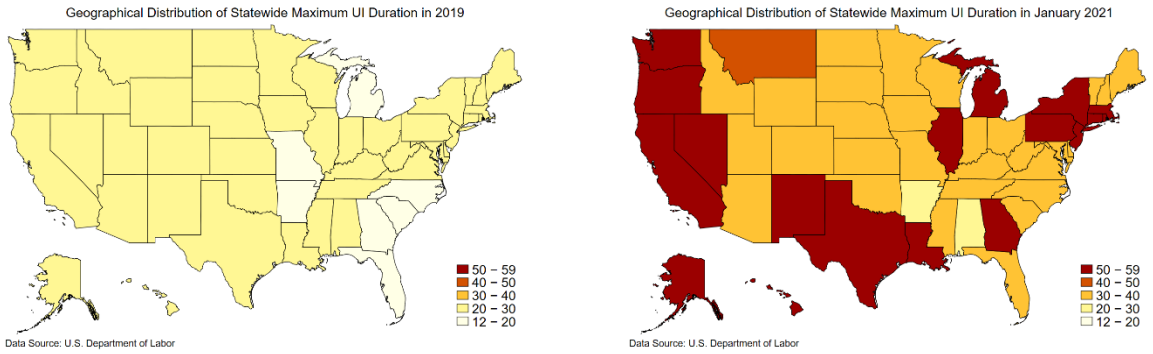

Figure 1. Maximum UI Duration in each state in the U.S. (2019 v.s. Jan 2021)

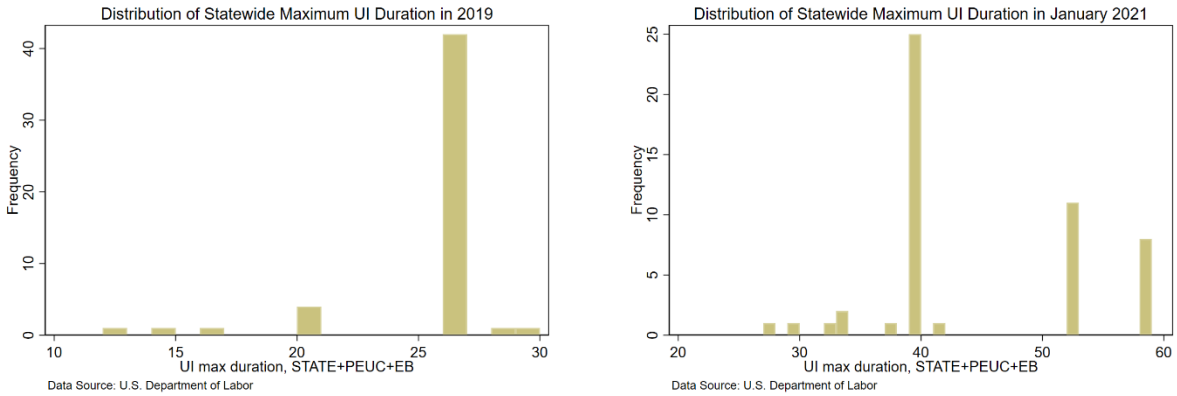

Figure 2. Distribution of State-level Maximum Unemployment Insurance Duration (2019 v.s. Jan 2021). The majority of the states have around 26 weeks of maximum unemployment insurance before the Covid-19 pandemic, whereas the mode in January 2021 is 39 weeks.

3.2 Current Population Survey

This project's primary data source would be the Current Population Survey, a panel dataset of monthly labor force statistics sponsored jointly by the U.S. Census Bureau and the U.S. Bureau of Labor Statistics. All observations are at the individual-month level, and I select the period from January 2018 to the present. Each monthly selected sample of the CPS is about 60,000 occupied households across all 50 states and the District of Columbia, who are in the survey for four consecutive months, out for eight, and then return for another four months before

leaving the sample permanently. I can measure the month-over-month transitions between employment, unemployment, and not-in-the-labor-force (NILF) status by matching individual workers in the CPS sample for two consecutive months. Owing to the sample rotation and the eight-month gap, at most 75% of the individual workers in the sample can be matched. Each two-period panel contains information regarding workers' labor market transitions from the first month to the next month. It also includes unemployment duration (if the worker is unemployed) and other standard observable characteristics. I can thus calculate the unemployment-to-employment (UE) transition rates and unemployment-to-NILF transition rates (UN) by duration. Following Farber, Rothstein, Valletta (2015), I also assume that job losers are eligible for the full duration of benefits and that each draw benefits continuously from the date of job loss until benefit expiration or exit from unemployment.

The number of individuals in the sample varies mildly by month; in January 2018, the sample surveyed 125,473 individuals, with ages ranging from 0 to 85. The observation of two key variables, unemployment duration, and labor force status, also vary by month; in January 2018, 102,329 (81.55%) individuals reported their labor market status, and among them, 2,634 (2.57%) were unemployed and reported their unemployment duration. I restrict the sample to people within the prime working age (25-54 years old) because their responses to changes in UI are of interest to economists and policymakers. Such a restriction also mitigates potential heterogeneity since the labor force participation rate of the non-prime working-age population is noticeably lower than that of the prime-working-age population[6]. In addition, I excluded

[6] Bureau of Labor Statistics, U.S. Department of Labor, *The Economics Daily*, Employment–population ratio and labor force participation rate by age

unemployed individuals who did not classify as job losers in CPS, as they shall not be eligible for unemployment insurance.[7] [8] Moreover, since this paper studies the labor market transitions of unemployed people, the effective sample in January 2018 is the collection of individuals who reported being unemployed and remained in the dataset in February 2018. After applying these restrictions, the effective sample size becomes 26,153, or about 556 observations per month. Among them, 31.37% became employed in the next survey round, 53.29% stayed unemployed, and 15.34% left the labor force.

There are three significant limitations of the CPS dataset. Firstly, I do not have longitudinal observations of the same group of individuals over the sample period; instead, I have at most 6 "effective" observations for each individual. Secondly, I cannot observe the job searching behaviors or intensity of the unemployed. Lastly, the employment status variable and the unemployment duration variable exhibit many cases of inconsistencies. Namely, an individual who was employed two months ago shall not report their unemployment duration to be 16 weeks. This issue is widely well documented by economists: both Poterba and Summers (1984) and McGovern and Bushery (1999) highlighted that both unemployment status and unemployment duration are often mis-reported. Recently literature[9] tackled this inconsistency by

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at https://www.bls.gov/opub/ted/2017/employment-population-ratio-and-labor-force-participation-rate-by-age.htm (visited *September 10, 2022*).

[7] CPS asks every unemployed individual to report their reason of unemployed, recorded in the "whyunemp" variable. I restrict the sample to those who classified as "job loser - on layoff" or "other job loser". It is worth noting that some individuals who became unemployed after their temporary job ended also qualify for UI, and my results) are robust to including those individuals.

[8] An increase in UI also may affect unemployed individuals who are ineligible for UI, especially if another individual in the same household benefits from the UI extension.

[9] Including but not limited to Farber, Rothstein, and Valletta (2015) , Fujita (2010), and Rothstein (2011).

recoding the employment status variable instead of the unemployment duration variable. I instead recoded the unemployment duration variable[10], since it is easier for a survey respondent to make a mistake regards their past labor market status (calculating the number of weeks since unemployment) than their contemporary labor market status (reporting their status at the time of survey). Another drawback of recoding labor force status is that it reduces the sample size further.[11]

Table 1 presents the summary statistics of all unemployed individuals in the effective sample, divided into two groups – before or during the pandemic. In both subsamples, most of the individuals do not have any disabilities[12], people averaged at 39 years old, and around 77% of the individuals do not have a child under 8. There are some differences in characteristics: the share of females before Covid was disproportionately high, while the proportion of individuals with a college degree was 2.87% higher during the pandemic. Overall, the differences between the two subgroups are somewhat significant.

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[10] I imposed four rules to recode the unemployment duration variable, and for each rule I tried both top coding the inconsistent cases and recoding the inconsistent cases to missing values. This generates $4 * 2 + 1 = 9$ different unemployment duration variables. In all reported results, the duration variable I used is the top-coded version that corrected for impossible leaps in duration, insufficient increases in duration, and conflicts between past employment status and recorded duration. Please refer to Appendix B for details.

[11] Farber, Rothstein, and Valletta (2015) stated that "imposing this adjustment to observed transitions requires three consecutive matched months of data and hence restriction to respondents in the first two of each set of four consecutive CPS interviews".

[12] Including hearing difficulty, vision difficulty, difficulty remembering, physical difficulty, disability limiting mobility, and personal care limitation.

