NBER WORKING PAPER SERIES

DO UNEMPLOYMENT INSURANCE BENEFITS IMPROVE MATCH AND EMPLOYER QUALITY? EVIDENCE FROM RECENT U.S. RECESSIONS

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Working Paper 27574
http://www.nber.org/papers/w27574

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2020 , Revised April 2022

This research uses restricted microdata from the U.S. Census Bureau's Longitudinal Employer Household Dynamics (LEHD) Program, which was partially supported by the following National Science Foundation Grants SES-9978093, SES-0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. All results have been reviewed to ensure that no confidential data are disclosed. All results have been approved by the Disclosure Review Board. Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. This research was performed at the Georgetown Federal Statistical Research Data Center under FSRDC Project Number 1812. All errors are our own. We acknowledge the generous financial support from the Washington Center for Equitable Growth. We would like to thank George Akerlof, Joe Altonji, Heather Boushey, David Card, Raj Chetty, Larry Kahn, Kory Kroft, Jesse Rothstein, Emmanuel Saez, and Ann Stevens for comments on this paper, and especially Matt Notowidigdo for his thoughtful suggestions and Henry Hyatt for advise on the use of the algorithm to measure AKM and use of LEHD data. Adriana Kugler is the corresponding author. The views expressed herein are those of the authors and do not necessarily reflect those of their employers nor of the National Bureau of Economic Research.

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JEL No. C55,E24,H23,J21,J31,J64,J65

ABSTRACT

Unemployment Insurance (UI) benefits have a moral hazard effect and a liquidity effect, with both generating increases in unemployment spells but the latter increasing wages due to the ability to find better matches or better jobs. Previous papers, however, find mixed evidence on the impact of UI on wages. In this paper, we re-examine the effect of UI on wages in the U.S. and present novel evidence using LEHD data to examine the channels through which UI increases earnings, including: (1) the quality of the match, (2) positive sorting of employers and employees, and (3) the quality of the employer. We find that the increased UI generosity in the U.S. increased wages by both increasing the quality of the match as well as the quality of the job obtained after the unemployment spell, though there is less evidence of improved sorting. Consistent with improvements in match and employer quality, we also find that the likelihood of remaining on the job increases with UI generosity. Consistent with a liquidity effect on search, we also find that the effects on the quality of the match are larger for those who are more likely to be liquidity constrained, including women, minorities, and the less-educated.

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1 Introduction

Unemployment insurance (UI) has long been seen as a government benefit that helps workers keep up their consumption through periods of unemployment. A robust finding in the empirical literature is that UI benefits are positively associated with longer unemployment spells.[1] There are two interpretations for this finding. On the one hand, a broad literature highlights UI benefits may generate a "moral-hazard effect." Workers who receive UI benefits may put less effort in the search of new jobs staying unemployed for longer and facing negative or null impacts on re-employment wages.[2] On the other hand, some recent studies argue that more generous UI benefits may lead to longer unemployment spells because credit-constrained workers search longer for better job matches. This mechanism is commonly denoted as the "liquidity effect." Card et al. (2007) and Chetty (2008) find that the "liquidity effect" accounts for most of the impact of UI on prolonging unemployment durations.[3] In addition, contrary to most studies, Nekoei and Weber (2017) find a positive relationship between UI extensions and re-employment wages supporting the view that UI may improve match quality.

In this paper, we study the reasons why the extensions of UI weeks granted during past U.S. recessions may improve wages. To our knowledge, this is the first paper to decompose the sources of earnings gains resulting from more generous UI benefits. We investigate these mechanisms using disaggregated data that allows us to decompose earnings gains due to more generous UI benefits, including: (1) the quality of the match, (2) positive sorting of workers and firms, and (3) the quality of the employer. We also examine if more generous UI prolongs the likelihood that a worker stays employed with the same employer, which could be consistent with all three mechanisms—finding a better match, finding a better job or better sorting. Finally, we explore if the gains from UI benefits are greater for workers who are more likely to be liquidity-constrained. Understanding the mechanisms may help reconcile the different findings that other studies have documented, as positive impacts on firm quality raise wages unambiguously, while improved match quality has ambiguous effects on wages and positive sorting not based on productive complementarities may even reduce wages because of compensating differentials or have no effect on wages.

Our work is motivated by a number of theoretical contributions which show that UI extensions may lead to better employment outcomes and higher productivity by correcting distortions in the labor market. Acemoglu and Shimer (1999) argue that more generous UI benefits encourage risk-averse workers to seek higher productivity jobs leading to a compositional change in the vacancies posted by firms. Marimon and

[1] Despite this consensus, the magnitude of the effect varies across studies. Earlier research such as Moffitt (1985), Katz and Meyer (1990), Meyer (1990), and Card and Levine (2000) find that extensions in the UI benefits program substantially prolong the unemployment spell. More recent studies such as Rothstein (2011), Farber and Valletta (2015), and Farber et al. (2015) about UI extensions during and after the Great Recession find small but significant decreases in the probability of leaving unemployment, mostly driven by the decrease in the likelihood of moving out of the labor force and with a little decline in the likelihood of exiting to employment.

[2] See, among the others, Moffitt (1985), Katz and Meyer (1990), Meyer (1990), Card and Levine (2000), Card et al. (2007), Lalive (2007), van Ours and Vodopivec (2008), and Schmieder et al. (2016).

[3] Card et al. (2007)) argue that the representative job seeker is much closer to credit-constrained behavior than to the permanent income hypothesis. Chetty (2008) quantifies that 60% of the increased duration of unemployment spells is due to the "liquidity effect" and the remaining 40% to the "moral hazard effect."

Zilibotti (1999) present a search-matching model with risk-neutral agents and two-sided ex-ante heterogeneity that predicts extensions in UI benefits reduce employment, but improve job matches. In Marimon and Zilibotti's (1999) framework, better matches are formed by improved positive assortative matching in the labor market and decreased skill-mismatch bias. Fuller et al. (2014) have proposed a multi-sector model with sector-specific productivity shocks and moving costs across sectors. In their framework, UI benefits allow workers to cover moving costs across sectors and increase the probability of switching industries, insuring individuals against low future wage prospects from searching in relative less productive sectors.[4] These papers consider each of the effects of UI on firm quality, match quality and sector switching one at a time, while in this paper we consider all of these mechanisms together.

Intuitively, extensions in the duration of UI benefits prolong the unemployment spells by making the value of being unemployed higher for a longer periods. Thus, the threshold for the acceptance of a job offer increases for an unemployed worker who can claim UI benefits. In a Diamond-Mortensen-Pissarides (DMP) model with ex-ante two-sided heterogeneity and match-specific quality that affects production, workers who benefits from UI extensions stay unemployed for longer by rejecting offers that are bad matches. As a consequence, UI extensions lead to an improvement in the average quality of the realized employer-employee matches. Similarly, with an increasing production function in worker and firm quality, extensions in UI benefits lead workers to be less willing to accept low quality employers independently of the match-specific quality. Thus, the average employer quality of the realized matches increases. As wages are determined as a fraction of the match surplus, higher production implies higher surplus and higher wages.

To examine the impacts of UI on wages, match and firm quality and sorting, we exploit the fact that the UI extensions introduced during the early 2000s and the Great Recession had different durations across states depending on whether each state qualified or not for the various extensions (determined by somewhat random cutoff unemployment rates) introduced by Congress at somewhat random points in time. We use the Longitudinal Employer-Household Dynamics (LEHD) database to study if offering more weeks of UI benefits increases wages of workers transiting from non-employment to employment. Our estimates suggest that a longer duration of UI benefits increases wages. An increase in UI duration of 53 weeks, from 26 weeks (the average duration of UI in most states during normal times) to 79 weeks (close to the average UI benefit duration at the end of 2009) increases earnings by 12% for workers transiting from non-employment to employment.[5] We also compare our previous results with the ones estimated using the Current Population Survey (CPS) data. The CPS has the advantage of allowing us to separate workers transitions from unemployment to employment from transitions out of the labor force. Consistent with our LEHD results, we find a positive relationship between available weeks of UI benefits and wages for workers moving from unemployment to employment implying that exits out of the labor force have minor effects on our estimates.

