Do Greater Unemployment Benefits Lead to Better Matches? Evidence from Emergency Unemployment Compensation Programs

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Abstract

This paper presents new evidence on why unemployment insurance (UI) benefits might lengthen job search durations. Motivated by Chetty's (2008) finding that additional benefits have larger effects on unemployment durations for liquidity-constrained households, I examine whether the level of household wealth has an impact on occupational changes for unemployed individuals. I then exploit Propensity Score and Nearest Neighbor Matching methods to estimate the treatment effect of Emergency Unemployment Compensation Acts on job seekers' occupational choices. I find that an increase in UI benefit duration allows the unemployed to make larger changes in job-specific tasks relative to their pre-unemployment jobs.

Key words: unemployment durations, occupational choice, unemployment benefits

JEL codes: J64, J62, J65

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1 Introduction

Budget-constrained job seekers may encounter serious consumption drops while unemployed (Gruber (1997), Browning and Crossley (2001)). Pressed by an urgent need for funds, they might be forced to take any available jobs, even if those jobs do not necessarily match their skills appropriately, with the subsequent result that such workers might experience lower productivity and earn less than what they could have earned if they were working on tasks more suitable their competitive advantages.

Past empirical research has found that UI benefits reduce the labor supply. (Moffitt (1985), Katz and Meyer (1990), Card and Levine (2000)). This trade-off between benefits and disincentives is central to the design of UI systems and to discussions about the generosity of UI benefits. A great number of studies have attempted to understand why UI benefits lengthen periods of unemployment, and there are two main explanations. The first hypothesis is moral hazard from a substitution effect; recipients of UI reduce their search efforts, as UI benefits can distort the relative costs of leisure and consumption (Krueger and Meyer (2002), Gruber (1997)). Second, in response to higher benefits, the reservation wage may go up, such that the probability of a UI recipient's accepting a new job offer diminishes (Ehrenberg and Oaxaca (1976)).

Alternatively, Chetty (2008) suggests that a substantial share of the response to longer UI benefits periods is attributable to a liquidity effect; UI allows liquidity-constrained households to spend as much time as they would have spent if they had enough funds for searching. In this way, UI benefits increase aggregate utility. Whether or not a job seeker's job-match quality positively correlates with the length of time spent searching is still an open question, with mixed evidence. Most existing empirical studies measure match quality using the post-unemployment wage or using tenure at the new job (Card et al. (2007), Chetty (2008), Van Ours and Vodopivec (2008)); however, while studies do find small-but-positive effects, others find none.

In this paper, I present new empirical evidence to suggest occupational choices as a channel by which unemployment insurance (UI) benefits might affect search behavior and result in longer unemployment durations. Recent literature on occupational tasks (Autor et al. (2003), Bacolod et al. (2009), Yamaguchi (2012), Autor and Dorn (2013)) allowed researchers to evaluate a new dimension of occupational mobility: how occupations differ in terms of the types of job tasks they entail – that is, cognitive, manual, or routine skills. Using the occupational task data constructed by Autor and Dorn (2013), I show a positive correlation between changes in occupational tasks (i.e., from one task type to another or task levels) and unemployment durations. This evidence suggests that it takes longer to search and be matched with an occupation that requires skillsets or skill levels that differ from previous jobs. In turn, such a finding raises an interesting question: does additional wealth allow workers to take the risk of spending more time on their job searches, in order to find a better career match?

In this essay, thus, I report two findings that shed light on the positive relationship between additional wealth and the possibility of "experimentation" in the job search. Firstly, those with higher household wealth switch their job tasks more substantially than those with less wealth. I further measure a match quality with a previous job by pre-unemployment wage residuals, and find that, regardless of the level of household wealth, those whose previous occupation was a bad match are more likely to change their job tasks. Secondly, I show that an extension in the duration of UI benefits appears to induce occupational change.

My empirical strategy is closely related to Chetty (2008) and Kroft and Notowidigdo (2016), and I use cross-state variations in unemployment benefit durations during the early '90s. In 1991, the Extended Unemployment Compensation (EUC) program was established to increase the number of weeks of benefits during high-unemployment periods, to protect people who remain unemployed longer due to the greater difficulty in finding new jobs. As the additional benefit durations depended on state-level unemployment rates, UI durations varied across states. Although benefit levels and qualifications were revised five times during the EUC, one feature of the program that remained consistent was that these benefits were provided in two tiers through all periods. In addition, the unemployment rates during these periods were highly correlated across time. Therefore, I group the states into two categories to create a treatment status, depending on whether a state received higher-tier benefits or not.

Given the cross-state variations in UI benefit durations, then, I use data from the Survey of Income and Program Participation (SIPP) panels of 1990-1994, which cover dates before and after the EUC program's inception in late-1991. I estimate the average treatment effects of receiving UI benefits for longer durations, using the Difference in Differences and Matching methods. Using the Difference in Differences method, I find that occupational changes are more likely to be observed in the states that qualify for higher-tier EUC after EUC is implemented, compared to states that do not qualify. I then estimate the average treatment effect using Matching methods; I exploit both Nearest Neighbor and Propensity Score matchings to test the effect of the EUC on occupational changes and find consistent results using both methods.

The findings in this paper are consistent with the results in Chetty (2008): namely, that increases in UI benefits have much larger effects on unemployment durations for liquidity-constrained individuals than for wealthier ones. If it is to be expected that an individual must spend a longer time searching for an occupation that has no (or little) connection to his or her prior experience, and if unemployed people utilize UI benefits to subsidize these extended periods of unemployment, then it follows that increases in UI benefits will have larger effects on search durations for people who are financially constrained.

Although a change in occupation does not, in itself, constitute direct evidence for an improvement in job suitability or in post-unemployment welfare, this finding still sheds light on the possibility that UI extensions might facilitate improvements in welfare. First of all, people who are able to extend their job search until finding a better match might experience wage growth in the long run. Like many previous studies, the current paper reports that there is little change in accepted wages when comparing an individual's wages from his/her the final year at the previous job compared to his/her wages in the first year at the new job. However, given that individuals who change their occupation might reasonably lack occupation-specific experience at the start of a new job, this lack of experience might account for their lower starting wages. Taking this possibility together with another finding in this paper – namely, that individuals whose previous jobs were a poor match are more likely to switch occupations – it is possible that workers are freer to seek jobs with a better fit when they have the option of leaning on the support of extended UI benefits. Second, by selecting new, more-suitable occupations, workers might experience improvements in welfare that are associated with non-pecuniary job preferences.

The paper proceeds as follows. Section 2 presents a short summary of the previous literature on UI and post-unemployment match quality. Section 3 introduces the datasets used for the empirical analysis. Section 4 examines the effects of EUC extensions on search outcomes, namely, wage growth and occupation. Then, in Section 5, I estimate average treatment effects of EUC extensions, using the Difference in Differences and Matching methods. Section 6 discusses the evidence for the positive relationship between occupational change and search duration, and Section 7 provides a sketch of an occupational search model for credit-constrained individuals. Lastly, Section 8 concludes.

2 Previous studies on post-unemployment match quality

Given the universal empirical findings on the positive relationship between the generosity of UI benefits and unemployment spells, there is little evidence to show that generous UI benefits actually result in improved post-unemployment match quality. In other words, it has thus far remained unclear whether UI is associated with wage gains or with longer job tenures upon re-employment.