	Before Covid		During Covid		Total	
	Count	%	Count	%	Count	%
Gender						
Female	3286	56.218	5565	50.403	8851	52.416
Male	2559	43.781	5476	49.596	8035	47.583
Marital Status						
Not married	3269	55.928	5894	53.382	9163	54.263
Married	2576	44.071	5147	46.617	7723	45.736
Having a kid under 8						
No	4521	77.348	8453	76.560	12974	76.832
Yes	1324	22.651	2588	23.439	3912	23.167
any disability						
No	5466	93.515	10439	94.547	15905	94.190
Yes	379	6.484	602	5.452	981	5.809
RaceEthnicity						
NH White	3396	58.100	6041	54.714	9437	55.886
NH Black	840	14.371	1532	13.875	2372	14.047
NH American Indian	103	1.762	134	1.213	237	1.403
NH Asian	244	4.174	839	7.598	1083	6.413
NH other/mixed	120	2.053	189	1.711	309	1.829
Hispanic (any race)	1142	19.538	2306	20.885	3448	20.419
Education attainment						
Less than HS	774	13.242	1055	9.555	1829	10.831
HS degree	3583	61.300	6911	62.593	10494	62.146
Bachelor's degree	1034	17.690	2270	20.559	3304	19.566
Graduate degree	454	7.767	805	7.291	1259	7.455
age	39.322	8.712	39.022	8.632	39.126	8.661

Table 1. Summary Statistics of All Unique Unemployed Individuals by Covid

4. Econometric Specifications

4.1 Base Model

In this section, I model how unemployed individuals across the U.S. responded to the extension in the maximum duration and amount of unemployment insurance by some of the COVID-19 relief programs. I choose a multinominal logistic model to estimate the conditional labor market transition probabilities, given an individual was unemployed in the previous month. Fujita (2010) underscored that there are a few advantages of choosing such a discrete chose model over a survival model empirically, and the main advantage is that "its flexibility and simplicity in terms of incorporating different types of covariates, whether categorical, continuous, time-invariant, or time-varying". Two important control variables I use, the ratio of non-farm job-openings to unemployed and unemployment rate, are only observed at monthly frequency, while they vary within each month. Incorporating these two control variables in a continuous time framework poses technical challenges; therefore, restricting the analysis within a discrete choice model is a plausible compromise.

In the base model (1), I estimate an unemployed individual's future labor market status (LM Status) as a function of their unemployment duration, the availability, the length, or the amount of "extra" unemployment insurance, and socio-demographic measures including levels of educational attainment and gender. To control for local labor market conditions and economic cycles, I control for (future) unemployment rate and the ratio of (future) job-opening per

unemployed. I also include census region ($Region_s$) and quarter ($Quarter_{i+1}$) fixed effects. Specifically[13],

$$\Pr(LM\ Status_{i,t+1} | U_{i,t}) = \beta_0 + \beta_1 UI_{s,t+1} + \beta_2 UD_{i,t} + \beta_3 (UI * UD)_{i,s,t,t+1} + \alpha X_{i,s,t+1} + \gamma Quarter_{t+1} + \delta Region_s + \varepsilon_{i,t+1} \quad (1)$$

, where

$$LM\ Status_{i,t+1} = \begin{cases} 1\ E\ (Employed) \\ 2\ U\ (Unemployed) \\ 3\ NILF\ (Not\ In\ Labor\ Force) \end{cases}$$

In the above equation, the subscript i indicates an individual, t denotes the month of the observation, and s indicates the state of individual residence.

- $LM\ Status_{i,t+1}$ is categorical variable with three levels: employed (E), unemployed (U), and not in labor force (NILF). In essence, since only individuals who are unemployed at time t enter the regression, the variable reflects three levels of labor market transition status: become employed (UE transition), remain unemployment, and exit the labor force (UN transition).
- $UI_{s,t+1}$ represents a set of explanatory variables that indicate the length or amount of additional unemployment benefits next month at $t + 1$ in a state s. It includes a dummy variable for the availability of Extended Benefits, a dummy variable for the availability of PEUC, a categorical variable for the number of weeks of Extended Benefits, and a dummy variable for the availability of additional

[13] I estimated model (1) without the variables for UI extensions twice separately, once for the pre-covid era (2018-2019) and once for the covid era (2020-2021). Estimated coefficients are qualitatively consistent over the two periods. Results are available upon requests.

benefits.[14] In each estimation, only one of these variables and its interaction with the unemployment duration variable is included in the estimated equation.

- $UD_{i,t}$ measures the length of unemployment duration of individual i at time t in months. While the original duration is reported in weeks, I recoded it in months for the ease of visualizations of results. I expect β_1 to be negative for unemployment-employment transitions (UE transition), as studies including Marinescu & Skandalis (2021) and Shimar (2008) have repeatedly proven the duration-dependence of unemployment spells[15][16].
- $X_{i,s,t+1} = \{X_i, X_{i,t+1}, X_{s,t+1}\}$ is a vector of control variables predicted to influence labor market transitions. It consists of time-constant individual characteristics X_i including the respondent's gender, race/ethnicity, educational attainment, and any disability; characteristics which may vary over time X_{t+1}, including marital status, age group, and having a child below seven years old; and monthly state-wide economic conditions $X_{s,t+1}$ including the unadjusted unemployment rate, the number of non-farm job openings per unemployed and the Oxford Stringency Index.
- $\varepsilon_{i,t+1}$ is an unobserved time-varying determinant of labor market transition, which is assumed to be uncorrelated with the other variables in the model.

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[14] For my main results, I choose not to use continuous explanatory variables as their marginal effects are difficult to calculate or to interpret.

[15] Marinescu and Skandalis (2021) presented evidence that workers decrease the wage they target by 1.5% over each year of unemployment, irrespective of their UI status. Shimer (2008) showed that workers who have been unemployed for longer are less likely to find a job.

[16] A common explanation of duration dependence is that longer unemployment duration presents a negative signal to employers and makes it more difficult for an unemployed individual to find a job.

In model (1), β_3 is my coefficient of interest. The interpretation of β_3 is that with an additional month of unemployment, how would the log-likelihood of UE/UN transitions change differently when UI extension are available? In other words, it measures how the availability of a UI extension affect the duration dependence of unemployment spells. The abundant literature on unemployment insurance shares a consensus that extending UI discourages individuals to exit unemployment. However, as I have summarized in Section 2, they do not agree on whether such an effect shall be attributed to a decrease in the likelihood of UE transition or UN transition. To gauge the source of the prolonged unemployment, I calculate the average marginal effect (AME) of an additional month of unemployment on labor market status next month, with or without UI extensions.

I employed three control variables that vary at the state-level. Firstly, the (unadjusted) monthly unemployment rate is a proxy for the local economic conditions as well as labor market conditions. Following Fujita (2010) and Farber, Rothstein, and Valletta (2015), I control for the slackness of the local labor market, using the ratio of total non-farm sector job openings over the number of unemployed. The higher this ratio is, the higher the labor demand is unmet, and the more job-finding opportunities an unemployed individual has[17]. The rationale behind is that (Kroft and Notowidigdo 2016) extension of the unemployment insurance program could have larger effects on job finding in a relatively tight labor market. As we can observe from figure 4, the ratio of total non-farm sector job openings over the number of unemployed dropped sharply

[17] Fujita (2010) used the inverse of this ratio as a control, as the inverse was available directly via the JOLTS data set. I prefer my choice for its ease of interpretation.

when COVID hit the US and resulted in the (temporary or permanent) closure of many businesses. An implicit assumption here is that job openings are to the availability of UI extensions. This assumption does not hold in a standard search and matching model (Mortensen and Pissarides 1994) that features the endogenous job creation margin, because the number of job seekers, which is influenced by the generosity of UI, affects job creation. Nevertheless, the violation of this assumption would only lead to underestimation.

Historically, most studies of individual-level impacts of unemployment insurance focused on males. Moffitt (1985), Meyer (1990), and Katz and Meyer (1990) claimed that women are often secondary earners in a household (which may complicate the interpretation of the results) and excluded females from their estimation). Ehrenberg & Oaxaca (1976) ran separate estimations for males and females in different age groups. With the high marriage rates and mostly heterosexual households in the past, a male partner was generally the primary breadwinner and considered the household head, and the crude exclusion of females from the estimation sample may not be significant. However, the composition of American households has drastically transformed in the last thirty years. According to the 2019 American Community Survey (ACS), the share of female-headed households (including both married and non-married households) has increased from 32.5% in 1990 to 49.8% in 2019 (see Figure 6), and more than half of African American households nowadays are headed by women. As more women become household heads, they take upon the responsibility of being the breadwinner for their familiar, and their responses to the UI extensions are also of significant policy interest. Therefore, my sample includes both men and women. Still, since societal pressures continue to discourage women from remaining in the labor force after marriage and after giving birth to their child, it is

natural to suspect that the same characteristics would affect the labor market transitions for males and females differently. I thus included the interaction terms between female and marriage and between female and having a child below seven years old in $X_{i,s,t+1}$.