Importantly, the LEHD data allows us to test the importance of the theoretical mechanisms in terms of

[4] The calibration of their model to the U.S. economy shows that UI extensions increase productivity.

[5] Since the LEHD data do not report the wage rates and hours worked, we use total earnings in a quarter.

re-employment earnings through improvements in firm quality, match quality and sorting. An advantage of the LEHD data is that we can track employer-employee linkages, and we can also control for many other factors that may affect earnings. We use the estimation approach proposed by Woodcock (2015) to estimate worker and employer time-invariant productivities and the time-invariant quality of the match between any pair of employers and employees.[6] Match effects capture the concept of worker's characteristics and skills that are intrinsically good fits for a given employer's skills and needs and generate production complementarities (Gibbons and Katz, 1992). In addition, we also rank employers and employees by their percentile based on the estimated employer and employee fixed effects, and we construct a rank similarity index that captures how close the ranks of each firm and worker pair are. We interpret the similarity index as a measure of sorting between employers and employees, which need not increase productivity.

We use the residuals, similarity index, and the employer fixed effects as dependent variables to infer the effect of extension in UI benefits on match quality,[7] positive sorting, and employer quality, respectively. We find that extensions in UI benefits significantly improve the quality of employer-employee matches and the quality of employers. An increase in UI duration of 53 weeks increases the match quality for workers transitioning from non-employment to employment by 4.1%, and the average quality of employers hiring workers transitioning from non-employment to employment by 5.8%. By contrast, we find limited evidence of increased positive sorting in the labor market due to the increase of available weeks of UI benefits. In terms of magnitude, an increase of 53 weeks in the UI duration increases positive sorting in the labor market by 0.6%, but the effect is only significant during the Great Recession and not during the full period of study. We also find that contrary to the prediction of Fuller et al. (2014), the probability that unemployed workers switch industries remains unaffected by UI duration.

We document larger effects of extending UI benefits on match quality for demographic sub-groups who are more likely to be liquidity-constrained. Specifically, the effect is 0.81% larger for women than men, 1.15% larger for non-white than white workers, 1.6% larger for less-educated than more-educated workers, and 10% larger for younger than older workers. By contrast, we do not observe a clear pattern that UI extensions improve employer quality of workers who are more likely to be liquidity-constrained. We do not find any statistical difference in the effect of UI on firm quality between men and women and between more- and less-educated workers. Yet, we estimate that 53 additional weeks of UI benefits improve the employer quality for white workers by 1% more than for non-white workers, but worsen it for older relative to young workers by 1%.

We validate the previous findings by examining whether more generous UI benefit duration increases tenure on the new job as both improved match quality and employer quality could be consistent with longer tenure. Matching models predict better jobs should lead to longer tenures (Jovanovic, 1979). Similarly, a number of studies have found that employer wage premia are inversely associated to turnover (Krueger and

[6] The Woodcock's (2015) estimation approach consists in a variation of the individual fixed effects methodology pioneered by Abowd et al. (1999).

[7] Lachowska et al. (2020) also use the Woodcock's (2015) estimation strategy, and use the residuals as a proxy for the quality of any pair of employer-employee matches.

Summers, 1988). We find that extending UI benefits by 53 weeks increases the probability of remaining employed with the same employer one year after the transition from non-employment to employment by 2.7% for non-whites, 2.1% for less-educated workers, and 1.6% for young workers. The effects for white, more-educated, and older workers are around 0.5%, which corresponds to one fourth of the effect on more-liquidity-constrained workers.[8]

As argued by Gibbons and Katz (1992), match effects capture the concept of worker's skills that are an intrinsically good fit for a given employer's required skills. Thus, we also construct a measure of match quality using the Current Population Survey (CPS) data that quantifies the educational mismatch between the educational attainment of workers and the educational requirement in the new jobs after their unemployment spells. We find that increasing UI benefit duration significantly decreases the mismatch for women, non-whites, and less-educated workers. Moreover, increasing UI benefit duration from 26 to 79 weeks increases the likelihood that the educational requirements in the occupation is higher by 14.4%.

The rest of the paper is structured as follows. Next section summarizes the related literature. Section 3 describes the structure of the federal unemployment insurance programs in the U.S. and their extensions during recessions. We describe the data in Section 4, and the estimation strategy in Section 5. Section 6 presents the empirical results. Section 7 concludes.

2 Literature Review

Our study relates to four branches of the literature. First, our paper is linked to the empirical literature on the effects of extensions in UI benefits on wages.[9] This literature has used different variations of regression discountinuities designs to quantify the causal relationship between UI and wages,[10] and has found mixed results on the effect of UI on re-employment wages. Card et al. (2007), Lalive (2007), van Ours and Vodopivec (2008), and Jäger et al. (2020) find no effect of UI on re-employment wages; Schmieder et al. (2016) find a negative impact; and Nekoei and Weber (2017) is the only one of these studies that finds a positive and significant effect.[11] All these studies focus on European countries because legislative reforms in Europe have created perfect laboratories for regression discontinuity designs. We propose an identification strategy to quantify the causal effect of UI on wages in the U.S., and we enhance this literature by not only by showing that longer duration of UI benefits have significantly positive effects on re-employment wages,

[8]Our estimates also suggest women do not experience any significant increase in their probability of remaining employed by the same employer after one year, while the probability for men decreases by 0.5%.

[9]There is also a vast literature examining the impact of UI benefit compensation on unemployment duration and wages (Card et al., 2015; Hunt, 1995; Johnston and Mas, 2018; Landais, 2015). As UI benefit compensation is typically not very cyclical in the U.S. system, we focus on the extensions in the number of UI weeks available to unemployed workers.

[10]Lalive (2007) exploits the discontinuous changes in UI benefit duration at age 50 which are embedded in the Austrian UI system implemented in the regional extended benefit program. Schmieder et al. (2016) use German administrative data and compare individuals younger and older than 42 years old, the age cutoff for eligibility for 18 months of UI benefits rather than 12 months. Nekoei and Weber (2017) use Austrian data and compare individuals around 40 years old that represent the cutoff after which individuals are eligible for 39 weeks of UI benefits instead of 30 weeks. Card et al. (2007) use a tenure-based regression discontinuity design comparing Austrian workers just before and after the 36 month cutoff for UI benefit eligibility. van Ours and Vodopivec (2008) exploit the 1998 reform of the Slovenian UI system, and they compare the outcomes pre- and post-reform for workers exempted or not from the reform who were employed for at least 18 months prior to their unemployment spell.

[11]Nekoei and Weber (2017) reconcile their results with previous findings by explaining that depending on the heterogeneity of the population the "liquidity effect" or the "moral hazard effect" may dominate.

but importantly by showing the sources though which more generous UI increases wages. Specifically, we decompose the increase in wages into employer quality, match quality and sorting.

Our paper, thus, also builds on the literature that investigates the mechanisms through which UI benefits affect the functioning of the labor market. A few theoretical contributions such as Acemoglu and Shimer (1999), Marimon and Zilibotti (1999), and Fuller et al. (2014) provide explanations for the positive effects of UI on wages, but as pointed out above, there are only a handful of empirical papers examining this link and only Nekoei and Weber (2017) looks into the mechanisms through which UI may affect wages. Nekoei and Weber (2017) provide some evidence suggesting that the positive effects of UI on wages are due to unemployed workers finding new jobs in larger firms, but their estimates are imprecise. They also examine the impacts of UI on the likelihood of moving firms, industries, occupations and geographic location and find no effects, though their occupation measure is coarse. While they interpret these findings as a lack of evidence on the impact of UI on reallocation, their analysis does not capture employer-employee matches. Our paper builds on Nekoei and Weber (2017) by providing novel evidence that relies on direct measures of employer-employee match quality, firm quality, and sorting. To our knowledge, our paper is the first paper to undertake this approach.[12] This is important because it identifies the channels through which wages are affected by UI and it can help determine if UI may be welfare improving.