In Ehrenberg and Oaxaca (1976), the authors analyze unemployment duration and wage gains, using samples of UI recipients and non-recipients. They estimate the effect of the UI replacement ratio, which is defined as the ratio of weekly UI benefits to the UI recipient's weekly earnings at his/her former job. Their results show that the UI replacement rate does not have a significant impact on post-employment wages, and these results have been viewed as evidence for moral hazard in UI programs, as well as evidence that UI might reduce recipients' job-search efforts. Similarly, using Continuous Wage and Benefit History data for Pennsylvania and Arizona, Classen (1977) estimates the effect of an increase in the weekly benefit amount on post-unemployment wages and does not find UI to have a statistically significant effect on accepted wages in the post-unemployment job.

Addison and Blackburn (2000), on the other hand, do find a small but statistically significant effect in support of post-unemployment wage gains for UI recipients (compared to non-recipients) using data from the Displaced Worker Surveys for 1988, 1990, and 1992. The authors note that their finding may be biased by virtue of their decision to compare the recipients with non-recipients, as a similar effect is not found when comparing recipients at different levels of benefits.

More recently, Lalive (2007) has studied the effects of small (13-week) and large (170-week) extensions of UI benefits in Austria. The author finds that the more time-generous benefit programs seem to lengthen unemployment durations; however, these do not affect post-unemployment match quality, as measured by re-employment wage gains.

Using data from Austria and Slovenia, respectively, two recent studies – Card et al. (2007) and Van Ours and Vodopivec (2008) – examine multiple aspects of match quality, such as post-unemployment job duration and the probability of finding a permanent rather than a temporary job, in addition to wage changes. However, again, these authors find UI benefits to have little or no effect on post-unemployment match quality.

The current paper adds to the previous literature by testing and identifying the effects of UI programs on a new aspect of post-unemployment job match: occupational change. Occupational change alone is not direct evidence for improvements in worker-job match quality. However, this new aspect of search behavior sheds light on potential welfare improvements, taken together with two other findings in this paper; first, workers who were found to be poorly matched with the previous job also tend to experience larger shifts in terms of job tasks, and secondly, additional wealth have positive impacts on occupational changes.

3 Data

3.1 Dictionary of Occupational Titles

Starting from Autor et al. (2003), a growing body of studies (Ingram and Neumann (2006), Bacolod et al. (2009), Yamaguchi (2012), Autor and Dorn (2013)) take a new approach to define occupations, using task data from the Dictionary of Occupational Titles (DOT) or from its successor, the Occupational Information Network (O*NET). DOT and O*NET contain detailed task information on 12,099 distinct occupations. Each occupation is evaluated with respect to 62 characteristics, such as aptitudes, temperaments, necessary training time, and physical demand. Ingram and Neumann (2006), Bacolod et al. (2009), and Yamaguchi (2012) categorize these job characteristics by assigning them to just one of two dimensions – cognitive or motor – and define each occupation by task intensity. By contrast, Autor et al. (2003) and Autor and Dorn (2013) consider three skill dimensions – abstract, manual, and routine – in analyzing the allocation of tasks between labor and capital, due to technological changes in the labor market.

In this paper, I use the three continuous task measures (abstract, routine, and manual) established by Autor and Dorn (2013). The measures are constructed from the DOT and matched to their corresponding three-digit Census occupation classifications. They collapse the original five task measures of Autor et al. (2003) to three task aggregates. The abstract task measure is the average of two DOT variables: "direction control and planning" and "GED Math," which measure managerial, mathematical, and formal reasoning requirements. The routine task measure is the average of two DOT variables: "set limits, tolerances, and standards" and "finger dexterity." And the manual task measure corresponds to the DOT variable "eye-hand-foot coordination."

Table 1 indicates the average task intensities for five major occupation groups. Managerial and professional specialty occupations, on average, have the highest abstract task scores, while Precision production, craft, and repair occupations have the highest routine and manual task scores. The lowest abstract task score was for operators, fabricators, and laborers, and the lowest routine task score was observed in the service occupations. Finally, the lowest manual task score was found among technical, sales, and administrative support occupations.

An advantage of using task-based occupational definitions rather than traditional categorization methods is that task-based definitions allow for evaluating whether two distinct occupations are similar or not. In addition, continuous task measures carry a computational advantage. In this way, despite the fact that the tripartite dimensional scheme accounts for a very low objective number of job characteristics, these categories (in combination) still account for theoretically infinite types of work; thus, researchers can still work conveniently with a large number of occupations.

3.2 Extended Unemployment Compensation

In the United States, UI benefits are normally provided for 26 weeks under the federal Unemployment Compensation (UC) program established by the Social Security Act of 1935. The UC program is periodically extended by a permanent Extended Benefits (EB) program or by temporary programs during economic downturns to protect people who (during a downturn) remain unemployed longer-than-normal due to the (temporarily) greater difficulty in finding new jobs. The permanent EB program was enacted in 1970 and provides one-half of regular benefits up to a maximum of 13 weeks. This program can be activated in a specific state if its adjusted insured unemployment rate (AIUR)[1] for 13 weeks is 4% or higher and if the quarterly average is at least 20% higher than the average of the previous 2 years. Meanwhile, the EB program can be activated nationally when the national IUR is 4.5% or higher for at least 3 consecutive months.

A temporary program, the Emergency Unemployment Compensation (EUC) Act of 1991[2] was established to increase the duration of UI benefits during periods of high unemployment. The EUC program was signed into law November 15, 1991, and paid benefits through April 30, 1994. The EUC was superseded the EB program. A state that triggered on to EB had to drop from it in order to qualify for EUC. Also, an individual's EUC entitlement was reduced by any EB received under the EUC program.

The EUC program was revised five times, creating a complex web of benefit durations and levels across states. During that time (i.e., its November 1991 inception through the end of April 1994), a total of $27.9 billion in benefits was paid to recipients, and 5 million individuals exhausted their EUC benefits. Benefit durations and benefit tiers depended on the legislation of the time, across the five different iterations of the program's terms and conditions, as well as on state-level unemployment rates. Although the benefit levels and qualifications were revised five times, benefits were provided in effectively just two tiers[3] throughout all periods.

[1]The insured unemployment rate is defined as the average of continuing UC claims for 13 weeks, divided by the average number of individuals in UC-covered employment over the first 4 of the last 6 quarters.

[2]Source: Emergency Unemployment Compensation: the 1990's Experience, Revised Edition, U.S. Department of Labor Employment and Training Administration,VI Occasional Paper 99-4. January 1999.

[3]The first legislation that was effective from November 17, 1991 to February 8, 1992 had three benefit tiers: 20, 13, and 6 weeks added to a recipient's regular unemployment compensations. However, all states were qualified and received either 20 or 13 additional weeks of unemployment benefits.

Figure 1 shows a summary of cross-state variations in total weeks of UI benefits available during EUC periods. Two graphs in panel A indicate the high and low tiers of UI benefits, and panel B shows the mean and standard deviations across states. The spread between the two tiers was typically 6-7 weeks, and both the means and the standard deviations across states increased drastically from late-1991 to early-1992, decreasing thereafter.

Table 2 shows the additional weeks of EUC benefits for each tier by legislation, and the number of weeks benefits that each state received. Data on Puerto Rico and the Virgin Islands are not available. Furthermore, Maine, Vermont, Iowa, North Dakota, South Dakota, Alaska, Idaho, Montana, and Wyoming are excluded, as SIPP does not provide unique state identifiers. The table, thus, includes the 14 states that qualified for Tier 2 benefits for at least one week during the legislative periods, while the remaining 27 states remained always in Tier 1. We can see that most states were in Tier 2 during the fourth and fifth legislative periods. For this reason, I focus only on the first three legislative periods, from November 17, 1991 to March 5, 1993.