4.2 Heterogeneity of UI's Effects

The base model (1) assumes a linear duration dependence, and implicitly only allows UI extension to influence duration dependence linearly. However, duration dependence may not be a linear relationship[18]. Here, I present two improved versions of model (1). First, I estimate:

$$\begin{aligned}\Pr\left(LM\ Status_{i,t+1} \middle| U_{i,t}\right) &= \beta_0 + \beta_1 UI_{s,t+1} + \sum_{k=1}^{3} \beta_2^k \left(UD_{i,t}\right)^k + \sum_{k=1}^{3} \beta_3^k (UI * UD)_{i,s,t,t+1}{}^k * UI_{s,t+1} \\ &+ \alpha X_{i,s,t+1} + \gamma Quarter_{t+1} + \delta Region_s + \varepsilon_{i,t+1}\end{aligned} \tag{2}$$

To account for potential non-linearities, I let unemployment duration and its interaction with $UI_{s,t+1}$ enter the model in cubic. The non-linear shape of equation (2) allows unemployment duration to have a different effect for those with a duration of 4 weeks relative to those with 20 weeks. More importantly, model (2) also allows UI's effect to differ by the length of unemployment spells in a non-linear manner.

The second improvement follows from an important observation: the non-linearly of UI's effects on labor market transitions is largely determined by the maximum duration of UI. Beyond

[18] For example, Katz (1986) showed that the negative duration dependence can be largely attributed to a fall in the probability of being recalled, which does not exhibit a linear relationship with unemployment duration. Another observation is that at certain points, longer duration makes it very difficult for people to find a job.

the threshold, people are no longer eligible for UI benefits, and I would expect an increase in UI would not have a direct impact on them[19]. It therefore seems natural to investigate the effect of UI around the point of exhaustion.

In model (3), I follow Fujita (2010). Instead of reporting unemployment as a continuous variable, I group the reported durations into the following seven categories[20]. I shall refer to model (3) as the "constant bin" model. Fujita argued that an advantage of reporting duration is that this approach would allow us to capture the shape of the hazard functions in a flexible manner; for instance, it allows for a jump in the transition rate at a certain duration bin. The effects of the extended unemployment insurance are also allowed to differ at different duration bins. Specifically, I estimate:

$$\begin{aligned}\Pr\left(LM\ Status_{i,t+1} \middle| U_{i,t}\right) \\ &= \beta_0 + \beta_1 UI_{s,t+1} + \sum_{k=1}^{7} \beta_2^b UD_{i,t}^b + \sum_{k=1}^{7} \beta_3^b \left(UD^b * UI\right)_{i,s,t,t+1} \\ &+ \alpha X_{i,s,t+1} + \gamma Quarter_{t+1} + \delta Region_s + \varepsilon_{i,t+1}\end{aligned} \tag{3}$$

I define $UD_{i,t}^b$ as a vector of seven dummy variables that indicate which duration bin the unemployed individual is in: (1) 0-6 weeks, (2) 7-16 weeks, (3) 17-26 weeks, (4) 27-36 weeks, (5) 37-46 weeks, (6) 47-59 weeks, (7) 60 weeks or more. Two important cut-off points are 26 weeks and 59 weeks. Before the pandemic, most US states (78.64% of all

[19] UI extensions can affect non-eligible unemployed individuals through various channels. For instance, if one member of a household is eligible for UI, and if there is an increase in the duration or amount of UI, it become less urgent for other member(s) in the same household to find a job.

[20] Fujita (2010) defined 12 dummy variables indicating which duration bin the worker is in: less than or equal to 4 weeks, 5 − 8 weeks, 9 − 12 weeks, 13 − 16 weeks, 17 − 20 weeks, 21 − 24 weeks, 25 − 28 weeks, 29 − 32 weeks, 33 − 40 weeks, 41 − 68 weeks, 69 − 96 weeks, and 97 weeks or more.

observations at individual-month level) had a maximum UI duration of 26 weeks; during the pandemic, the maximum total duration of UI was 59 weeks. I use b=2 as the base categorical. From past literature, a spike in transitions out of unemployment is usually observed at the point of exhaustion (26 weeks in most states), so I expect $\beta_2^3$to be positive for either UE or UN transition (or both). More importantly, when the maximum duration of UI was extended, the bin (3) 17-26 weeks was no longer the point of exhaustion. As a result, the unemployed no longer have an incentive to leave unemployment as soon as possible, and β_3^3 is thus expected to be negative.

5. Results

5.1 Estimated Marginal Effects

This section presents the estimation results from models (1) to (3). First, I present the baseline results from estimating model (1) with different explanatory variables. Tables 2, 3, and 4 present the average marginal effects (AME) of an additional month of unemployment duration on labor market status transitions, per the availability of EB, PEUC, or additional weekly benefit amount (either by FPUC, FPUC2, IBA, or LWA), respectively. All estimated marginal effects are statistically significant and are consistent across specifications. On average, in the absence of the specific UI extension (the key explanatory variable in each specification), an additional month of unemployment duration is associated with a 2.2%-2.3% lower probability of becoming unemployed, a 1.3%-1.4% higher probability of staying unemployed, and a 0.8%-0.9% higher probability of leaving the labor force. These results are consistent with the well-document duration dependence nature of unemployment spells; the estimates of average marginal effects show that the

longer an individual stays unemployed, the less probable re-employment becomes. With EB (FPUC/additional weekly benefit amount) available, on average, unemployed individuals are 2.4%-2.6 less likely to transition to employment, 1.7%-1.9% more likely to remain unemployed, and 0.7%-0.8% more likely to detach from the labor force. The data of interest is the difference between AMEs per the availability of UI extensions. For EB and PEUC, two programs that extended the maximum duration of unemployment benefits, I have weak evidence ($p<0.10$) that the extensions lead to a lower UE transition rate (-0.4%), as well as some evidence ($p<0.05$) of a positive effect on prolonging unemployment (0.4%). Using the indicator of the availability of extra weekly benefit amount, I found statistically more significant effects on both U-E (-0.4%) and U-U (0.6%) status. Across the three specifications, contrary to many studies that focus on the Global Financial Crisis, I did not find evidence supporting the existence of a labor force attachment effect of UI. The estimated average marginal effects are both economically (close to 0) and statistically insignificant. [21] Therefore, it can be inferred that UI extension may have prolonged unemployment spells during Covid by delaying re-employment.

[21] I've also estimated model (1) with other explanatory variable, such as the (logged) amount of benefits. I found that the coefficients on the interaction term between unemployment duration and the amount of FPUC benefits or the total amount of additional benefits are negative and statistically significant for U-E transition, whereas the interaction terms with the amount of FPUC2, IBA, and LWA are not. I also found mixed results on U-N transitions. Results are not reported due to the difficulties in calculating and interpreting the average marginal effects of a continuous variable identified by another continuous variable.

LM Transitions	Without EB	With EB	Difference
U-E	-0.022***	-0.026***	-0.004*
	(0.001)	(0.002)	(0.002)
U-U	0.014***	0.018***	0.004**
	(0.001)	(0.002)	(0.002)
U-N	0.008***	0.008***	-0.000
	(0.001)	(0.001)	(0.001)

Table 2. Average Marginal Effects of an additional month of unemployment duration on labor market transition probabilities per the availability of Extended Benefits (EB).

LM Transitions	Without PEUC	With PEUC	Difference
U-E	-0.023***	-0.024***	-0.004*
	(0.002)	(0.001)	(0.002)
U-U	0.014***	0.017***	0.004**
	(0.002)	(0.001)	(0.002)
U-N	0.008***	0.008***	-0.000
	(0.001)	(0.001)	(0.001)

Table 3. Average Marginal Effects of an additional month of unemployment duration on labor market transition probabilities per the availability of Pandemic Emergency Unemployment Compensation (PEUC).