Third, our study provides new insights on the debate about the relative importance of the "liquidity effect" and "moral hazard effect" in determining the impact of UI benefits on labor market outcomes. On the one hand, a long-standing view is that UI benefits decrease job search efforts of workers by raising the reservation wage and generating moral hazard incentives (Moffitt, 1985; Katz and Meyer, 1990; Meyer, 1990; Card and Levine, 2000). Mulligan (2012) argues that the decline in job search effort due to the extensions of UI benefits may partially explain the slow recovery and the persistence of high unemployment rates in the aftermath of the Great Recession. However, Kroft and Notowidigdo (2016) find that the moral hazard cost of UI is pro-cyclical, while the consumption smoothing benefit is acyclical. Thus, the adverse effects of UI benefits might be quantitatively small in a deep recession like the Great Recession when labor demand was weak, and the return to job search was lower.[13] On the other hand, Card et al. (2007) and Chetty (2008) show that prolonged unemployment spells due to more generous UI are mostly due to liquidity-constrained workers who search longer for a better job. Our findings are consistent with Card et al. (2007) and Chetty (2008) by showing that workers who are more likely to be liquidity-constrained are also the ones who benefit the most from the effects of UI on match quality, suggesting that the "liquidity constraint" motive plays a central role in shaping workers' search behaviors.

Finally, the paper is related to the literature on worker reallocation. Dustmann et al. (2021) show that the introduction of a nationwide minimum wage in Germany has led to reallocation effects, with low wage

[12] Similar to Nekoei and Weber (2017), we also expect bigger effects on wages, match and firm qualities than other studies, given the small effects of UI on the likelihood of exiting into employment found by Farber et al. (2015).

[13] Other studies have examined the impact of UI benefit generosity on labor demand and have found mixed results. Hagedorn et al. (2015) exploit the discontinuity of UI benefits at state borders and find that higher reservation wages due to extended benefits reduce firm vacancy creation rates. These findings are, however, in sharp contrast with Marinescu's (2017) work using online job postings, which finds no effects of UI extensions on labor demand.

workers moving from small and low paying firms to larger and higher paying firms. Lachowska et al. (2020) show that in the U.S. the loss of specific worker-employer matches explain more than half of the wage losses after worker displacements highlighting the importance of workers allocation to firms in determining wages. Raposo et al. (2021) also find that firm and match effects contribute 27% of the hourly wage losses of displaced workers. Similar to these two studies, we show that employer and match quality effects account for a substantial fraction of workers' earnings gains.

3 Institutional Background

In the U.S., the UI system is a joint federal-state program. The Federal government sets minimum benefits, and standards, but each state is free to go beyond these minima. Workers qualify for UI benefits if they have paid into the UI system through their employer, usually for the last four quarters before the start of the claim. They must also have received a minimum level of earnings in the base period, which varies across states.[14] In addition, workers are required to be totally or partially unemployed and must have lost their jobs other than for cause. Finally, the unemployed must be looking for work, and, in many states, they must show they have applied for jobs during the week for which they are claiming UI benefits.

In the vast majority of states, unemployed workers can claim UI benefits for up to 26 weeks. Eleven states offer 26 weeks of benefits uniformly to all workers. In another 31 states, the duration of UI benefits is capped at 26 weeks, but unemployed workers can only claim UI benefits for fewer weeks, depending on their contributions. Only 2 states provide more than 26 weeks of benefits: Montana pays for up to 28 weeks, and Massachusetts pays for up to 30 weeks, but only during periods of high unemployment. There are also 10 states which provide benefit payments for less than 26 weeks: Florida and North Carolina up to 12 weeks; Alabama and Georgia up to 14 weeks; Arkansas and Kansas up to 16 weeks; and Michigan, Missouri, and South Carolina up to 20 weeks.[15]

Additional weeks of benefits are granted during recessions through Federal programs to ensure that workers who lose their jobs do not suffer severe drops in their income and consumption. Two major Federal programs extend the duration of UI benefits during recessions. The first is the Extended Benefits (EB) program, which is a permanently authorized program established by Congress in 1970. The Federal-State Extended Unemployment Compensation Act establishes the provision of financial support to extend benefits for individuals who have exhausted their state UI benefits when unemployment rates are high. The EB Program grants an extension of UI benefits by 13 and 20 additional weeks if the 3-month state average unemployment rate exceeds 6.5% and 8.5%, respectively. The additional weeks of UI benefits granted through the EB Program can be claimed once the regular UI benefits and the extended weeks granted through temporary programs are exhausted.

The second type of programs are federally-funded temporary UI benefit extension programs, which have

[14] A detailed description of the state minimum contributions for UI eligibility can be found in the following report: https://oui.doleta.gov/unemploy/comparison/2010-2019/comparison2019.asp.

[15] While all states are included in the CPS analysis, only Arkansas and Kansas are included in the LEHD analysis.

been introduced during each recession since the late 1950s. Although these programs have changed over time in name, duration, and conditions to qualify, they all provide additional weeks for unemployment insurance benefits on top of the ordinary state UI benefits. Temporary Unemployment Compensation (TUC) programs were introduced between June of 1958 and June of 1959, between April 1961 and June of 1962, and between January of 1972 and March of 1973 for an additional period of 13 weeks. Federal Supplemental Benefits or Compensation (FSB or FSC) programs were introduced between January 1975 and January of 1978, and between September of 1982 and June of 1985 for various lengths of time. Between March of 2002 and March of 2004, the TUC program was re-introduced, granting up to 13 or 26 additional weeks of benefits. Finally, the temporary benefit programs were renamed Emergency Unemployment Compensation (EUC) Programs between November 1991 and April 1994, and during and in the aftermath of the Great Recession, between July of 2008 and January of 2014. The exact timing of when these extensions are introduced and when they expire is somewhat idiosyncratic as they may depend on when Congress is in session or what other legislation is being approved through Congress.

The TUC 2002 Program consisted of two separate levels or tiers. Individuals who exhausted their regular state UI benefits were automatically eligible for 13 weeks of benefits. The second tier, instead, contained a trigger mechanism and established a threshold requirement such that individuals in states with a 3-month average unemployment rate above 6.5% were eligible for an additional 13 weeks of UI benefits. The TUC 2002 Program jointly with the EB Program guaranteed up to a maximum of 72 weeks of UI benefits.

The EUC Program introduced in the Great Recession was extended several times. It became increasingly more complicated by adding more tiers over time. To simplify the exposition, Figure 1 provides a schematic summary of the available benefits during the Great Recession. Initially, EUC was launched from July 2008 to March 2009 and allowed the unemployed to claim an additional 13 weeks of benefits. In November 2008, the program expanded to allow 20 instead of 13 weeks of benefits. Between March and December 2009, the program changed from a one-tier to a two-tier program, with those in the first tier continuing to receive 20 weeks and those in the second tier receiving an additional 13 weeks of benefits. Between December of 2009 and February 2010, the program consisted of four tiers. The first two tiers were activated unconditionally for all states and offered 20 and 14 additional weeks, respectively. The last two tiers were conditioned on state-specific unemployment rates. The third tier offered 13 extra weeks in states with a 3-month average unemployment rate above 6%, while the last tier offered 6 additional weeks of unemployment benefits to individuals in states with a 3-month average unemployment rate above 8.5%. This same four-tier program was then extended six more times through new legislation until May 2012. From May to September 2012, the program went back to a four-tier system with benefits of up to 20, 14, 13, and 6 weeks in Tiers 1 through 4, respectively. Starting in June 2012, the second tier required a 3-month state unemployment rate above 6% to qualify, and the third and fourth-tiers now required unemployment rates above 7% and 9%, respectively. Finally, between September 2012 and January 2014, the four Tiers maximum benefit weeks changed to 14, 14, 9, and 10, respectively.