Given the complexity of temporary UI laws during this period and the inadequate information on the date of UI claims, it is difficult to predict each individual's benefit level precisely. Therefore, I group the states into two categories, depending on the benefit tier in which each state found itself during the first three legislative periods, in order to construct a treatment variable. However, since the benefit level was able to change at any time depending on the state's unemployment rates, 12 out of 41 states are 'misclassified' for some weeks. The last column of Table 2 indicates the number of weeks deviated from Tier 2 from November 17, 1991 to March 5, 1993, where the deviation is 0 if a state was in Tier 2 the entire time and 68 if it was in Tier 1 the entire time. I use two measures of the treatment status, τ_b and τ_s, where $\tau_b = 1$ if the number of weeks deviated from Tier 2 is less than 34 weeks, and $\tau_s = 1$ if less than 20 weeks.

The two dummy variables τ_b and τ_s are time-invariant. However, as Table 2 shows, the number of potential weeks by state often changes over different periods. Therefore, in addition to the two treatment status variables, τ_b and τ_s, I also test allowing the impacts of the EUC to vary by legislation period. For unemployment spells that stretch continuously across multiple periods, I assume the potential weeks to be based on the beginning of their unemployment spells.

3.3 Survey of Income and Program Participation

The ideal unemployment data for this paper are panels that include individual records of pre- and post-unemployment periods, as before-and-after comparison allows for a true measure of occupational change. Also, data on the availability of household assets are crucial in determining the effects that wealth might have on occupational change. At the same time, the ideal dataset should be time-expansive enough to contain observations both pre- and post-dating the policy intervention. Considering all of these factors, I use data from the 1990, 1991, and 1992 panels of the Survey of Income and Program Participation (SIPP), starting from January 1990 to August 1994; this date range is possible because each SIPP panel entails household surveys that continue for 2-4 years from the starting date, at 4-month intervals. The SIPP data contain information on weekly employment status, UI benefit status, and household assets.

To measure search durations, I follow Chetty (2008). I use weekly employment status (ES) from the SIPP data. ES can take any one of the following values: 1. With a job this week; 2. With a job, absent without pay, no time on layoff this week; 3. With a job, absent without pay, spent time on payoff this week; 4. Looking for a job this week; 5. Without a job, not looking for a job, not on layoff. Following Chetty (2008), I also define the duration of job separation by summing the number of weeks that ES $>=3$, starting at the time of job separation (i.e., when a change in ES from 1 or 2 to 3, 4, or 5 first becomes apparent) and stopping when the individual finds a job that lasts for at least 4 weeks (i.e., respondent reports on 4 consecutive occasions that ES $=$ 1 or 2). The search duration is defined as a period of active job search, summing the number of weeks that a respondent reports that ES $=$ 4. In case the tenure on a new job is less than 1 month, the search duration is calculated by summing the number of weeks in which ES $=$ 4, until the person finds another job that lasts for 4 weeks, and the number of weeks in between wherein ES $=$ 1 or 2 is excluded.

I restrict the samples to prime-age males between the ages of 18 and 65 who have at least 3 months of work history and appear in a panel for at least 3 months. The unemployment start date is considered to be when a worker with at least 3 months of work history becomes separated from a job. I exclude those who experienced their first job separation after March 1993, in order to cover the first three of the five EUC legislations. I further restrict the sample to those who are matched with a new job within the sample periods, excluding those who were still unemployed by the end date of the panel in question. I also exclude anyone on temporary layoff. In the end, I include only those people who lost jobs on or before March 1993, and the unemployment spells go as late as August 1994. Some have multiple unemployment spells in the data and have multiple observations. The final sample consists of 4,502 unemployment spells, 3,709 individuals, and an average of 1.21 unemployment spells per person.

To focus on the first three EUC legislations, I select only people who lost jobs on or before March 1993, leaving 1,248 unemployment spells that starts separations after March 1993 truncated. We may observe search outcomes and total search durations only for those who are successfully matched with new jobs during the panels. Therefore, individuals with shorter search durations are more likely to be selected in the sample, resulting a right-censoring problem. To adjust for this selection bias, I use a two-step sample correction method developed by Heckman (1979).

Table 3 presents summary statistics for the SIPP samples used in this paper. Note that wage information is missing for some samples that have records for pre- and post-unemployeemnt occupations. The average tasks before and after unemployment are similar to each other. Scores for abstract, routine, and manual tasks before job separation are 2.234, 4.34, and 1.7, respectively; in post-unemployment occupations, these are 2.221, 4.28, and 1.714, respectively. The average of the task scores across all 3-digit Census occupations are, respectively, 2.886, 4.627, and 1.308. Therefore, the unemployment SIPP samples have relatively lower abstract and routine tasks compared to the average across all occupations, while the SIPP is higher than the average on manual tasks.

The average ln wage is also similar before and after job separation. The average ln wage before unemployment is 5.520 for the whole samples, and 5.592 for those who have post-unemployment wage data, while the mean post-unemployment ln wage is 5.677.

4 Effects of UI on Post-Unemployment Wages and Occupations

As in the previous studies discussed in Section 2, I do not find any evidence to suggest that UI increases a job seeker's accepted wages after unemployment. As in Card et al. (2007), I define wage growth $h_i = ln(w_i^n) - ln(w_i^p)$ where w_i^n is individual i's wage in the first year at the post-unemployment job, and (w_i^p) is the wage in the final year at the previous job.

To evaluate effects of UI on post-unemployment wages, I use only samples after EUC is implemented to estimate the following OLS regression,

$$h_i = \mu_0 + \mu_1 \tau_i + \theta \tilde{X}_{it} + \epsilon_{it} \tag{1}$$

A treatment status $\tau_i = 1$ indicates the eligibility for the Tier 2 UI extension as defined in the previous section. Controls $\tilde{X}_{it}$ include search durations, age, age squared, years of education, a race dummy, and quartiles of household wealth distribution.

In addition, I test whether the UI benefit extension has affected occupational changes, using a similar specification, but replacing wage growth h_i to the distance between pre- and post-unemployment occupations, D_i. D_i is measured by the Euclidean distance in three-dimensional occupational tasks, according to Autor and Dorn (2013) 's categories: abstract, routine, and manual.

$$D_i = (T_{iA}^p - T_{iA}^n)^2 + (T_{iR}^p - T_{iR}^n)^2 + (T_{iM}^p - T_{iM}^n)^2 \tag{2}$$

T_{iA}^k, T_{iR}^k, and T_{iM}^k are abstract, routine, and manual task intensities, respectively, and k indicates pre- $(k = n)$ and post- $(k = p)$ unemployment. Therefore, D_i measures how different the new occupation is compared to the previous job.

Tables 4 and 5 present summary statistics of covariates by the treatment statuses τ_b and τ_s and their differences. Both wage and occupational distance are higher in the treated group, and the difference is bigger between $\tau_s = 1$ and $\tau_s = 0$ compared to the difference between $\tau_b = 1$ and $\tau_b = 0$. Similar trend is found in search duration (weeks) and net liquid household assets. Black respondents make up about 10% of the whole sample, and the population is slightly larger in the control groups. The three job task measures, as well as age and years of education, are not significantly different between the two groups.

The regression results in Table 6 are also consistent with the findings in existing studies on the effects of UI on accepted wages. Table 6 examines the effects of treatment status τ, the eligibility for the longer UI extensions. Columns (1) and (3) include only age and its squared term as controls, and Columns (2) and (4) add the full control set, including education, a race dummy, household wealth distribution quartiles, and search durations. Regardless of how the treatment group is defined, for both τ_b and τ_s, a respondent's eligibility for the longer UI extensions does not have a statistically significant effect on his post-unemployment wage growth, h_i. The coefficients of the treatment status for all four regressions are positive, but close to 0 and not significant. Search durations do not have any significant effect on wage growth either, as shown in columns (2) and (4).