LM Transitions	Regular UI Amount	Extra UI Amount	Difference
U-E	-0.022***	-0.026***	-0.004**
	(0.001)	(0.002)	(0.002)
U-U	0.013***	0.019***	0.006***
	(0.001)	(0.001)	(0.002)
U-N	0.009***	0.007***	-0.002
	(0.001)	(0.001)	(0.001)

Table 4. Average Marginal Effects of an additional month of unemployment duration on labor market transition probabilities per the availability of additional weekly benefit amount

While the PEUC program extended a maximum of 13 weeks for all eligible participants, the UI extension provided by the EB program depends both on the local maximum statutory UI duration and the state-level insured unemployment rate (to trigger additional EB). I, therefore, explore EB extensions with different lengths. EB in my sample may have a maximum duration of 6, 7, 8, 13, or 20 weeks when available. Table 5 presents the estimated average marginal effects of an additional month of unemployment, depending on the length of the extension provided by UI. The results are mixed – at all levels, it is estimated that, on average, the availability of EB extensions lowered the UE transition rates (from 0.2% to 6.1%). However, the most substantial drop of UE transition probability occurred at EB=7, whereas the lowest two drops correspond to the two highest lengths of EB duration. The AMEs of an additional month of unemployment with EB at 8, 13, or 20 weeks are not statistically significantly different from the AME when no EB is available; the differences are statistically significant ($p<0.05$) with EB at 6, 7, or 10 weeks. Nevertheless, it is vital to note that the majority of individuals in my sample had either 13 or 20 weeks of EB during the pandemic. A total of merely 793 observations had 6, 7, 8, or 10 weeks of EB, and therefore the estimates on these categories of the EB duration variable may not be accurate. In three out of the six levels of EB extension, I observed a statistically significant and positive AME on the probability of remaining unemployed. Consistent with results in tables 2-4, I found no evidence that the extra UI duration provided by EB gave people an incentive to stay in the labor force.

Nevertheless, any findings from the results of model (1) are weakened by the results of model (2), shown in table 7. The average marginal effect of an additional month of unemployment on the probabilities of transitioning from unemployment when EB, PEUC, or other extensions is available is not statistically significantly different from that when these UI extensions are absent. I also calculated the marginal effects of an additional month of unemployment at selected levels of unemployment duration[22]. For instance, the marginal effects at month = 4 can be interpreted as "if an individual has been unemployed for four months, how would the individual's probability of transitioning from unemployment be different if the individual adds another month of unemployment"? Again, the marginal effects are statistically not different with or without UI extensions at almost all levels of unemployment duration.

The limited and conflicted results from model (1) and model (2) underscores the importance of model (3). As discussed in the literature review, preview studies have found that the exit rate from unemployment typically would spike around benefit exhaustion. Where do spikes occur, therefore, depend directly on how "close" an unemployed individual is to the exhaustion threshold. I study if the UI extensions (in duration) during Covid fostered a similar phenomenon with model (3). Table 8 shows the results from model (3).

When the maximum UI duration is extended by EB or PEUC, people with current unemployment durations between 17-26 weeks are 3.4%-6.6% more likely to stay

[22] Unreported, results available upon requests.

unemployed next month. The results are very intuitive: the maximum statutory UI duration in most states is 26 weeks, and the bin 17-26 weeks contains the maximum statutory UI duration of all but two states. Without UI extensions, these people have an incentive to exit unemployment as they are near benefit exhaustion[23]. With EB or PEUC, however, individuals with duration 17-26 weeks are relatively "far" from the exhaustion threshold, and they do not have the urge to leave unemployment immediately. Results from table 8 do not agree on why people with duration 17-26 weeks are more likely to stay unemployed during Covid (see Figure 7 and Figure 8), i.e., why they would be more likely to exit unemployment in the absence of UI extensions. Regression analysis focused on EB shows a sharp decrease in the probability of U-N transition (labor-force attachment effect), whereas regressions focused on PEUC hint the presence of model hazard effect (people would otherwise leave unemployment via U-E transitions).

I then explore the subgroup of unemployed individuals with duration between 47 weeks to 59 weeks. Their durations are near the benefit exhaustion thresholds when UI extensions (in duration) are available. Following the same logic, we'd expect that among these people, those with extended UI have a greater incentive to leave unemployment than those with regular UI. From table 8, the predicted probability of becoming re-employed for people with duration between 47 and 59 weeks is 4.4% higher when longer UI via EB is

[23] There are several explanations why exits spike at exhaustion. Models with storable offers show that some workers may have received a satisfactory offer but opt to "store" the offer until benefit exhaustion. Katz (1986) found evidence that workers on layoff often schedule their dates of recall with their previous employers to receive more unemployment benefits. On the other hand, once job search is no longer compensated, some would leave the labor force.

available. This again infers that some people stayed unemployment for a longer period voluntarily and collected benefits. The estimated results studying PEUC is also positive but statistically insignificant. Overall, from model (1) to model (3), I find some limited evidence that the UI extensions during the Covid-19 pandemic reinforced the duration dependence nature of unemployment spells and people were postponing re-employment.

Finally, I briefly comment on the estimated average marginal effects of the control variables. Table 6 presents the estimated AMEs and corresponding standard errors of the control variables from Model (1)[24]. Relative to the base category (people between 25 and 29 years old), people in all other age groups are, somewhat surprisingly, about 20% less likely to leave the labor force. I propose two potential reasons: on one hand, people below 30 years old may go to attend graduate school after a couple of years of work; on the other hand, people tend to have their first baby in the late twenties, and they temporary leave the labor force to take care of their children. Females on average are less likely to stay unemployed; yet, they don't have a higher chance of re-employed but choose to leave the labor force more commonly than males. Married individuals are associated with a higher chance of finding a job and a lower chance of remaining unemployed. Consistent with common expectations, people with disabilities find it more difficult to find a job. This sub-population also has a higher chance of detaching from the labor force, potentially since they cannot appear to be capable of working if they wish to claim supplementary security

[24] Since the estimated coefficients on all of the control variables are consistent from models (1) to (3), I will only report them once.

income from the Social Security Disability Insurance program (SSDI). Future labor market status also depends heavily on one's education backgrounds: comparing to high school graduates, people without a high school diploma are more likely to quit searching for jobs. The trend is reversed for people with a bachelor's degree – they stay longer in unemployment and refuse to quit, potentially because they are confident that they can find a better job offer. People with the highest education attainments can quickly find a job. Overall, the estimated average marginal effects are in line with the conventional knowledge qualitatively.

5.2 Robustness Check

In this section, I explore several potential alternative explanations for my results. Kroft and Notowidigdo (2016) found that the moral hazard cost of UI is procyclical and more sizable when the unemployment rate is relatively low. Since my sample covers a period when the unemployment rate spiked (and simultaneously, UI extensions were available), ignoring this channel would lead me to overestimate the negative effect of UI extensions on re-employment. To mitigate this concern, I use both the unadjusted state-level unemployment rate and the ratio of total non-farm job openings over the number of unemployed to capture the business cycle. In my model, high moral hazard costs can be inferred from a low UE transition rate. My estimations yield a statistically negative association between unemployment rates and the probability of UE transitions, affirming the findings of Kroft and Notowidigdo (2016). Similarly, the coefficient on the ratio of total non-farm job openings per the number of unemployed is statistically significant and positive.

By my construction of models (1) – (3), an implicit assumption is that the duration dependence nature of unemployment spells is independent from individual characteristics. If violated, my results could be biased (upwards or downwards). Therefore, I explore how duration dependence varies by individual characteristics. In a multinomial logistic regression setting, I regress the future labor market status on each characteristic, unemployment duration, and their interaction terms. Overall, most characteristics do not seem to affect the duration dependence, as the coefficient on the interaction term between each characteristic and unemployment duration is statistically insignificant in all but one estimation[25]. The effects of several individual-level control variables on the likelihood of UN transition vary with unemployment duration. Most notably, people with college degrees or above are less likely to leave the labor force despite long unemployment durations. Overall, the duration dependence nature of unemployment spells is homogenous and is not markedly affected by observable individual traits. Introducing an interaction term between unemployment duration and educational attainment or the disability indicator does not significantly alter the estimated results on key explanatory variables.

5.3 Discussion

In section 5.1, I presented some limited evidence that the extensions to the unemployment insurance program during the Covid-19 pandemic reinforced the duration dependence characteristic of unemployment spell. Results from model (1) showed that as

[25] The interaction term between unemployment duration and $diffany_{i,t+1}$ is statistically significant. Here, clearly the people with disability in my sample is not a representative of all disabled people in the US; for a disabled individual to be able to work, they on average would have higher skills (than an average not-disabled person) or specific skills demanded by the market.

individuals stay longer in unemployment, their likelihood to leave unemployment, and specifically, to become re-employed, decrease further when UI extensions are available. A natural reaction is to treat the results as direct evidence of the moral hazard inefficiencies generated by (the extension of) unemployment insurance. However, as Chetty (2008) highlighted, over half of the increase in unemployment durations caused by UI benefits is due to a "liquidity effect" rather than distortions on marginal incentives to search ("moral hazard"). The two effects have divergent welfare implications: the moral hazard effect is a labor market inefficiency that lowers productivity and output of the economy. In contrast, the liquidity effect is a socially beneficial response to the correction of the credit and insurance market failures. The potential existence of a strong liquidity effect is especially significant since the pandemic put more families under liquidity constraints[26]. To gauge the situation, I infer to the results of model (3) in Table 9. We observe that with and without UI extensions, there exists a spike of exit near benefit exhaustion. This phenomenon is better explained by the moral hazard effect: individuals keep a low search effort until near benefit exhaustion, or that they "store" satisfactory job offers (or recall offer) until UI is exhausted for them. From the perspective of the liquidity effect, however, the UI extension granted households under liquidity constraints a lot more time to look for a satisfactory job offer. Thus, the distribution of exits shall not be very concentrated near benefit exhaustion when the maximum duration is large. Combining results from model (1) and model (3), I claim that the extensions to the unemployment insurance program during the

[26] Nearly 15 percent of U.S. households—and nearly 18 percent of households with children—reported food insecurity early in the COVID-19 pandemic, according to a survey conducted via social media by researchers at NYU School of Global Public Health. A high percentage of households with food insecurity infers a high proportion of household without the ability to smooth consumption via the credit and insurance market due to liquidity constraints.

pandemic generated model hazard inefficiencies. Nonetheless, the above argument is definitely not rigorously enough to draw any solid conclusion, and I aim to address this issue further in my Honors Thesis. Moreover, we cannot conclude anything in regards to the total welfare implications of EB, PEUC, or FPUC either; a more holistic welfare analysis is needed to weigh the consumption smoothing effect, the labor force attachment effects, the moral hazard effects, and any other potential mechanisms.