Since states could trigger "on" and "off" from tiers 3 and 4 due to changes in their unemployment rates, UI benefit durations varied in a given year within each state and also across states. Since June 2012, states could also trigger "on" or "off" from Tier 2. Together with ordinary UI benefits and the additional weeks granted under the EB program, the EUC allowed individuals to use up to 99 weeks of UI benefits. The exact number of weeks for which unemployment benefits were extended in each tier and the unemployment rates that triggered states to qualify for an additional tier are somewhat idiosyncratic and often choose to round to a whole number or a half decimal but has no particular justification.

Figure 2 shows heat maps constructed by using the maximum amount of statutory UI benefit weeks in three periods.[16] The first row shows the ordinary UI benefits between January 2000 and December 2001, and between January 2005 and July 2008. The middle row refers to the early 2000s recession extension between January 2002 and December 2004. The bottom row reports the Great Recession extension between August 2008 and December 2013. The maps on the left side include all U.S. states, while the maps on the right focus on the 20 states we use in the LEHD analysis.[17] Figure 2 shows that the duration of UI benefits is always extended during downturns. In our analysis, we not only take advantage of variations in UI durations between recessions and expansions, but we also exploit monthly or quarterly variations during recessions. The maps also highlight a significant increase in the dispersion of UI benefits durations across states during the early 2000s recession and the Great Recession.

Figures 3 and 4 examine each source of variation —across states and over time— further. Figure 3 reports the variation in weeks of UI benefits across states by quarter for the full sample of states (Panel A) and the 20 states in the LEHD data (Panel B). There are two main takeaways from this figure. First, there is little variability in UI durations during expansionary periods, but the variation grows widely during quarters in recessionary periods when the UI extensions were introduced. Second, there is little variation in UI durations during the early 2000s, while the interquartile range in the duration of UI benefits was much wider during the Great Recession. While the maximum weeks of UI benefits individuals could claim were 79 weeks by the second half of 2009, the maximum duration of UI benefits increased to 99 weeks for most states during the aftermath of the Great Recession. Yet, there are a few states with significantly lower benefit durations. The minimum amount of UI benefit weeks during the Great Recession is about 15 weeks below the average number of weeks at the 25% percentile.

Figure 4 shows the variation in the duration of UI benefits over the period 2000-2013 for each individual state (red states are in both the LEHD and CPS data, while blue states are only included in the CPS data). We observe large variations in UI benefits within states. The range between the minimum and the maximum amount of UI weeks is between 26 and 99 weeks for 65% of the states –33 out of the 50 states and the District of Columbia. Figure 4 also highlights that the availability of large extensions of UI benefits is a rare event. Importantly, this figure shows that there is substantial variation across states.

[16]The UI benefit weeks are grouped in Figure 2 based on the availability of weeks from the EB Program and the Four-Tiered EUC Program between December 2009 – May 2012 as showed in Figure 1.

[17]For graphical purposes, we omit Alaska and Hawaii from the map.

4 Data

4.1 Longitudinal Employer-Household Dynamics (LEHD) Data

Our primary source of data is the Longitudinal Employer-Household Dynamics (LEHD). The LEHD infrastructure consists of restricted microdata coming from information on the quarterly earnings disbursements paid by employers to their employees as part of unemployment insurance.[18] The records cover nearly all private sector and state and local government employment, excluding only self-employment or workers hired by the federal government.[19]

The records in the LEHD infrastructure correspond to any individual's UI-covered earnings paid by an employing tax-payer entity identified by a State Employer Identification Number (SEIN).[20] An individual is included in a quarter if at least one employer reports earnings of at least one dollar for that individual during that quarter.[21] We denote individuals to be employed in a quarter if they receive non-zero earnings in that quarter. Similarly, we denote individuals to be non-employed in a quarter if they do not receive any earnings in that quarter.[22]

Each record is completed with information on employers and individuals. Additional data on employers come from each state's Department of Employment Security administrative files collected as part of the Covered Employment and Wages (CEW) program, jointly administered between the BLS and the Employment Security Agencies. Individuals' demographic characteristics come from two administrative data sources, the Person Characteristics File (PCF) and the Composite Person Record (CPR), both maintained by the Census Bureau. Individuals are uniquely identified by a Protected Identification Key (PIK) that tracks them across states and time.

The Census Bureau has granted us access to data from 20 U.S. states, which account for about 50% of the U.S. labor market. The states included in our analysis are Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, Pennsylvania, Tennessee, Texas, Washington, and Wisconsin. These 20 states have joined the LEHD microdata at different times.[23]

We consider only the main job, which we define as the job with the employer at which that worker earns most of her earnings in a quarter. We restrict the sample only to employees who have had at least two

[18]Unemployment spells during the recessions we study are typically well beyond a quarter. For instance, the median duration of unemployment spell in June 2010 was about 6 months, reducing concerns our quarterly data will not be able to capture these transitions.

[19]The Census Bureau collaborates with its state partners through the Local Employment Dynamics (LED) cooperative federal-state program to compile the LEHD infrastructure. Each LED partner state collects earnings data from Unemployment Insurance (UI) administrative files, and extracts information from the Bureau of Labor Statistic's Quarterly Census of Employment and Wages (QCEW) administrative files.

[20]The correspondence between SEINs and firms is not one-to-one because a few large firms have multiple SEINs.

[21]Current earnings are deflated by the national CPI Index adjusted for the Regional Price Parities (RPP) Index that accounts for differences in purchasing power between states. The BEA reports the RPP starting from 2008. As the RPP Index varies little over time, we use the average over the available years to adjust the years before 2008.

[22]Since we only have access to 20 U.S. states, we cannot distinguish between non-employed individuals and individuals who have moved to one of the states which we do not have access to in our data.

[23]Apart from DC which entered the LEHD in 2002Q2 and Arkansas in 2002Q3, all other states were already part of the LEHD program prior to 2000Q1.

different employers over the period 2000-2013. In addition, we restrict our sample to employees of working-age between 20 and 65 years old to study the effect of UI on labor market outcomes of employees who transit from non-employment to employment.[24] We assign to each individual the maximum number of statutory UI weeks available based on the state of their last employer before the beginning of the non-employment spell.[25]

Our empirical analysis spans from the first quarter of 2000 to the fourth quarter of 2013, and includes two recessions, the early 2000s recession and the Great Recession. We also present the results for the sub-period 2008-2013, which includes the Great Recession and its aftermath. This is the period with the largest extension of UI benefits and the greatest variation across states. The sample of workers who transit from non-employment to employment consists of approximately 120 million individuals for the period 2000-2013, and approximately 92 million individuals for the period 2008-2013.[26]

Table 1 reports the summary statistics of the U.S. working population for the full period from 2000-2013 (Columns (1)-(3)) as well as for our main period of analysis —the period 2008-2013 (Columns (4)-(6)). Column (1) reports the summary statistics for the 20 U.S. states we have access to from 2000-2013, while Columns (2) and (3) provide summary statistics for the states with UI duration above and below the average UI duration across all 20 states for the full period.[27] The shares of male and female workers are about the same (52% vs. 48%). The working population in our sample is predominantly composed of white workers (80%). The share of workers with some college or more is higher than the percentage of workers with high school or less (59% vs. 41%).[28] There are more educated workers in states with UI benefit duration above the average compared to those in states offering shorter UI benefit periods. Otherwise, workers in more and less generous UI states are similar in terms of gender, race, and age. The average quarterly earnings are around $5,200 in the full sample as well as in the sub-samples of workers living in states that offer UI benefits for longer and shorter periods.

The population characteristics are very similar for the full period (2000-2013) and the sub-period 2008-2013. Columns (4)-(6) shows summary statistics for the period 2008-2013 for all workers and workers in states with UI benefits duration above and below the average. The share of men (51.5% vs. 51.1%), whites

[24]Focusing on individuals who move into employment could potentially introduce a bias in our estimates. However, the bias is likely to be negative as those who are more motivated are more likely to become employed, but they are also less likely to live in states with more generous benefits.

[25]As the LEHD records are quarterly, we take the average of the weeks of UI benefits by quarter.

[26]Due to Census disclosure rules, we cannot report the exact number of observations used in our analysis.