Estimation results in Table 7 show that the treatment status τ, however, affects post-unemployment occupational choices. Changes in occupational task, D_i increase with eligibility for longer UI extensions. Both treatment status τ_b and τ_s have similar levels of coefficients without additional controls, 1.848 and 1.949, respectively. With full controls, then, the coefficients on τ_b and τ_s are 1.150 and 1.658, respectively, and the τ_b coefficient is not statistically significant. Search durations – that is, weeks of unemployment while searching for a job – are also positively related to occupational change, as shown in Columns (2) and (4). Therefore, those who spend longer searching for a job are more likely to switch occupational tasks when reemployed.

Although an immediate improvement in wages is not evident, occupational changes as a response to UI benefit extensions do imply that not all behavioral responses to more generous UI benefits are explained by moral hazard; when they can avail themselves of more funds during the search period, it seems that workers may expand their searches to include new occupations with which they have no previous experience. This explanation is consistent with the findings in Chetty (2008) that increases in UI benefits have much larger effects on unemployment durations for liquidity-constrained individuals. If it is to be expected that searching for a new occupation will require more time than searching for a more familiar one, and if unemployed people utilize UI benefits to subsidize extended periods of unemployment while they find a new job, then increases in UI benefits should have larger effects on search duration when people are credit-constrained.

In the following section, I will explore the effects of UI extensions on occupational change in more detail, using the Difference in Differences and Matching methods.

5 Average Treatment Effects of UI on Occupational Changes

5.1 Difference in Differences Analysis

The outcome of interest is the distance between the observed pre- and post-unemployment occupations, D_i. Using a treatment status $\tau_i = 1$, eligibility for Tier 1 UI extension, I first analyze the average treatment effect of EUC on occupational change by the conventional DID approach. $T = 0$ indicates pre-EUC spells that began before November 1991, even if a given spell ended after November 1991, and $T = 1$ indicates post-EUC spells that began in or after November 1991.

Table 8 shows the average occupational distance by treatment status for pre- and post-spells. We can see that the occupational distance is greater in the treatment group on average. Moreover, it increases after EUC in the treatment group, while it decreases after EUC in the control group. Difference in difference is higher for $\tau_s = 1$ than $\tau_b = 1$.

$$D_{it} = \beta_0 + \beta_1 g_i + \beta_2^j Q_{ij} + \beta_3 \mathbf{1}_T + \beta_4 \mathbf{1}_\tau + \beta_5 \mathbf{1}_T \mathbf{1}_\tau + \gamma X_{it} + \epsilon_{it}. \tag{3}$$

Q_{ij} is an indicator variable that $=$ 1 if individual i belongs to quartile j of the wealth distribution. And $\mathbf{1}_T = 1$ indicates a time period after the treatment event. Control X_{it} includes current occupational task levels, age. To interpret β_5 as an average treatment effect of EUC, it is required that the average outcomes for the treated and control groups would have followed parallel paths over time in the absence of the treatment. Finally, g_i denotes the wage signal for individual i derived from the following ln wage regression.

$$ln \text{ wage}_{it} = \alpha_{0t} + \alpha_1 \text{task} + \alpha_2 \text{task}^2 + \gamma' X'_{it} + \mu_i + \delta_t \eta_{it}, \tag{4}$$

where X'_{it} includes educational attainment, race dummies, age, and age squared. μ_i represents state fixed effects, and δ_t represents year fixed effects. Wage signal g_i is the residual from the ln wage equation (4). Therefore, g_i, which is the excessive wages from the previous occupation, is used as a proxy for the match quality from the previous job. Workers make occupational choices based on their past experience. In particular, workers can change occupations to insure themselves against earnings risks attributable to a poor match with job tasks. Workers whose previous occupations were a bad match might adapt themselves to different kinds of tasks, and those who are well matched in their current occupations would likely stay with similar occupations (in terms of job tasks) going forward.

5.2 Truncated Distribution and Correction for Sample Selection Bias

The dependent variable distance D_i is observed only if an individual is successfully matched with a new job within the panel in question, resulting in a right-censoring problem. Individuals with shorter unemployment spells are more likely to be included in the sample. To adjust for this selection bias, I use the two-step sample correction method developed by Heckman (1979).

The occupational distance D_i is observed only if

$$\gamma_0 + \gamma_1 n_i + \gamma_2 \tilde{X}_i + \mu_i + u_i > 0. \tag{5}$$

n_i indicates the year and month when individual i is separated from a job. n_i is a monthly time variable, starting from January 1990 where $n_i = 1$ to March 1993 where $n_i = 39$. The later that an individual lost his job, it would be more likely that he was not ultimately matched successfully with a new job and thus was omitted from the data. $\tilde{X}_{it}$ includes search durations, age, age squared, years of education, race dummy, and quartiles of household wealth distribution, while μ_i represents state fixed effects.

Table 9 shows estimates from the Probit regression in equation (5). Calendar year and month at job losses and the current search durations are negatively related to the sample selection. On the other hand, older people and people who engaged more in (as represented by higher scores on) abstract tasks in their previous job are more likely to find a job before the panel ends and are included in the sample. From selection equation (5), I construct the non-selection hazard, or the inverse of Mill's ratio $lambda_i = \frac{\theta(Z_i)}{1-\Theta(Z_i)}$, where Z_i is the predicted selection probability.

5.3 Results

Table 10 shows the classic difference in differences estimation result. Specification (1) does not include the non-selection hazard, *lambda*, while Specifications (2), (3), and (4) include it to correct the selection bias caused by the truncated distribution of occupational distance. In addition to *lambda*, Specification (3) includes dummies for each legislation period for EUC, and Specification (4) includes legislation period dummies and their interactions with EUC status in each state for that period. For some states that changed their EUC status within a given period, I assigned 1 if the state was in the higher tier for the majority of that time; otherwise I assigned 0.

For all four specifications, the average treatment effect of EUC is positive and statistically significant, and the estimates are higher for τ_s than τ_b. The coefficient is larger and more significant with the controls for legislation period dummies $p1, p2$ their interactions with EUC status in each state for that period. The negative time specific effect was strongest during the first legislation period, which is in the beginning of the recession. And the effect of EUC weakest in the last period of legislation, p.

Regardless of the treatment status, household assets play an important role in occupational task changes. Q_j is an indicator variable for the jth quartile of the wealth distribution. Those who are in the 3rd and 4th quartiles are more likely to switch their jobs more drastically compared to the people in the 1st and 2nd quartiles. There are two possible explanations for this finding. The first explanation centers on the fact that unemployment search duration is highly correlated with occupational change; the data show that the bigger a change is, the longer it has likely taken a worker to be successfully matched with his new job – and we can surmise that only those who are not liquidity-constrained can afford to wait through longer searches. The finding here supports Chetty (2008)'s findings that the effect of unemployment benefit extension on search duration is much larger for workers with low household liquidity.

The second explanation is consistent with the positive relationship between wealth and optimal levels of occupational risk taking, as addressed in Bahk (2020). If workers are uncertain about their skill levels, then choosing occupations that require very different (combinations of) occupational tasks is risky, since a worker's productivity and potential earnings in the new occupation would be unknown; as such, only those workers with greater wealth might be able to take on the bigger risks associated with changing one's occupation substantially.