As for the insignificant results from model (2), a candidate explanation would be that an increase in unemployment insurance can increase the value of employment, as the record of being employed qualifies an individual to receive unemployment benefits in the future. This, in turn, mitigates the findings from model (1) that people become re-employed more slowly when more benefits are available. However, it was clear to all workers that the PEUC and Extended Benefits during the Covid-19 pandemic were transitory. In other words, working during the pandemic would not make individuals qualified in the future for benefits above the statutory state UI benefits, as these additional benefits would expire as the society recovers from Covid. Since the value of potential unemployment benefits to be collected in the future was not changed by PEUC or EB, I argue that the extension in UI did not positively affect the UE transition, and I call for alternative explanations from future studies.

6. Conclusion

This paper studies the effects of the extensions to the unemployment insurance program during the Covid-19 pandemic on labor market outcomes. Mainstream search models underscore

two exit directions from unemployment: 1) re-employment and 2) leaving the labor force, and the likelihood of both types of exits depends on the generosity of the UI program. Using a subsample of involuntarily unemployed individuals in the CPS dataset, I aim to find the more dominant channel of the two. I found modest evidence that the UI extensions reinforce the duration dependence characteristic of unemployment spells. As unemployed individuals stay longer in their unemployment spell, they are more likely to remain unemployed when extra benefits are available. By exploiting the predicted probability of transitioning from unemployment, I highlight that the moral hazard effect at least partially explains the fall in exit hazard.

There are several limitations of this research project. Firstly, for simplicity, the specifications do not allow the numerical value of additional unemployment benefits to enter the logistic regression. The generosity of the UI extensions, more specifically, the generosity conditioning on an individual's pre-unemployment wage rate and (or) on the statewide statutory maximum benefit amount, is correlated with the magnitude of UI's effects and potentially may alter the economic significance of each type of exit. Future research can study the relationship between (the change in) individual-level replacement ratio and labor market transitions during the pandemic. In addition, the design of the models does not allow me to fully separate the effects between the two types of UI extensions (duration and amount). A comprehensive welfare analysis is also required to comment on the net welfare implications of the program.

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Appendix A - Figures

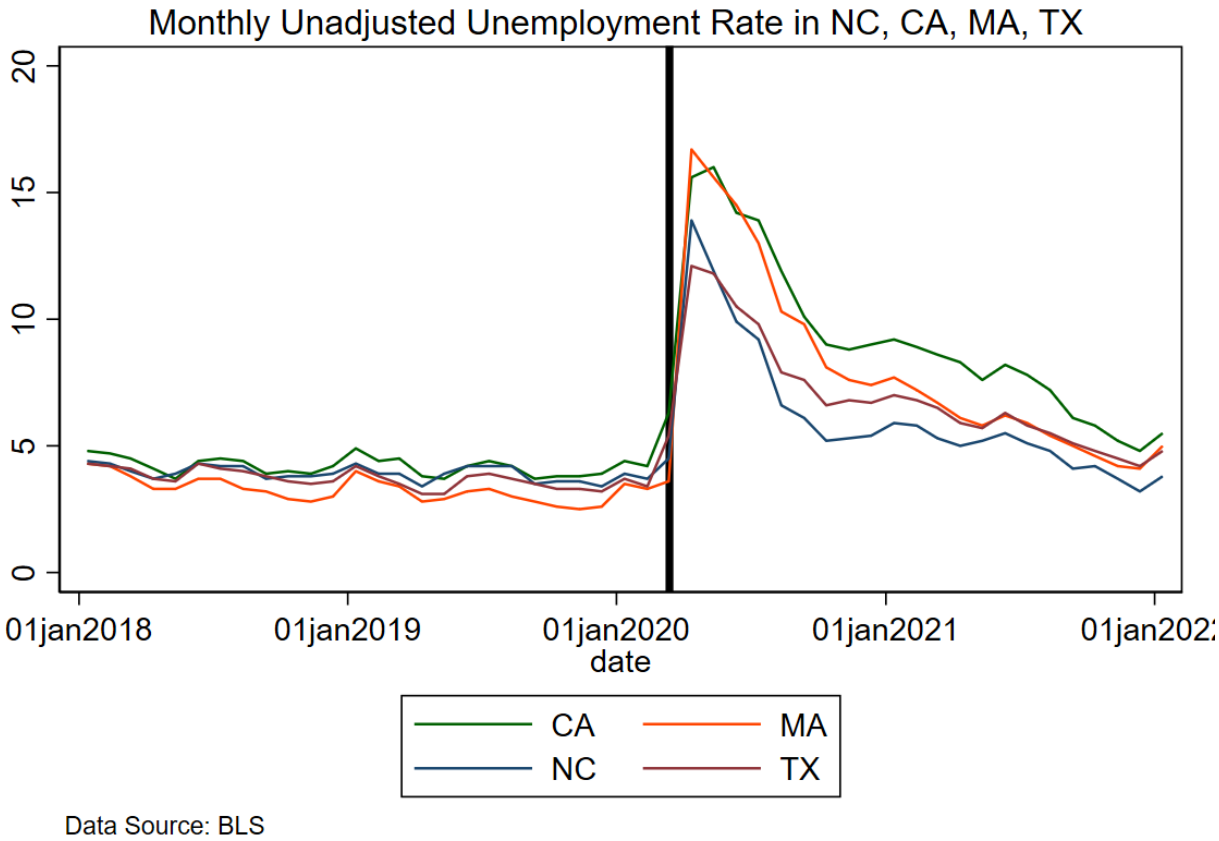

Figure 3. State-level unemployment rate from Jan 2018 to Jan 2021 (not seasonally adjusted) for fmy selected states: NC, CA, MA, TX. The solid vertical black line indicates March 13, 2020, when President Trump declared a national emergency concerning the coronavirus disease 2019 (COVID-19) pandemic. Figure is created based on BLS data (https://www.bls.gov/charts/employment-situation/civilian-unemployment-rate.htm).