[27]The average number of weeks of UI is computed over the period 2000-2013 among the 20 states available to us through the LEHD program. The average UI is equal to 46 weeks. For comparability, we classify the states in both periods, 2000-2013 and 2008-2013, using the same cutoff. Although the average UI for the sub-period 2008-2013 is higher (around 63 weeks), the classification between states above and below the UI average is substantially unchanged by using the average over the period 2000-2013 or the average over the period 2008-2013. We classify states as above the mean as the states whose average over the 2000-2013 is above the mean across all states. States below the mean are classified following a similar procedure. The states with UI benefits above the average are: Arizona, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Nevada, Pennsylvania, Tennessee, and Washington. The states with UI benefits below the average are: Arkansas, Iowa, Kansas, Maine, Maryland, Oklahoma, Texas, and Wisconsin.

[28]Our estimates are similar to estimates from the Current Population Survey (CPS), indicating that the characteristics in our LEHD sample are not very different from those reported for the full population using more standard sources. The main difference is in the level of educational attainment. Since the education variable is imputed in the LEHD and based on the 2000 education distribution, it is thus not surprising that the share of more educated workers is lower in the LEHD than recent estimates from the CPS.

(81.1% vs. 80.4%), and those with college (58.9% vs. 58.2%) are somewhat lower during the 2008-13 subperiod compared to the full period, but the average age is slightly higher (40.6 vs. 41.1 years of age) though none of them are significantly different from each other. As for the full time period, the states with more and less generous benefits look very similar in terms of worker characteristics, except that workers in the more generous states are more educated. Quarterly earnings are around $5,200, and similar for workers living in more than less generous states.

4.2 CPS Data

We also use the Monthly Public Data files from the Current Population Survey to examine the impact of the UI benefits extensions on wages and the job skill requirements for workers after unemployment spells. Households in the CPS are interviewed four months, then let go for eight months, then interviewed again for another four months. Every month one-eighth of the households enter the sample, and a similar number leaves the sample. Households are asked questions about wages and hours worked only in their fourth and eighth interviews. The monthly CPS collects extensive demographic characteristics and labor market information, including information on current and past occupations, education, age, gender, race, and marital status.

Our analysis using CPS data complements the one based on LEHD data, and addresses some of the well-known weaknesses of the LEHD data. First, while the LEHD data report the non-employment status, in the CPS we can distinguish between unemployment and non-participation in the labor force This feature enables us to address the concern that the LEHD results are driven by individuals who exit the labor force. Second, as in the CPS data we observe the duration of the unemployment spell, we can construct the available UI benefits at the individual level rather than the statutory benefits at the state level. The available UI benefits are calculated as the difference between the statutory UI benefits weeks in a state at a point in time minus the duration of unemployment for an unemployed individual in the sample (Rothstein, 2011; Farber and Valletta, 2015; Farber et al., 2015). Third, as the CPS data are monthly, we can measure short-term unemployment spells, which we cannot identify in the LEHD data since they are quarterly.[29] By comparing the estimates between the CPS and LEHD data, we can assess the importance of short-term unemployment spells.[30] Finally, differently from the LEHD data, the CPS survey collects information on occupation, and enables us to study a different measure of match quality based on skill mismatch.

Due to the fact that the BLS increased the CPS sample size in the early 2000s, we restrict the analysis to the Great Recession and its aftermath to avoid problems with the estimation of the coefficients. We link individuals from one month to the next using household and individual identifiers following Shimer (2012), and we rule out spurious matches by checking the consistency of sex and age of individuals from one month

[29] The wage information is only available at month 4 and 8 of the CPS rotation structure. The monthly data can only be used for our other metrics based on occupation codes.

[30] As the CPS data are not employer-employee data, we cannot assess the importance of short-unemployment spells on match quality. Fujita and Moscarini (2017) show that a large share of short unemployment spells end up with recalls. For our analysis, this finding implies that we would not observe any change in these dimensions in cases of recalls. Thus, we believe missing short non-employment spells should not significantly affect our findings.

to the next. To study how UI benefit extensions impact the job skill requirements, we restrict the sample to workers who are present in the CPS in months 3, 4, 7 and 8 so that we made a transition from unemployment to employment either from month 3 to 4 or from month 7 to 8, or both.

We construct two measures to capture the skill requirements. The first measure is the difference in the educational requirements of the occupations in the current and previous jobs. To construct the educational requirements, we use the U.S. Labor Department's O*NET database that gathers data on requirements for entry-level jobs by surveying each occupation's working population. We quantify the educational requirements of a job based on the responses, recorded as a categorical variable,[31] from the current employees.[32] To obtain a numerical proxy for the educational requirements for each occupation, we convert the categorical responses into years of education by multiplying the shares of responses for each category by the number of years required to attain the corresponding education level.[33] We measure the educational mismatch as the difference between the workers' educational attainments and the educational requirements in their current jobs.[34] Thus, the educational mismatch is measured in terms of the difference in years of education, and it can take either positive or negative values reflecting whether a person is over-qualified or under-qualified for the job. The second measure is an indicator that takes the value of one if the number of years of education required by the new occupation is greater than the number of years of education required by the previous occupation and zero otherwise.

We construct a second sample to measure the medium-term effect of UI benefits on hourly wages of workers who exit the labor market at a particular point in time.[35] We create longitudinal histories of workers several months apart by using the personal identifiers present in the CPS monthly data files from IPUMS. As we are interested in medium-term effects, we focus on workers who have participated in all the 4-months in the CPS before leaving the sample,[36] and we restrict our sample to workers who transitioned from unemployment to employment any time during months 1 through 3 and analyze their wages in month 4. As individuals may transit from unemployment to employment in one of the months from 1 through 3, we assign the available UI benefits for the month in which the individual made the transition.

Table 2 reports the descriptive statistics for the CPS. Column (1) shows the descriptive statistics for the entire sample, while columns (2) and (3) show the descriptive statistics for those in states with UI benefits duration above and below the average benefit weeks, respectively.[37] The first row of Table 2 shows that

[31]The categorical variable can take the following options: less than high school, high school, some college, associate' some college, associate, and graduate degree.

[32]Employees answer the following survey question: "If someone was being hired to perform this job, indicate the level of education that would be required." Responses refer to the level of education of a new hire.

[33]For example, if out of the respondents 80% say that a Ph.D. degree is required to perform the job of an Economist, and 20% a Masters' degree, the requirement for an Economist job is 17.6 (0.8 x 18 + 0.2 x 16) years of education.

[34]We have also checked the robustness of our results to alternative definitions of the educational mismatch such as the mode of the responses rather than the mean. The results remain quantitatively similar

[35]The nominal wages reported in the CPS are converted to real terms by deflating with the 2010 national CPI.

[36]In the analysis, we use individual weights from the 8th month.

[37]The states above the average of available UI in the CPS are Alabama, Alaska, Arizona, California, Colorado, Connecticut, Delaware, District of Columbia, Georgia, Idaho, Illinois, Indiana, Kentucky, Maine, Massachusetts, Michigan, Nevada, New Jersey, New York, North Carolina, Ohio, Oregon, Pennsylvania, Rhode Island, Tennessee, Texas, Washington, West Virginia, and Wisconsin. The states below the average are Arkansas, Florida, Hawaii, Iowa, Kansas, Louisiana, Maryland, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Mexico, North Dakota, Oklahoma, South Carolina, South

81.5% of people exiting unemployment move to occupations with higher education requirements than their previous job. The share of those moving to jobs with higher education requirements after being unemployed is the same for those in states with UI benefit weeks above the average and those in states with shorter UI benefits durations. The third row shows that, on average, workers have 0.13 more years of education than it is actually required on the job, and this mismatch is greater in more generous states where UI benefits duration is longer (0.19 vs. 0.11). The average age for those in the sample is 37 years old. Also, 81% of those in the sample are white, 58% are male, and 47% have some college or more as the highest educational attainment. Workers living in states with more generous UI benefits are less educated, younger, and less likely to be white and male. It is worth highlighting that these summary statistics are not representative of the entire U.S. population. The differences are explained by the fact that our sample is restricted only to individuals who moved from unemployment to employment. For example, as a lower share of more educated workers is unemployed, it is reasonable to obtain a lower share of more educated individuals in the sample than in the national statistics. The last row of Table 2 reports the average hourly wages in month 4 after exiting from unemployment. The average hourly wage for the entire sample of states is about $12.50. The average wage for workers in states with UI benefit weeks above the average is slightly lower than for those in states with shorter UI durations ($12.17 vs. $12.74), though they are not significantly different from each other.