Another key variable in Table 10 is the signal from equation (4). The negative coefficients on the signal suggest that workers with low productivity signals from their previous job are more likely to make bigger changes in occupational tasks, and those with good signals are matched again with rather similar occupations after unemployment. The estimates are similar in all specifications and range from -1.523 to -1.566. This result is consistent with the findings in Arcidiacono et al. (2016) that workers with positive wage residuals are more likely to stay in the same occupation. Wage regression results, as well as the estimates exploited to derive the signals, are presented in Table 11.

Also, the occupational changes drop significantly with age, which is consistent with the classic findings in the search and matching literatures that occupational change is particularly active in the beginning of a worker's career. Meanwhile, although unemployment rates have strongly negative correlations with search durations, they do not have a significant impact on occupational change.

5.4 Matching Methods

5.4.1 Methods and Results

One of the key benefits of randomized experiments in estimating causal effects is that both observed and unobserved covariates in the treated group are only randomly different from the control group. Unfortunately, in many non-experimental studies, the status of having received a treatment is not always independent of the treated units' characteristics; in such cases, if the treated units' outcomes are at least partly determined by some of these factors, the treatment process itself may result in selection bias (Rubin (1973), Heckman et al. (1998)). Therefore, when estimating causal effects in non-experimental studies, it is desirable to reduce bias as much as possible by obtaining well-matched treated and control groups with similar covariates.

As shown in Tables 4 and 5, which present summary statistics of covariates in treated and control groups and their differences, a simple comparison suggests that there is a room to improve the balance between the two groups. For both broad (τ_b) and narrow (τ_s) distinctions, there are significant discrepancies in the distributions, although the differences are bigger in the narrow treatment group. *ln* wage, signal, and occupational distance are slightly higher in the treated group, as well as household net liquid assets. Job task measures, years of education, and age are not significantly different between the two groups.

Matching methods (Althauser and Rubin (1970), Rubin (1973)) are based on the idea of balancing the distribution of covariates in the treated and control groups to compare the outcomes of subjects that are as similar as possible with the single exception of their treatment status. Matching methods include matching in covariates (Abadie and Imbens (2002)), and methods based on propensity score (Rosenbaum and Rubin (1984), Hirano et al. (2003)). In this paper, I use both nearest neighbor matching and propensity score matching to compare similar units between the treatment and control group.

Nearest neighbor matching entails finding the closest pairs of observations with regard to a set of covariates.

$$\hat{D}_i(0) = \begin{cases} D_i & \text{if } \tau_i = 0 \\ \bar{D}_{l(i)} & \text{if } \tau_i = 1 \end{cases} \tag{6}$$

$$\hat{D}_i(1) = \begin{cases} \bar{D}_{l(i)} & \text{if } \tau_i = 0 \\ D_i & \text{if } \tau_i = 1 \end{cases} \tag{7}$$

$\bar{D}_{l(i)}$ indicates the average of Mth closest units (in terms of covariates X_i) in the opposite treatment group, where M is the number of matches. Then the simple matching estimator in Abadie and Imbens (2002) is

$$\Gamma^{sm} = \frac{1}{N}\sum_{i=1}^{N}(\hat{D}_i(1) - \hat{D}_i(0)). \tag{8}$$

However, unless the covariates are exactly matched, there may still be bias due to the difference in the covariates, even though the difference is smaller after matching. Regardless, to account for this possible bias, I adjust using a linear function of covariates as suggested in Abadie and Imbens (2002).

Propensity score matching is an alternative to the nearest neighbor matching method. Instead of correcting the bias that may arise in cases where all covariates are not exactly matched, propensity score matching matches on the estimated predicted probabilities of treatment, also known as the propensity scores. The propensity score naturally includes all information about the covariates and can perform as a single covariate for use in matching.

Table 12 shows the matching estimators. The matched covariates comprise the three task-type measures, a wage signal g_{it} from the pre-unemployment job (equation (3)), household net liquidity quartiles, age, race, and years of education. Since the available sample includes a large number of controls and the treated, I used a single match in the estimation. Selecting multiple matches generally increases bias, since the second or next-closest controls are further away from the treated unit than is the absolute closest match. However, when the sample size is small, utilizing multiple matches can decrease variance by increasing the matched sample size. For both treatment groups, the matching estimators for the average treatment effect are significantly positive, ranging from 1.284 to 1.855.

5.4.2 Discussions for Unconfoundedness and Overlap assumptions

There are two assumptions critical to identifying the treatment impact using matching methods. The first assumption is "unconfoundedness" which is also referred to as exogeneity (or the conditional independence assumption), formally articulated in Rubin (1990),

$$\tau_i \perp (D_i(0), D_i(1)) \; |X_i. \tag{9}$$

Conditional on the covariates X_i, the outcomes δ_i are independent with the treatment status τ_i. Unconfoundedness is an important condition when estimating casual effects using observational data, which assures that the assignment to treatment is based on observational pre-treatment variables only. In many non-experimental studies, assessing the plausibility of the unconfoundedness assumption can be a challenge. In this study, the assignment rule for $\tau_i = 1$ is clear: it depends on a single variable, which is the state's unemployment rate. However, there may exist unobserved systematic differences in the covariates that affect occupational changes and this unemployment rate, hence the treatment and control status.

The unconfoundedness assumption is not testable, since only one of the outcomes for each treatment status is observable. However, researchers can assess the plausibility of this assumption by estimating the causal effect of the treatment on a pseudo-outcome, which is a variable known to be unaffected by the treatment. I use the samples before the treatment status; therefore, the value is determined prior to the treatment, to perform (otherwise-identical) matching estimations. Table 13 reports that for all four specifications, the matching estimators are insignificant and close to 0, showing that the unconfoundedness assumption is plausible.

The second assumption is overlap. The overlap assumption states that each individual has a positive probability of receiving the treatment. Formally, the overlap assumption requires that for each possible X in the population,

$$0 < \Pr(\tau = 1|X) < 1. \tag{10}$$

where $\Pr(\tau = 1|X)$ is a propensity score. The overlap assumption is satisfied when there is a positive probability of seeing observations in both the treatment and the control group given each combination of covariates. Figure 2 shows the two estimated densities of the predicted probabilities of being treated using the covariates X. Both plots have its mass in the middle, not near 0 or 1, and there is a sufficient overlap between the two groups. Therefore it is plausible to say that the overlap assumption is not violated.

6 Search Durations and Changes in Occupation

In this section, I provide evidence for a positive relationship between occupational changes and search durations using a simple OLS regression. There are various kinds of potential risk involved in choosing a new job that entails different kinds of job tasks. A model of task-specific job searching (Bahk (2020)) shows that job seekers with higher household assets tend to move further in terms of (change in) occupational tasks. At the same time, a positive relationship between occupational changes and search durations suggests another potential risk when changing occupations; it may take longer to search for jobs outside of one's field of experience.

$$\text{search durations}_i = \zeta_0 + \zeta_1 D_i + \zeta_2 w_i + \zeta_3 g_i + \gamma' X_i' + \nu_i \tag{11}$$

Table 14 shows estimation results for the OLS regression (equation (9)), where X_i includes state unemployment rates, year-specific effects, and individual characteristics, such as age, years of education, race, and household liquidity asset levels. Search durations are measured as the weeks of unemployment periods people have reported that they are actively searching for a job.

Search durations are likely extended with higher unemployment rates. Also as people age, it is likely that finding a new job will take them longer. Household net liquidity assets, on the other hand, do not seem to have a significant impact on job search durations. While productivity signals from a respondent's previous job are negatively correlated with search duration, this relationship might relate to unobserved characteristics, such as search efforts or innate ability – or it may be partly attributable to the fact that people with low wage signals from their previous jobs are more like to choose a different sort of job, involving different tasks. Interestingly, then, when controlling for the productivity signal, higher wages at the previous job are positively correlated with unemployment duration. Lastly, search duration and occupational change show a positive and significant relationship.