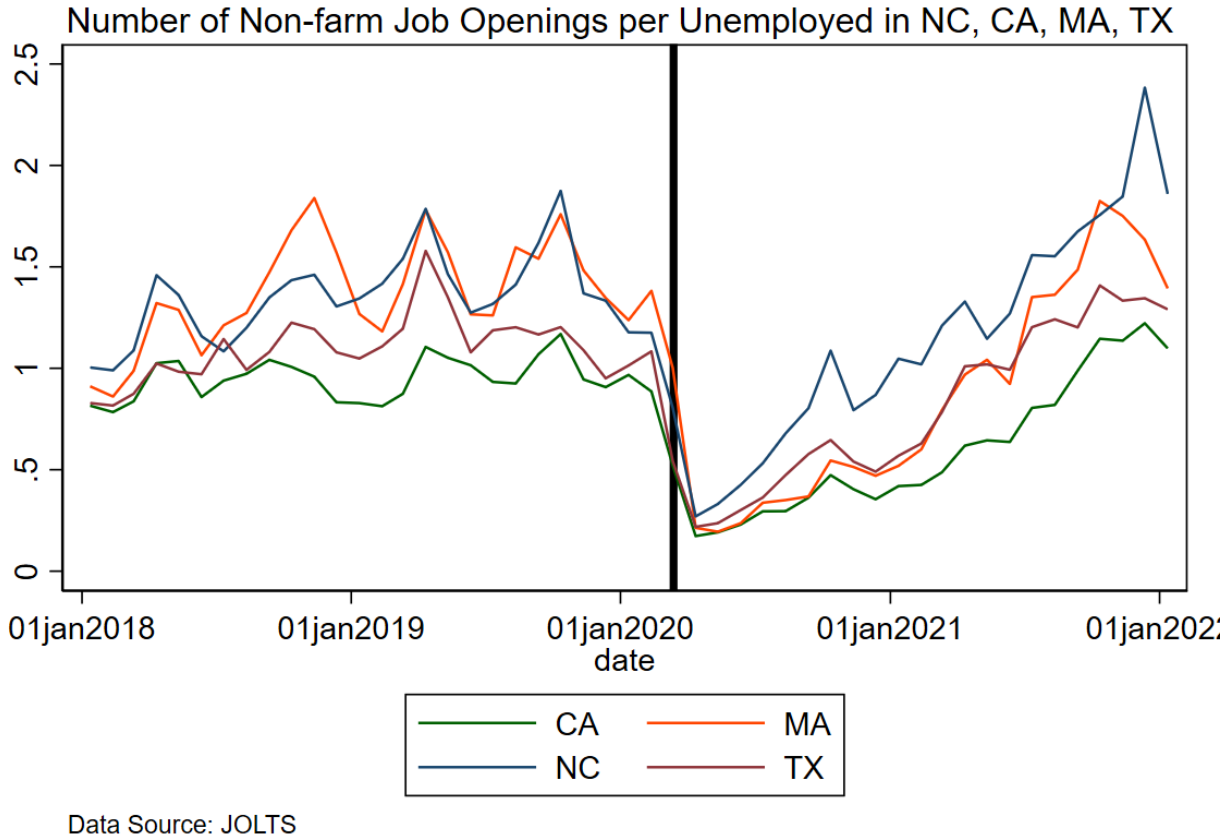

Figure 4. The ratio of the number of non-farm job openings in each state to the number of unemployed, from Jan 2018 to Jan 2021 for fmy selected states: NC, CA, MA, TX. The solid vertical black line indicates March 13, 2020, when President Trump declared a national emergency concerning the coronavirus disease 2019 (COVID-19) pandemic. Figure is created based on JOLTS data from the BLS (https://www.bls.gov/jlt/).

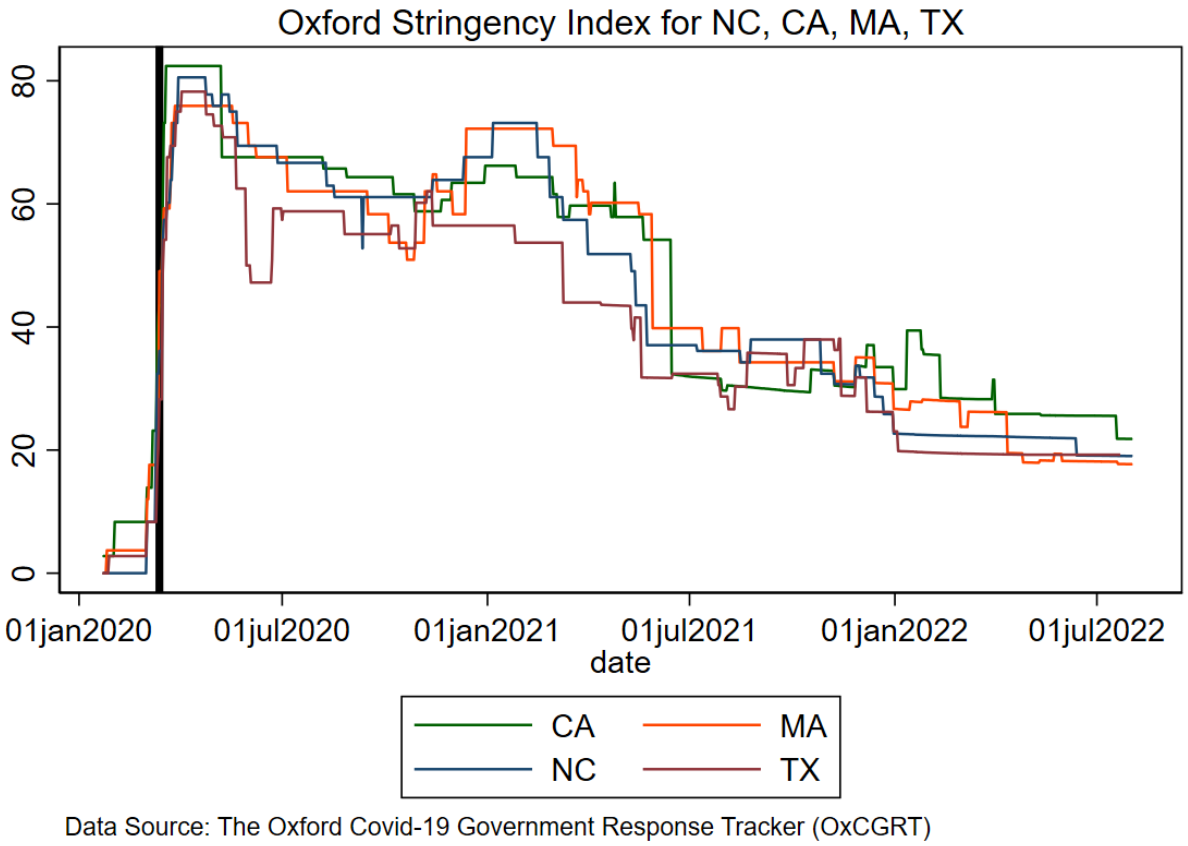

Figure 5. State-level Stringency Index from the Oxford Covid-19 Government Response Tracker (OxCGRT) since January 1st 2020 for fmy selected states: NC, CA, MA, TX. The solid vertical black line indicates March 13, 2020, when President Trump declared a national emergency concerning the coronavirus disease 2019 (COVID-19) pandemic. Figure is created based on data from the University of Oxford (https://www.bsg.ox.ac.uk/research/research-projects/covid-19-government-response-tracker).

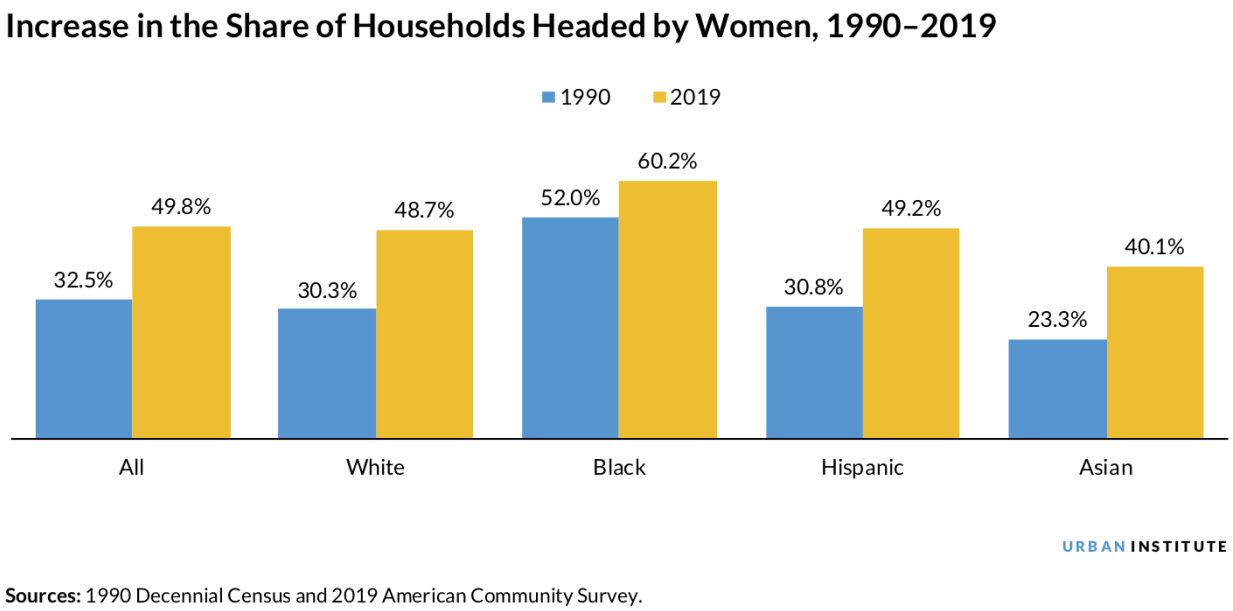

Figure 6. Increase in the Share of Households Headed by Women, 1990-2019. Source: Urban Institute