4.3 State Controls

The extension of the UI benefits is concurrent with weak economic conditions, and some of the "tiers" were activated at the realization of extreme adverse economic events. Those weak economic conditions may also affect the quality of the matches between employers and employees. To address this potential issue in the identification of the effect of UI on labor market outcomes, we include state-specific macroeconomic variables to capture the deterioration of the economic environment when UI extensions are activated. At the same time, the exact timing of the extensions and at what duration the tiers occurred is somewhat random.

We collect unemployment rates for every state over time from the Local Area Unemployment Statistics (LAUS) at the Bureau of Labor Statistics (BLS). The BLS reports local unemployment rates at a monthly frequency. Since the LEHD microdata has quarterly periodicity, we take the average of the 3-month unemployment rates within each quarter to obtain a quarterly rate. In the case of the monthly CPS data, instead, we use the monthly unemployment rates. The unemployment rate reached its peak during our period of study in the aftermath of the Great Recession with a spike of nearly 10%, driven mostly by large drops in demand and increases in layoffs.

We also include a second macroeconomic control —the gross state product (GSP)—from the Bureau of Economic Analysis (BEA). The BEA computes quarterly GSP data starting from 2005. For years before 2005, GSP series are calculated only annually. We assume that for the years before 2005, all quarters have

Dakota, Utah, Vermont, Virginia, and Wyoming.

the same value, and this value is equal to the annual GSP.[38]

The last two rows of Table 1 report the summary statistics for the macroeconomic controls at the state-level.[39] States with UI benefit durations above the sample mean have higher gross states product (contrary to what one may have expected), but also higher unemployment rates than states with UI benefits durations below the average. As expected, due to the slow recovery after the Great Recession, unemployment rates for the sub-period 2008-2013 are significantly higher than for the period 2000-2013.

By controlling for the unemployment rate and GSP, we can capture the impact of unemployment benefits on match and employer quality while holding the state's economic conditions constant. Thus, any additional variation in UI would probably come from the somewhat random timing of when Congress extended the benefits and the somewhat random length of each tier and whether the unemployment rate of a state falls above or below the unemployment rate that triggers qualification for the tier.

5 Empirical Methodology

First, we describe the estimation of the quality of employers and employees. Then, we present alternative specification to identify the causal effect of extensions in UI benefits duration on a set of outcome variables. Our investigation will shed light on which mechanism—match quality, sorting, and employer quality—plays a significant role in determining re-employment wage changes.

5.1 Estimation of Employer and Employee Effects

We derive the employer and employee quality by implementing the fixed effects estimator proposed by Woodcock (2015). This estimation approach is a variation of the individual fixed effects methodology pioneered by Abowd et al. (1999). The Woodcock's (2015) estimator enables us to estimate time-invariant worker-employer match effects.

Similar to Lachowska et al. (2020), we first absorb from the log wages of worker i hired by employer j in state s at time t the contribution of age, years of working experience, tenure, and state, time, and industry fixed effects.[40] Then, we calculate the average residual from the first stage for each pair of employer and worker, and we denote it as $\overline{\log{(\eta^w_{ij})}}$ where η are the residuals from the first stage. In the second stage, we use the average residuals for employer-worker pairs, and we implement an AKM estimation such as:

$$\overline{\log{(\eta^w_{ij})}} = \alpha_i + \theta_j + \varepsilon_{ij} \tag{1}$$

where α_i are the employee fixed effects that capture the time-invariant worker characteristics that affect earnings, θ_j are the employer fixed effects which capture firm-specific pay premium, and ε_{ij} is the error term. The residuals $\hat{\varepsilon}_{ij}$ represent the variation in the average residualized earnings after accounting for worker and employer fixed effects. We interpret the residual $\hat{\varepsilon}_{ij}$ as a measure of the quality of the match

[38] While the GSP is at quarterly frequency, the CPS data are monthly. We assume that all months in a quarter have the same value and that this value is equal to the quarterly statistic.

[39] The statistics are computed without weighting the state figures by the state populations.

[40] Industries are defined at the 3-digits NAICS.

between employer j and worker i.

Individual fixed effects are computed by implementing a variation of the computational algorithms used in Guimarães and Portugal (2010) and Crane et al. (2018).[41] We estimate the individual fixed effects via an iterative procedure. We use the average of the residuals from the first stage as initial guess. Then, we compute the employers' fixed effects by removing the guess of the individual fixed effects from the average residualized earnings, and we recalculate the workers' fixed effects in the same manner. We repeat these steps until the convergence in the goodness-of-fit criterion is achieved.[42] The estimation of the employee and employer fixed effects is based on the longest time series available. Using the longest time series possible helps to attenuate the "limited mobility" bias,[43] and returns more precise estimates of the fixed effects.

5.2 Estimating Impacts on Match Quality, Sorting, and Employer Quality

In line with previous studies, we start our empirical analysis by examining the effect of extensions in UI benefits on re-employment wages. We test the following specification:

$$\log\left(w_{ijst}\right) = \delta_1 UR_{st-1} + \delta_2 \log(GSP_{st-1}) + \beta UI_{st-1} + \rho X_{ijst} + \mu_s + \tau_t + u_{ijst} \tag{2}$$

where $\log\left(w_{ijst}\right)$ are the log wages, UR_{st-1} is the state unemployment rate, $\log(GSP_{st-1})$ is the log of the gross state product, X_{ijst} are the same individual characteristics mentioned above and used to estimate the fixed effects and match quality, time and state fixed-effects, and u_{ijst} is the error term. In all specifications, we cluster the standard errors at the state level to capture the correlation in errors within states.

Extensions in UI benefits in the U.S. occur when economic conditions are weak. Weak economic conditions themselves may affect wages. In all empirical specifications, we control for observable state's aggregate macroeconomic variables, including the unemployment rate and the gross state product, to capture the variation in the match quality linked to weak economic conditions. By controlling for aggregate macroeconomic variables, we isolate the effect of the duration of UI benefits from the effect of the cycle. This is important because UI benefits were extended nationwide when the state of the economy worsened. Moreover, additional tiers of UI benefits sometimes triggered in response to higher unemployment rates in a state. Since worse labor market conditions may be related to hiring into worse jobs but also coincides with extended benefits, this may bias the effects of UI generosity on wages downwards. To address this concern, we use lagged macroeconomic by a quarter.[44]

As already mentioned, legislative changes in UI benefits voted by Congress occurred due to weak economic conditions and aimed to ensure that unemployed workers during recessions do not suffer significant drops in incomes and consumption. As we control for unemployment rates and GSP, it is reasonable to

[41] We are grateful to Henry Hyatt for having shared with us their computational algorithms.

[42] We set as stopping criterion the difference between the R^2 in two consecutive iterations to be less than 0.001.

[43] See Abowd and Kramarz (2004) and Andrews et al. (2008) for a discussion of the "limited mobility" bias and its effect on the individual fixed effects estimates. The "limited mobility" bias in the estimation of the worker fixed effects occurs because workers' moves across firms that help identify these fixed effects are not frequent enough. Thus, the bias is bigger when there are fewer movers.

[44] Implicitly, we assume that previous state economic conditions affect wages, but not the opposite.

think the timing and the incidence of legislative changes on individual states are exogenous to individual matches. Therefore, we believe, in our context, these legislative changes are a suitable natural experiment to investigate causal effects. While discontinuity or kinked designs provide a clean method to identify UI impacts, an advantage of our identification design over RD designs is that instead of capturing local average treatment effects around a discontinuity cutoff, we capture the general equilibrium effect of the policy. This is particularly important given that we want to capture effects on sorting and matching which requires both sides of the labor market responding to the policy and would only be captured by an identification strategy that allows to capture general equilibrium effects.