7 An Occupational Search Model

In this section, I sketch a simple occupational search model. This model builds on Card et al. (2007) and on Chetty (2008), who developed job search models with a borrowing constraint. One major difference from the search models is that the agents choose occupational distance (i.e., the magnitude of the change in occupational task types) d_t when they become unemployed, instead of search efforts. In addition, utility when employed depends on the match quality with a new occupation. I make the following two assumptions for simplicity: first, I assume that all jobs last indefinitely once matched, and second, I assume that wages are exogenously fixed.

Time is discrete in a finite horizon. Agents become unemployed at $t = 0$. An agent chooses occupational distance $d_t \in [0, \bar{d}]$. If $d_t = 0$, then the occupation is same as in the previous job. Distance d_t affects the probability of a successful match, $p(d_t)$, which I assume $p'(d_t) < 0$. Therefore, as d_t increases, the agent is less likely to be matched with a new job. δ denotes a time discount rate, and r is the fixed interest rate. $m(d_t, g)$ denotes match quality in the new job, where the match quality is determined by the productivity signal from the previous occupation g and by the distance d_t. If the search is successful, the agent begins working and receive wage w_t until the end of the periods. If the agent fails to find a job in period t, the agent receives an unemployment benefit b_t and searches again in period $t + 1$.

The value function for an agent who are matched with a job in period t, given the assets A_t is

$$V_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - \frac{A_{t+1}}{(1+r)} + w_t) + m(d_t, g) + \frac{1}{1+\delta} V_{t+1}(A_{t+1}), \tag{12}$$

where L is a lower bound on assets. The value function for an agent who fails to find a job is

$$U_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - \frac{A_{t+1}}{(1+r)} + b_t) + \frac{1}{1+\delta}, J_{t+1}(A_{t+1}), \tag{13}$$

where

$$J_t(A_t) = \max_{d_t} p(d_t) V_t(A_t) + (1 - p(d_t)) U_t(A_t). \tag{14}$$

An unemployed individual chooses d_t to maximize expected utility given by equation (13). Given the level of assets A_t and the productivity signal g from the previous occupation, the first order condition that the optimal distance $d_t^* \in (0, \bar{b})$ is

$$p'(d_t^*)(V_t(A_t) - U(A_t)) + p(d_t^*) \frac{\partial m(d_t^*, g)}{\partial d_t^*} = 0, \tag{15}$$

and the optimal distance $d_t^* = 0$ if $\frac{\partial m(d_t, g)}{(\partial d_t)} \leq 0$ given g.

The optimal occupational distance is determined by the productivity signal g. Intuitively, an unemployed individual who was well matched at his previous occupation would be better off looking for the same job tasks or even the same exact occupation. However, if the match quality was very low, the agent might choose $d_t > 0$ in order to increase his match quality at his next job, even if the probability of a successful match (in such a case) is lower.

8 Conclusion

In this paper, I suggest and test occupational choices as a new channel between UI benefits and longer unemployment durations. Using cross-state variations in weeks of UI benefits available in the early 1990s, I find that unemployed individuals with higher levels of wealth search for different kinds of jobs with different task levels than do individuals with lower levels of wealth. Also, using different levels of EUC extensions as a treatment status, I find similar behavioral responses to UI benefits extensions, as though these benefits function as a stand-in for wealth. I control for previous occupation and previous job's match quality, and I find that people tend to "experiment" and move further away from their previous occupations when they are supported by UI for longer periods.

The occupational choices of the unemployed offer insight into a "new" possible facet of the value of UI, one that has previously not received enough attention. In particular, the fact that people whose previous jobs were a poor match are more like to change their occupational tasks when they have greater household assets or more generous UI benefits highlights some potential welfare improvements for credit-constrained workers. However, occupational change itself dose not provide direct information about workers' post-unemployment welfare, such as post-unemployment occupational tenure, match quality at these new jobs, or the question of whether workers are more satisfied with their new occupations.

Assessing the value of the occupational changes facilitated by UI can be a future avenue of research. One potential reason why post-unemployment wage levels are not affected by UI extensions in this study – while occupational choices are – is because of how the current study has defined wage growth. Wage growth is measured by the difference between wages during the final year in the previous occupation and wages during the first year in the new one. Even if individuals find a better occupational match by changing job tasks, the monetary payoffs may not be immediate when they change job tasks completely, because they may yet lack valuable task-specific experience.

Moreover, observations on occupational choice across a longer span of time might afford a better understanding of post-unemployment occupational tenure. Using the task-based occupation data, with the measure for the direction of occupational movement in hand, we can evaluate how an individual's professional focus – as measured by job task type – is evolving over time.

In both cases, long-panel data would allow researchers to ascertain whether there were any long-term benefit to task changes. These, and any further evaluations for post-unemployment welfare in a context of occupational change, are left for future studies.

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Table 1: Average Task Intensity of Major Occupation Groups

	Abstract	Routine	Manual
Managerial and professional specialty	5.558	3.682	0.980
Technical, sales, and administrative support	2.559	4.801	0.506
Service	1.629	2.803	1.512
Precision production, craft, and repair	1.955	6.655	1.879
Operators, fabricators, and laborers	1.165	4.792	1.788

The table indicates the average task intensities (abstract, routine, and manual) for five major occupational groups. Three continuous task measures are established by Autor and Dorn (2013). The measures are constructed from the DOT and matched to their corresponding three-digit Census occupation classifications.

Table 2: Treatment Status and EUC Benefit Durations (Weeks) by State and Law

	11/17/91 P.L.102-182	2/8/92 P.L. 102-244	6/14/92 P.L.102-318	3/6/93 P.L.103-6	10/2/93 P.L.103-152	Dev. from Tier 2
Tier 1	13	26	20	10	7	
Tier 2	20	33	26	15	13	
Arkansas	13 20(2/2/92)	33	20	10	7	49
California	13 20(1/5/92)	33	26	15	13	7
Connecticut	20	33	26 20(11/1/92)	10	7	18
Massachusetts	20	33	26 20(8/2/92)	10	7	31
Michigan	20	33	26 20(10/25/92)	10	7	19
Mississippi	20	33 26(2/16/92)	20	10	7	51
Nevada	13	26 33(3/8/92) 26(6/6/92)	20	10	7	55
New Jersey	20	33	26 20(11/22/92)	10 15(3/7/93) 10(6/13/93)	7	15
New York	13	26 33(2/16/92)	26 20(7/12/92)	10	7	47
Oregon	13 20(1/12/92)	33	26 20(9/27/92) 26(1/31/93)	15 10(7/11/92)	7 13(2/26/94)	26
Pennsylvania	13 20(1/26/92)	33	26 20(8/16/92)	10 15(3/21/93) 10(6/20/93)	7	39
Rhode Island	20	33	26	15	7 13(1/16/94)	0
Washington	13 20(2/2/92)	33	26 20(7/4/92)	15 10(6/27/93)	7	41
West Virginia	20	33	26	15	13	0

Source: Emergency Unemployment Compensation: the 1990's Experience, Revised Edition, U.S. Department of Labor Employment and Training Administration,VI Occasional Paper 99-4. January 1999. Data on Puerto Rico and Virgin Islands are not available. Maine, Vermont, Iowa, North Dakota, South Dakota, Alaska, Idaho, Montana, Wyoming are excluded as SIPP does not provide unique state identifiers.