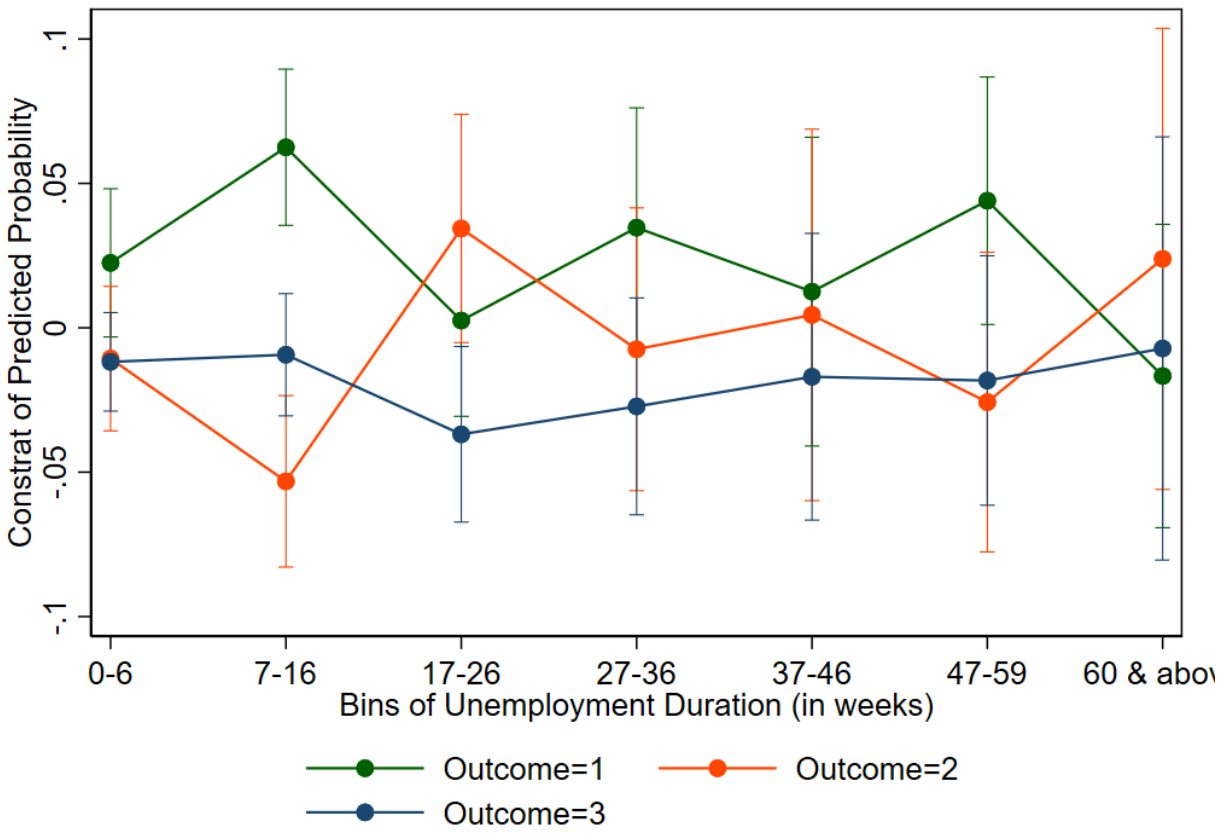

Figure 7. Contrast of predicted probabilities of labor market transitions by the availability of Extended Benefits. Outcome 1 / 2 / 3 = Employed / Unemployed / NILF.

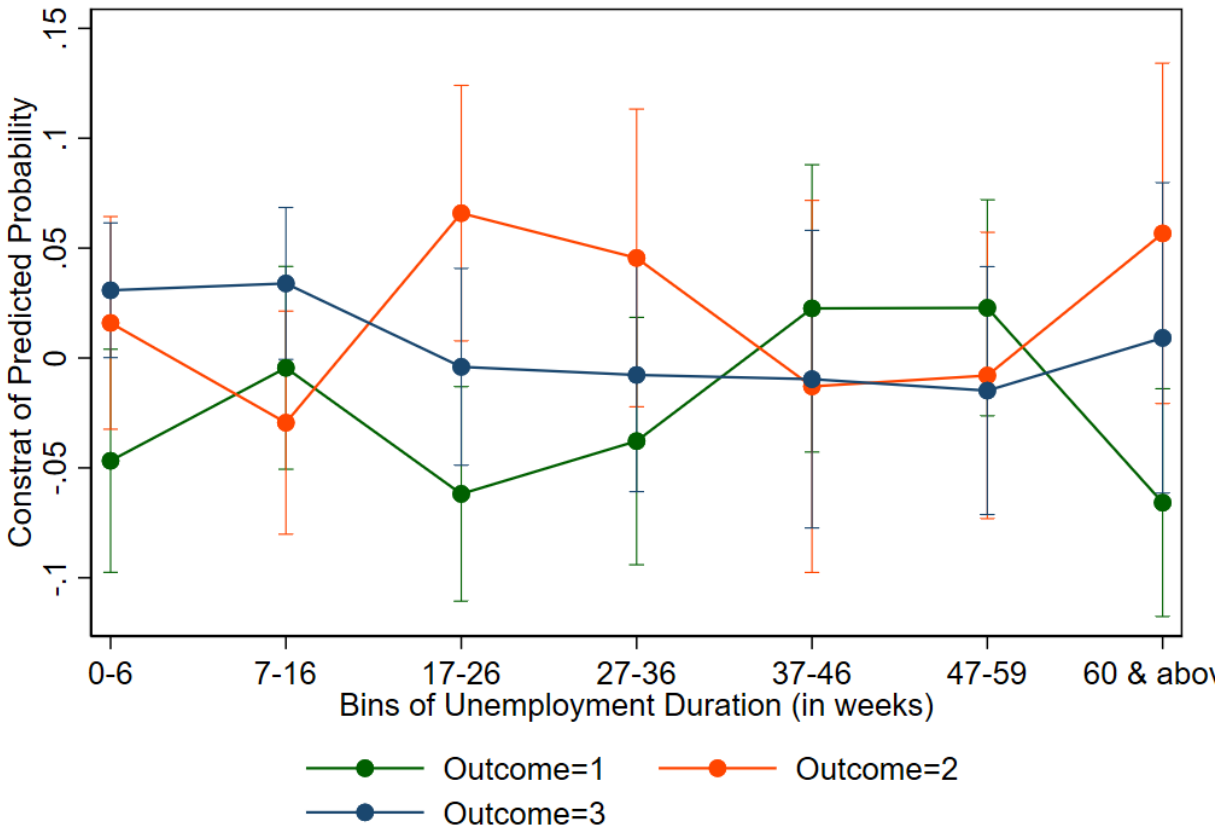

Figure 8. Contrast of predicted probabilities of labor market transitions by the availability of PEUC. Outcome 1 / 2 / 3 = Employed / Unemployed / NILF.

Appendix B: CPS Cleaning

The CPS dataset contains two vital variables regarding an individual's labor market status: unemployment duration and employment status. When a worker reports that he/she is unemployed, transitioning from other labor market status in the previous month, or when he/she is in the incoming rotation group, the worker reports unemployment duration in weeks. Nonetheless, the employment status variable and the unemployment duration variable exhibit many cases of inconsistencies. When a worker reported "employed" or "NILF" in the previous month's survey, the worker cannot logically be unemployed for more than five weeks. However, these logical conditions are often violated. Here are the four rules that I used, one on top of another one, to clean the unemployment duration variable. In each step, I create two different versions, one version top-codes the inconsistencies, and the other version recodes the inconsistent data to missing values. This gives me 4 * 2 + 1 = 9 different unemployment duration variables and nine different constant duration bins and variable durations accordingly.

Table 5. Correlation between unemployment duration variables created by different rules

	Original	Rule A	Rule A+B	Rule A+B+ C	Rule A+B+C+D
Original	1.0000				
Rule A	0.9772	1.0000			
Rule A+B	0.9671	0.9899	1.0000		
Rule A+B+ C	0.9207	0.9429	0.9491	1.0000	
Rule A+B+C+D	0.9077	0.9282	0.9367	0.9803	1.0000

<u>Rule A: the increase in unemployment duration between cannot exceed the number of weeks between the two survey rounds</u>

In rule A, I looked for and corrected the cases when there is a leap in the reported unemployment duration. I defined a maximum increase of unemployment duration:

$$Max_{t,t+i} = 4 * i + 1, 1 \leq i \leq 3$$

Here, i denotes the number of months between two survey observations. I added 1 to account for the potential noise due to the survey dates. For instance, if an individual has an unemployment duration of 2 weeks in the first month, they shall at most report 7 weeks, 11 weeks, and 15 weeks in the second, third, and fourth observation if the individual is to remain unemployed throughout this period. Suppose the reported duration is above the maximum increase defined above. In that case, I either recode it with missing values or replace the reported duration with the maximum potential unemployment duration of the individual. Table 5 shows that the original duration variable and the duration variable created by rule A have a correlation of 97.72%; i.e., 2.28% of the observations were amended by rule A.