UI benefits may affect wages through three channels: changes in match quality, changes in sorting of employers and employees, and changes in the employers' quality. In the remaining of this sub-section, we will discuss the empirical approach to test the relevance of these three mechanisms.

Our benchmark measure of match quality between employers and employees is the residual from equation (1). The residual term captures unobserved variables and, among other things, the quality of the match between worker i and employer j. This measure should capture production complementarities between employers and employees which increase productivity and wages. We estimate the following model to identify the causal effect of UI durations on the quality of employer-employee matches:

$$\hat{\varepsilon}_{ijst} = \delta_1 UR_{st-1} + \delta_2 \log(GSP_{st-1}) + \beta UI_{st-1} + u_{ijst} \tag{3}$$

where $\hat{\varepsilon}_{ijst}$ are the residuals.[45] β is the main coefficient of interest, and it quantifies the causal effect of extenstions in UI benefit durations on the quality of the match between employers and employees. A positive and significant estimate of the coefficient β would mean that longer durations of UI benefits improve employer-employee matches, providing support for the view that with more generous UI benefits workers will search longer for better matches.[46]

We also investigate the heterogeneous effects of UI benefits on match quality for different groups of workers. Specifically, we augment the previous model with interactions of UI benefits durations and indicators for demographic sub-groups:

$$\hat{\varepsilon}_{ijst} = \delta_1 UR_{st-1} + \delta_2 \log(GSP_{st-1}) + \beta UI_{st-1} + \phi D_i + \gamma D_i \times UI_{st-1} + u_{ijst} \tag{4}$$

where D_i takes the value of one if a worker i is male (zero if female); or white (zero if non-white); or more-educated (zero if less-educated); or older (zero if younger).[47] Investigating differences across these

[45]The specification in equation (3) does not include state and time fixed effects because the outcome variable has been constructed after absorbing state and time fixed effects. We ran as a robustness check the same specification including state and time fixed effects. The results remain consistent with the ones presented in the paper.

[46]Note that to estimate firm and employer fixed effects and the residual, we consider a sample of workers in the labor force with non-negative wages. While this may generate a selection bias, this bias is likely to be negative since those in the labor force are not only more motivated but they are less likely to go live in states with less generous UI. This means that our estimates would likely be downwardly biased.

[47]We define a young worker as a worker younger than 40 years old. The choice of the threshold is motivated by the average age of the population in our sample reported in Table 1.

groups can help disentangle "moral hazard" and "liquidity constraint" channels. If the estimated effects are greater for more credit-constrained demographic sub-groups such as women, less-educated, non-white, and younger workers, that would provide support for the importance of liquidity constraints in individuals' search behaviors during periods of UI extensions. By contrast, "moral hazard" effects should not differ much by group.

Next, we turn to exploring the impact of UI benefit extensions on labor market sorting. Match-specific quality and sorting capture two different concepts, and have different economic implications. In one case, extensions in UI benefits help workers find an employer with whom they have higher match-specific quality. In this case, extensions improve the wage of at least one worker without hurting any other workers. In the case of positive sorting, more generous UI can lead to more sorting such that higher-quality workers match with more productive employers and potentially have higher wages, the opposite is true for lower-quality workers, implying that more sorting benefits some workers while hurting others. If the effects for higher- and lower-quality workers cancel out, then more sorting could potentially explain the non-positive effect of UI on the average wage documented in previous studies.

We proxy positive sorting by constructing a similarity index between worker and employer rankings. Contrary to the matching measure above, positive sorting in terms of observable and unobservable characteristics (including amenities) need not generate production complementarities and may not increase productivity and wages. We first compute the percentile of each employer and employee fixed effects estimated from equation (1). For any employer-employee pair, we then calculate the absolute value of the difference between the percentile of an employee and the percentile of an employer. This measure captures the distance in the position of an employer and an employee relative to their peers. The distance between an employer and an employee is bounded between zero and one. Zero distance means that an employer and an employee are in the same percentile of their distribution. A distance of one implies that either an employer with the highest quality matches to an employee with the lowest quality or the other way around. The larger is the gap between an employer and employee, the more dissimilar they are. To interpret our estimates as positive assortative matching, we write the dissimilarity index as one minus the previously defined distance:

$$d_{ijst} = 1 - |p_i - p_j| \tag{5}$$

with p_i and p_j being the percentiles for employee i and employer j, respectively. To investigate the effect of UI benefits on labor market sorting, we replace the dependent variable in equation (3) with the dissimilarity index defined above.

The last channel we examine is the change in employer quality due to extensions in UI benefits. A shift to the right of the distribution of employer qualities in the realized matches such that the new distribution first-order stochastically dominated the previous distribution would imply the average quality of employers increases. In an environment with complementarities in production between employers and employees and in which wages are determined by the surplus sharing rule, increases in employers' productivities would also imply positive wage changes for workers.

We use the estimated employer fixed effects from equation (1) to test this hypothesis:

$$\theta_{j(i)st} = \delta_1 UR_{st-1} + \delta_2 \log(GSP_{st-1}) + \beta UI_{st-1} + u_{ijst} \tag{6}$$

in which $\theta_{j(i)st}$ are the estimated employer fixed effects associated with worker i. Similarly to the previous specification, we also test the heterogeneous effects for relevant demographic groups.

5.3 Alternative Measures of Job Stability and Skill Match Quality

Both improved matching and better jobs predict that workers will stay longer with their employers, so we examine if more generous UI leads to greater job stability. Matching models predict better jobs should lead to longer tenures (Jovanovic, 1979). Similarly, higher employer quality is predicted to reduce turnover (Krueger and Summers, 1988). Since both higher match and employer quality would be consistent with longer tenures, we also examine the impact of UI on the likelihood of remaining employed. The LEHD data are available only until 2015Q2. Therefore, the tenure for matches formed towards the end of the period is mechanically bounded by the availability of data. As most of the variation in UI benefits occurs in the last few years of the sample period, the mechanical truncation in job tenure could significantly affect our estimates. For this reason, we follow a different approach and investigate if extensions in UI benefits affect the probability of remaining hired with the same employer one year after the match is formed. We estimate the following specification:

$$Prob(e_{ijst+4} = 1 | e_{ijst} = 1) = \delta_1 UR_{st-1} + \delta_2 \log(GSP_{st-1}) + \beta UI_{st-1} + \rho X_{ijst} + \mu_s + \tau_t + u_{ijst} \tag{7}$$

where $Prob(e_{ijst+4} = 1 | e_{ijst} = 1)$ is the probability that a worker i employed by j remains employed at the same employer four quarters ahead. We also explore the same specification for demographic sub-groups.

Finally, as a robustness check, we compare the results for the match and employer quality with estimates using two alternative measures: skill requirements on the job and educational mismatch. We examine a different aspect of job quality and mismatch by focusing on the educational requirements and a measure of educational mismatch. Our previous analysis sheds light on the effect of extensions in UI benefits on the quality of employer-employee matches. LEHD data do not provide information about the specific tasks performed by workers. One additional way of thinking about improvements in job quality is to focus on the tasks required to perform a new job. In addition, Gibbons and Katz (1992) highlight that match quality captures the intrinsic good fit between a worker's skills and an employer's skills.

We proxy job quality with a measure of the educational requirements of the new job from the O*NET data. We also use as an alternative measure of mismatch the difference between a worker's educational attainment and the educational requirement of the new job. As the LEHD data do not contain information on workers' occupations, we use CPS data to construct these alternative measures. Since the source of data is different, our estimation strategy is also slightly different and follows the works of Rothstein (2011), Farber and Valletta (2015), and Farber et al. (2015). We estimate the following model:

$$Y_{ist} = \delta_1 UR_{st-1} + \delta_2 \log(GSP_{st-1}) + \beta AvailableUI_{st-1} + \rho X_{it} + \mu_s + \tau_t + u_{ist} \tag{8}$$

where Y_{ist} are the re-employment job quality or the education mismatch measures. $AvailableUI_{st-1}$ measures the available duration of benefits in each state during each month for a given individual i calculated as the total UI benefits weeks in the state s at a point in time minus the duration of unemployment for an unemployed individual in the sample. Given the different data source, the set of available controls is also slightly different. X_{ist} includes age, squared age, years of education, a set of dummies for race, gender, and marital status.