Table 3: Summary Statistics

	Mean	S.D.	Observations
Worker Characteristics			
Age	32.089	10.806	4,627
Education	12.264	2.737	4,627
Black	0.109	0.312	4,627
Net liquid assets	17280.31	72502.38	4,627
Pre-unemployment			
ln wage	5.520	0.854	4,502
Abstract task	2.334	2.063	4,627
Routine task	4.340	2.238	4,627
Manual task	1.700	1.474	4,627
Post-unemployment			
Search duration	13.857	13.424	4,627
ln wage	5.677	0.764	3,452
Abstract task	2.221	1.979	4,627
Routine task	4.280	2.236	4,627
Manual task	1.714	1.477	4,627

This table provides summary statistics of data samples from the 1990, 1991, and 1992 panels of the Survey of Income and Program Participation (SIPP), starting from January 1990 to August 1994. The samples are restricted to prime-age males between the ages of 18 and 65 who have at least 3 months of work history and appear in a panel for at least 3 months. To focus on the first three EUC legislations, I select only people who lost jobs on or before March 1993.

Table 4: Summary Statistics by Treatment Status τ_b

	$\tau_b = 0$		$\tau_b = 1$			
	Mean	S.D.	Mean	S.D.	Difference	t-stat
ln wage	5.488	0.839	5.584	0.881	-0.096	-3.525
Distance	10.491	14.842	11.390	16.149	-0.899	-1.852
Age	32.235	10.895	31.722	10.617	0.513	1.495
Education	12.244	2.685	12.300	2.865	-0.055	-0.226
Black	0.128	0.334	0.071	0.256	0.057	5.821
Search duration	13.003	12.540	15.202	14.208	-2.198	-5.279
Net liquid assets	13,293	55,484	26,149	99,658	-12,856	-5.549
Abstract task	2.325	2.044	2.388	2.131	-0.063	-0.961
Routine task	4.336	2.226	4.290	2.265	0.046	0.649
Manual task	1.703	1.473	1.676	1.484	0.028	0.586
Unemployment rate	6.567	1.192	7.935	1.421	-1.368	-33.869
Signal	0.000	0.699	0.00	0.712	0.000	0.000
N	3,029		1,473			

This table provides summary statistics by treatment status τ_b, where $\tau_b = 1$ if the number of weeks deviated from Tier 2 EUC benefit is less than 34 weeks. Data on Puerto Rico and the Virgin Islands are not available. Furthermore, Maine, Vermont, Iowa, North Dakota, South Dakota, Alaska, Idaho, Montana, and Wyoming are excluded, as SIPP does not provide unique state identifiers.

Table 5: Summary Statistics by Treatment Status τ_s

	$\tau_s = 0$		$\tau_s = 1$			
	Mean	S.D.	Mean	S.D.	Difference	t-stat
ln wage	5.492	0.844	5.596	0.879	-0.104	-3.572
Distance	10.515	14.905	11.552	16.302	-1.037	-1.999
Age	32.079	10.852	32.036	10.678	0.043	0.117
Education	12.299	2.656	12.161	2.984	0.137	1.472
Black	0.121	0.327	0.075	0.264	0.046	4.372
Search duration	13.252	12.748	15.061	14.144	-1.809	-4.058
Net liquid assets	14,196	61,375	26,885	98,777	-12,689	-5.119
Abstract task	2.239	2.050	2.393	2.135	-0.064	-0.912
Routine task	4.340	2.235	4.268	2.251	0.072	0.946
Manual task	1.696	1.478	1.691	1.475	0.004	0.087
Unemployment rate	6.636	1.200	8.092	1.461	-1.456	-33.681
Signal	0.000	0.705	0.000	0.699	0.000	-0.000
N	3,330		1,172			

This table provides summary statistics by treatment status τ_s, where $\tau_s = 1$ if the number of weeks deviated from Tier 2 EUC benefit is less than 20 weeks. Data on Puerto Rico and the Virgin Islands are not available. Furthermore, Maine, Vermont, Iowa, North Dakota, South Dakota, Alaska, Idaho, Montana, and Wyoming are excluded, as SIPP does not provide unique state identifiers.

Table 6: Effects of UI on Post-Unemployment Wage Growth

	τ_b		τ_s	
	No controls (1)	Full controls (2)	No controls (3)	Full controls (4)
τ	0.031	0.041	0.0023	0.035
	(0.039)	(0.040)	(0.042)	(0.042)
Search durations (weeks)		-0.0005		-0.0004
		(0.002)		(0.002)
N	1,785	1,785	1,785	1,785

All specifications control for age and age squared. Full controls includes search durations (weeks), years of education, race dummy, and household net liquidity wealth (quartiles). τ_b and τ_s indicate the treatment status; an eligibility for tier 1 UI extensions. Standard errors clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effects of UI on Post-Unemployment Occupational Changes

	τ_b		τ_s	
	No controls	Full controls	No controls	Full controls
	(1)	(2)	(3)	(4)
τ	1.848**	1.150	1.949**	1.658**
	(0.776)	(0.773)	(0.832)	(0.825)
Search durations (weeks)		0.205***		0.206***
		(0.031)		(0.031)
N	1,785	1,785	1,785	1,785

All specifications control for age and age squared. Full controls includes search durations (weeks), years of education, race dummy, and household net liquidity wealth (quartiles). τ_b and τ_s indicate the treatment status; an eligibility for tier 1 UI extensions. Standard errors clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Difference in Differences in Occupational Distance by Treatment Status

	T	Control	Treatment	Difference
	0	10.805	11.405	0.600
		(15.190)	(15.552)	
τ_b	1	10.221	11.617	1.396
		(14.566)	(16.894)	
	DID			0.796
	0	10.861	11.315	0.454
		(15.270)	(15.358)	
τ_s	1	10.227	11.792	1.565
		(14.547)	(17.229)	
	DID			1.111

$T = 0$ indicates periods before EUC is implemented, and $T = 1$ indicates post-EUC periods. Standard deviations in parenthesis.

Table 9: Selection Equation

	Selection
n	-0.013***
	(0.002)
Search duration	-0.026***
	(0.001)
Age	0.038***
	(0.011)
Age squared	-0.001***
	(0.000)
Net liquidity	
Q2	-0.264***
	(0.057)
Q3	-0.087
	(0.058)
Q4	-0.087
	(0.059)
Abstract task	0.027**
	(0.012)
Routine task	0.005
	(0.009)
Manual task	0.006
	(0.015)
Constant	-0.322
	(0.296)
N	6,292

Quartiles for household net liquidity is used to measure liquid asset levels. Standard errors clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Difference in Differences Estimation