<u>Rule B: insufficient increase in unemployment duration between survey rounds</u>

In rule B, I looked for the cases where unemployment insurance between survey rounds did not increase "enough". I defined a minimum increase of unemployment duration:

$$Min_{t,t+i} = 4 * i - 1, 1 \leq i \leq 3$$

Again, here i denotes the number of months between two survey observations. I subtracted 1 to account for the potential noises due to the survey dates. For instance, if an individual has an unemployment duration of 2 weeks in the first month, they shall report at least 5 weeks, 9 weeks, and 13 weeks in the second, third, and fourth observation, if the individual is to remain unemployed throughout this period. Then, I count the number of type-B inconsistencies an

individual has after applying rule A. For individuals with only one inconsistency, I trust the lagged reported duration and recode the inconsistent duration either with the minimum increase defined above or missing values. If an individual has multiple inconsistencies, I flag data quality issues for this individual and replace all inconsistent unemployment duration observations with missing values. It is possible that despite an individual reported being unemployed in four consecutive surveys, the individual was temporarily employed, and thus their record of unemployment duration was "renewed". I, therefore, made no adjustments to observations with reported unemployment duration less than or equal to five weeks. According to Table 3, the correlation between the original duration variable and the duration variable created by rule B is 96.71%; i.e., an additional 1.01% of the observations were amended by rule B after applying rule A.

Rule C: inconsistencies between the employment status variable and the unemployment duration variable

You might have realized that both Rule A and Rule B correct for inconsistencies within the report unemployment duration. In rule C, I looked for inconsistencies between the employment status variable and the unemployment duration variable. Specifically, an unemployed person who was employed 2 months ago cannot have the unemployment duration for more than 9 weeks[27]. I defined a maximum duration of unemployment duration, conditioned on the individual being employed i months ago:

[27] I allow past NILF status since it is well-documented that people sometimes misreport themselves as being NILF during a long unemployment spell (Clark and Summers 1982).

$$(Max|E_t)_{t+i} = 4 * i + 1$$

If the reported unemployment duration after rule A and B is above this maximum, I either top code it or recode it with missing values. According to Table 3, the correlation between the original duration variable and the duration variable created by rule C have a correlation of 92.07%; i.e., 4.64% additional observations were amended by rule C after applying rule A and rule B.

Rule D: Removing short-term changes between unemployment spells

Lastly, I remove short-term changes between unemployment spells. I ignore temporary transitions from unemployment spells and treat U-E-U and U-N-U as non-exit. Farber, Rothstein and Valletta (2015), Fujita (2010), and Rothstein (2011) all adopt a similar approach, as they were concerned about the likelihood of spurious transitions due to mismeasurement of labor force status. However, I must note that treating all temporary exits from unemployment as non-exit is a strong assumption. According to Table 3, the correlation between the original duration variable and the duration variable created by rule D have a correlation of 90.77%; i.e., 1.30% additional observations were amended by rule D after applying rule A, B, and C.

In all results reported, I used the unemployment duration variable obtained after applying rule C. In comparison to rule C, rule A and rule B do not clean enough, whereas rule D has the tendency to over-clean. I prefer the top-coded version of rule C because top-coding does not reduce the sample size. Regressions run with unemployment duration variable obtained by applying other rules in general gave quantitatively consistent estimates, and these results are ava

Appendix C: Tables

LM Transitions	EB	PEUC	Additional Benefit Amount
U-E	-0.003	-0.001	-0.006
	(0.004)	(0.004)	(0.004)
U-U	0.005	0.004	0.006
	(0.004)	(0.004)	(0.004)
U-N	-0.001	-0.003	-0.000
	(0.002)	(0.003)	(0.002)
Observations	25,411	25,411	25,411
R-squared			
Standard errors in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

Table 6. Average Marginal Effects of an additional month of unemployment on the probabilities of labor market transitions from model (2).

LM Transitions	Without EB	EB=6		EB=7		EB=8	
		AME	Δ	AME	Δ	AME	Δ
U-E	-0.022***	-0.054***	-0.032**	-0.083***	-0.061**	-0.055**	-0.033
	(0.001)	(0.013)	(0.013)	(0.024)	(0.024)	(0.027)	(0.027)
U-U	0.014***	0.042***	0.028**	0.047*	0.033	0.046**	0.032
	(0.001)	(0.012)	(0.012)	(0.027)	(0.027)	(0.021)	(0.021)
U-N	0.008***	0.012*	0.004	0.035*	0.027	0.009	0.001
	(0.001)	(0.007)	(0.007)	(0.021)	(0.021)	(0.011)	(0.011)

LM Transitions	EB=10		EB=13		EB=20	
	AME	Δ	AME	Δ	AME	Δ
U-E	-0.055***	-0.033**	-0.026***	-0.004	-0.024***	-0.002
	(0.013)	(0.013)	(0.002)	(0.003)	(0.002)	(0.003)
U-U	0.044***	0.030**	0.019***	0.005**	0.015***	0.001
	(0.012)	(0.012)	(0.002)	(0.002)	(0.002)	(0.002)
U-N	0.012*	0.003	0.007***	-0.001	0.009***	0.001
	(0.006)	(0.006)	(0.001)	(0.001)	(0.001)	(0.001)

Table 7. Average marginal effects of an additional month of unemployment on labor market status, per the level of Extended Benefits available (from model(1)). Columns under AME display the average marginal effects, and columns under Δ shows the difference in AME with the base category (no EB).

	U-E		U-U		U-N	
Unemployment Duration Bins	EB	PEUC	EB	PEUC	EB	PEUC
0-6 weeks	0.022*	-0.047*	-0.011	0.016	-0.012	0.031**
	(0.013)	(0.026)	(0.013)	(0.025)	(0.009)	(0.016)
7-16 weeks	0.062***	-0.004	-0.053***	-0.029	-0.009	0.034*
	(0.014)	(0.024)	(0.015)	(0.026)	(0.011)	(0.018)
17-26 weeks	0.002	-0.062**	0.034*	0.066**	-0.037**	-0.004
	(0.017)	(0.025)	(0.020)	(0.030)	(0.016)	(0.023)
27-36 weeks	0.035	-0.038	-0.007	0.046	-0.027	-0.008
	(0.021)	(0.029)	(0.025)	(0.035)	(0.019)	(0.027)
37-46 weeks	0.013	0.023	0.004	-0.013	-0.017	-0.010
	(0.027)	(0.033)	(0.033)	(0.043)	(0.025)	(0.035)
47-59 weeks	0.044**	0.023	-0.026	-0.008	-0.018	-0.015
	(0.022)	(0.025)	(0.026)	(0.033)	(0.022)	(0.029)
60 weeks and above	-0.017	-0.066**	0.024	0.057	-0.007	0.009
	(0.027)	(0.026)	(0.041)	(0.040)	(0.037)	(0.036)
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Table 8. Contrast of average predicted probability from model (3). Each column shows the differences in the predicted probabilities of being employed/unemployed/out of labor force (conditioning on being unemployment currently) by the availability of EB/PEUC UI extensions at different length of unemployment spells.

Variable	Age Group (Base Category: 25-29 years old)					Gender	Marital Status	Has a child below 8
Category	30-34	35-39	40-44	45-49	50-54	Female	Married	Yes
U-E	0.002	0.017*	-0.011	0.014	-0.024**	0.001	0.042***	-0.006
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.006)	(0.006)	(0.007)
U-U	0.025**	0.003	0.028**	0.013	0.050***	-0.054***	-0.038***	-0.005
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.006)	(0.007)	(0.008)
U-N	-0.027***	-0.020**	-0.017**	-0.027***	-0.026***	0.053***	-0.004	0.011*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.005)	(0.005)	(0.006)

Variable	Educational Attainment (Base Category: High School Graduates)			Any Disability	Race-Ethnicity (Base Category: Non-Hispanic White)				
Category	Below High School	Bachelor's Degree	Master or Doctorate	Yes	NH Black	NH American Indian	NH Asian	NH other/mixed	Hispanic (any race)
U-E	0.013	-0.005	0.054***	-0.056***	-0.042***	-0.027	-0.037***	0.027	0.013
	(0.010)	(0.007)	(0.011)	(0.012)	(0.009)	(0.025)	(0.012)	(0.022)	(0.008)
U-U	-0.041***	0.047***	0.003	0.010	0.011	-0.070**	0.013	-0.035	-0.026***
	(0.011)	(0.008)	(0.012)	(0.014)	(0.010)	(0.028)	(0.013)	(0.023)	(0.009)
U-N	0.028***	-0.041***	-0.057***	0.047***	0.031***	0.097***	0.023**	0.008	0.013**
	(0.009)	(0.006)	(0.008)	(0.011)	(0.007)	(0.023)	(0.010)	(0.017)	(0.006)

Table 9. Average Marginal Effects of Individual Characteristics.

*(p<0.01 ***, p<0.05 **, p<0.10 *)*