	(1)		(2)		(3)		(4)	
	τ_b	τ_s	τ_b	τ_s	τ_b	τ_s	τ_b	τ_s
Signal	-1.566***	-1.565***	-1.539***	-1.537***	-1.523 ***	-1.523***	-1.539***	-1.537***
	(0.297)	(0.297)	(0.280)	(0.280)	(0.276)	(0.276)	(0.278)	(0.278)
Net liquidity								
Q2	-0.637	-0.654	-1.698***	-1.703***	-1.676***	-1.681***	-1.660***	-1.672***
	(0.469)	(0.469)	(0.503)	(0.497)	(0.496)	(0.490)	(0.501)	(0.494)
Q3	1.134**	1.124***	0.800	0.796	0.805	0.802	0.801*	0.803
	(0.469)	(0.480)	(0.487)	(0.485)	(0.484)	(0.482)	(0.481)	(0.479)
Q4	1.625***	1.620***	1.150**	1.150***	1.175**	1.174**	1.176**	1.174**
	(0.527)	(0.525)	(0.529)	(0.529)	(0.538)	(0.537)	(0.531)	(0.529)
Age	-0.124***	-0.125***	-0.152***	-0.153***	-0.151***	-0.151***	-0.151***	-0.152***
	(0.024)	(0.024)	(0.026)	(0.025)	(0.025)	(0.025)	(0.026)	(0.026)
τ	-0.033	-0.290	-0.529	-0.826	-0.520	-0.815	-0.521	-0.817
	(0.751)	(0.815)	(0.646)	(0.834)	(0.759)	(0.830)	(0.762)	(0.832)
T	-0.300	-0.387	-1.049***	-1.136***	-0.775	-0.861	-0.355	-0.358
	(0.468)	(0.458)	(0.482)	(0.468)	(0.625)	(0.604)	(0.641)	(0.647)
$\tau \times$ T	1.314**	1.856**	1.227**	1.766***	1.234*	1.781***	1.804**	2.991***
	(0.669)	(0.701)	(0.646)	(0.668)	(0.641)	(0.659)	(0.847)	(0.862)
Nonselection hazard			10.058***	10.053***	9.877***	9.866***	9.853***	9.812***
			(1.639)	(1.644)	(1.564)	(1.570)	(1.556)	(1.573)
p1					-1.219	-1.229	-1.984*	-2.161*
					(0.804)	(0.807)	(1.074)	(1.160)
p2					-0.123	-0.125	-1.500	-1.547
					(1.246)	(1.244)	(0.928)	(0.933)
p1 $\times$ EUC tier at p1							1.270	0.785
							(1.283)	(1.460)
p2 $\times$ EUC tier at p2							1.819	1.430
							(1.343)	(1.403)
p3 $\times$ EUC tier at p3							-2.170	-2.994**
							(1.306)	(1.390)
Constant	20.974***	20.862***	12.706***	12.789	12.637***	12.719***	13.102***	13.280***
	(3.817)	(3.819)	(3.615)	(3.619)	(3.613)	(3.617)	(3.647)	(3.657)
N	4,502	4,502	4,502	4,502	4,502	4,502	4,502	4,502

All specifications control for the three tasks, abstract, routine, and manual, and their square terms. Distance measures occupational changes between pre- and post-unemployment occupations. τ_b and τ_s indicate treatment status. Quartiles for household net liquidity is used to measure liquid asset levels. $T = 1$ indicates times after EUC is implemented. Standard errors clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Wage Regressions

	ln wage
Abstract task	0.050**
	(0.021)
Abstract task squared	0.005*
	(0.002)
Routine task	-0.026
	(0.025)
Routine task squared	0.008***
	(0.003)
Manual task	-0.024
	(0.033)
Manual task squared	0.010
	(0.006)
Age	0.125***
	(0.007)
Age squared	-0.001***
	(0.000)
Black	-0.177***
	(0.041)
Constant	3.120***
	(0.182)
N	4,502

Residuals from this equation is the productivity signal from the pre-unemployment occupations. The regression is controlled by year and state specific effects, and years of education. Standard errors clustered by state in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Matching Estimation

	Nearest neighbor matching		Propensity score matching	
	(1)	(2)	(3)	(4)
	τ_b	τ_s	τ_b	τ_s
ATE	1.284*	1.333*	1.649**	1.855**
	(0.748)	(0.796)	(0.817)	(0.889)
N	2,403	2,403	2,403	2,403

Matching estimators for the average treatment effects on occupational changes between pre- and post-unemployment occupations. The number of match is 1. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Assessing Unconfoundedness:
Estimates of Average Treatment Effects for Pseudo Outcomes

	Nearest neighbor matching		Propensity score matching	
	(1)	(2)	(3)	(4)
	τ_b	τ_s	τ_b	τ_s
ATE	-0.525	-0.383	-0.042	-0.042
	(0.815)	(0.881)	(0.814)	(0.875)
N	2,099	2,099	2,099	2,099

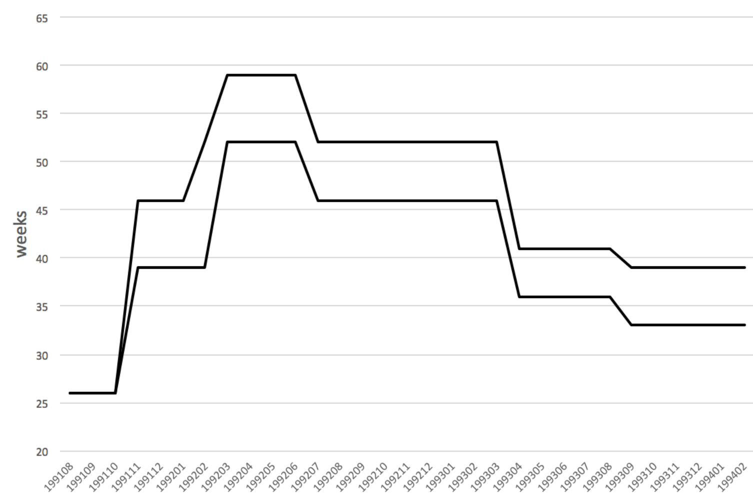
Matching estimators for the average treatment effects on the pseudo outcomes; occupational changes between pre- and post-unemployment occupations before the EUC is implemented. The number of match is 1.

Table 14: Relationship Between Search Durations and Occupational Changes

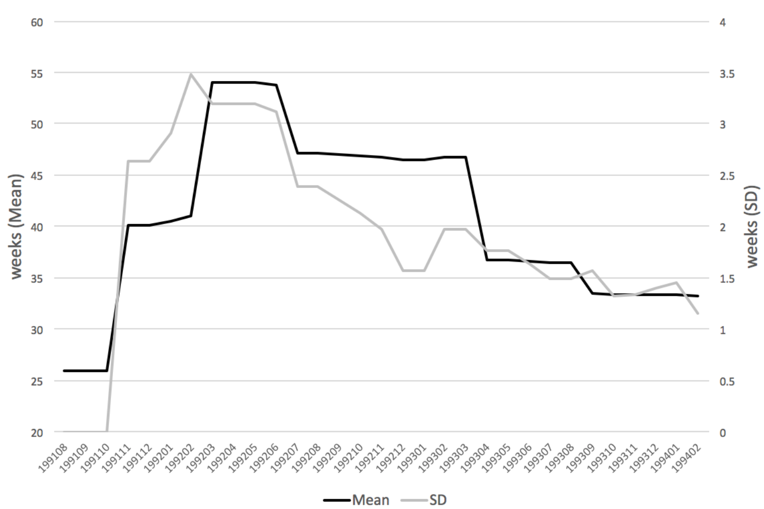
	Search durations (weeks)
Distance	0.114***
	(0.014)
ln wage	1.693***
	(0.454)
Signal	-2.075***
	(0.554)
Unemployment rate	0.767***
	(0.163)
Net liquidity	
Q2	0.753
	(0.563)
Q3	-0.130
	(0.511)
Q4	0.662
	(0.731)
Age	0.070***
	(0.020)
Education	-0.244**
	(0.075)
Black	4.358***
	(0.790)
Constant	-4.137
	(2.287)
N	4,502

Distance measures occupational changes between pre- and post-unemployment occupations. Quartiles for household net liquidity is used to measure liquid asset levels. Standard errors clustered by state in parentheses. The regression is controlled by year specific fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Variation in Total Weeks of UI Benefits Available



A. Maximum and Minimum Across States



B. Mean and Standard Deviation Across States

Figure 2: Propensity Score Overlap