6 Impacts of UI on Wages, Match and Job Quality, and Sorting

6.1 Wages

Wages capture many different effects such as the quality of the match, worker ability, employer productivity, bargaining power, or other observable and unobservable factors. We first examine the impact of extensions in UI benefits on wages. The estimates are reported in Table 3. The first two columns show the impact of unemployment insurance duration on the logarithm of earnings for workers who change their employment status from non-employment to employment. We find that UI extensions increase earnings. An increase of 53 weeks, from the standard benefits of 26 weeks to 79 weeks (the maximum benefits at the end of 2009), increases wages about 11.7%.[48] In these regressions, we control for the state unemployment rate and gross product, individual characteristics, and state, and time fixed effects. As expected, higher lagged unemployment rates reduce earnings, while the effect of lagged GSP on wages is positive.

The last column of Table 3 shows the effect of available UI benefits on wages based on the CPS data. The results in column (3) show that 53 additional weeks of available unemployment benefits increase wages by 4.2%. As the analysis with the CPS data is based on the period 2008-2013, the estimate in column (2) from the LEHD data is the most comparable. The relation of UI to wages documented using the CPS data is about double the effect estimated effect using the LEHD data for the period 2008-2013, although they are not statistically significantly different from each other. A difference in the point estimates could be explained by the differences in the sample definitions. While for the LEHD we use non-employed workers, for the CPS we focus on unemployed individuals, implying the estimated effects from the LEHD data can be considered a lower bound for unemployed workers.

[48] Nekoei and Weber (2017) find that 9 additional weeks of UI benefits increases wages by 0.5%. For comparison, our estimates imply that a 9-weeks extension increases wages by 1.9%. These differences can be attributed to the different identification strategies. While Nekoei and Weber (2017) use a local identification around an age cutoff of 40 years, we include all ages. As we will show later, the effect for young workers are greater than for old workers. Importantly, this exceeds the 9% loss in earnings losses from multiple job losses after displacement and 1-4% loss due to a single job loss estimated by Stevens (1997).

6.2 Match Quality, Sorting, and Employer Quality

We now turn to study the relationship between UI extensions and the three mechanisms highlighted in the previous sections: match quality, sorting, and employer quality.

We start by studying the impact of UI duration on the quality of the match between employers and employees proxied by the residuals from the Woodcock estimation. These estimates are reported in the first two columns of Table 4. Column (1) shows the estimates for the period 2000-2013, and column (2) for the sub-period 2008-2013. An increase in UI benefits improve the match quality between employers and employees coming from non-employment. The estimate in column (1) indicates that an increase of 53 weeks in UI benefits increases the match quality by 4.1%. Thus, improvements in the match quality due to greater generosity of UI would explain about 35% of the increase in wages due to longer UI benefit durations. The estimate for the period of the Great Recession is between 30% and 40% lower than the one estimated for the full period, but the two estimates are not statistically different from each other.

To ensure we do not confound the effect of labor market conditions with UI benefit duration, we control for the lagged unemployment rate and gross state product in all of our estimations. The effect of the unemployment rate (UR) is negative, as expected, but significant only in column (1) at the 5% level. The effect of Gross Domestic Product (GSP) is not significant at standard significance levels.[49]

The remaining two columns of Table 4 provide insights about the positive assortative matching in the labor market. Columns (3) and (4) report the estimates for the similarity index defined in equation (5). The estimates show extensions in UI benefits generate a higher degree of positive sorting between employers and employees during the Great Recession than in the full period. In terms of magnitude, the estimate in column (4) predicts that an increase of 53 weeks in the UI duration increases positive sorting in the labor market by 0.6%. The smaller estimated effects on wages compared to the match-specific quality results could be due to the fact that greater positive assortative matching implies that some workers experience wage gains from better sorting, while other workers experience wage losses. Thus, not only are the effects of UI on sorting small, but the contribution of this improved assortative matching on wages is ambiguous.

Table 5 presents the effects of UI extensions on the quality of employers. We report two measures of employer quality: i) the estimated employer fixed effects in columns (1) and (2), and ii) a dummy variable that takes the value of one if the employer fixed effect is above the average of the fixed effects in columns (3) and (4). All estimates in Table 5 are positive and statistically significant at least at the 5% level, suggesting that the average quality of employers in realized matches with workers improves due to the extensions in UI benefits. Using the estimate from column (1), we document that an increase of 53 weeks in UI benefits leads to an improvement in the average quality of matched employers by 5.8%.[50] This means that the impact of UI on employer quality would explain the bulk of the effect of UI on wages (49%). One interpretation of

[49] As a robustness check, we also include the state unionization coverage rate to control for changes in workers' bargaining power. Unionization coverage rates include both active union members as well as non-affiliated workers whose jobs are covered by a union or an employee association contract. The data are collected from the BLS as part of the Current Population Survey (CPS). Our findings are unchanged when we include this additional control.

[50] The results are not statistically significantly different for the full period and the period of the Great Recession.

these results is that greater generosity of UI may allow workers to search for longer until they can find a job with a good employer. Thus, the realized quality of the employer is higher when workers get UI for a longer period of time. Another interpretation is that more generous UI leads employers to post fewer higher quality vacancies to be able to attract better workers given that wages are higher. We do find some evidence that the distribution of vacancies does seem to shift with higher UI, as the median employer quality is 0.09 for the entire period and 0.11 for the period of the Great Recession.[51]

An improvement in the average employer quality after UI extensions could also partially explain why we find a small effect of UI on sorting. The ranking of workers and employers is based on the individual fixed effects that are estimated over all available periods. Improvements in employers' qualities would mean the mass of employers in the lower part of the quality distribution is lower. In the absence of changes in the distribution of workers' qualities, a switch in the employers' distribution mechanically could imply a lower degree of sorting in the labor market.

We now move to models with interactions to check whether the impact of the extensions in UI benefits are bigger for male vs. female, or white vs. non-white, or more- vs. less-educated, or old vs. young workers. These additional tests provide further insights on the importance of liquidity constraints in shaping the search behavior of workers during periods of unemployment. Workers with greater liquidity constraints, such as women, non-whites, less-educated and young workers, should benefit the most from having access to extensions in UI benefits, and having the possibility to search for a better match for a longer period of time.

Table 6 reports the results based on the match-specific quality measures as the residuals from the Woodcock estimation in equation (1) for the period 2000-2013.[52] Two main results stand out. First, the estimates of the interaction terms of the UI duration and the dummy for the less-likely liquidity constrained groups are all negative and significant at least at the 5% level. Thus, UI benefits have a bigger impact on more-likely liquidity constrained demographic groups. An additional 53 weeks of UI benefits imply that the improvement in the match quality is 0.81% bigger for women than men, 1.15% for non-white than white workers, 1.6% for less-educated than more-educated workers, and finally, 10% for younger than older workers.

Second, extensions in UI durations benefit all demographic sub-groups we focus on with the exception of old workers for whom we document a negative impact. Prolonging the provision of UI benefits by 53 weeks implies an improvement in the match quality that ranges from 3.3% for more-educated workers to 8% for young workers. These magnitudes highlight that the effect of UI benefits extensions are sizeable and widespread across most demographic groups.

Table 7 reports the effects of employer quality by demographic groups. We find that extensions in UI benefits durations improve the employer quality for all demographic groups. Contrary to the previous findings, we do not observe a clear pattern showing greater effects of UI extensions on employer quality for those workers most likely to be liquidity constrained. We do not document any statistical difference

[51] A more precise test would require looking at how the statutory UI duration changed the variance of employer quality. The evidence on this is mixed. On the one hand, Marinescu (2017) finds no effect on vacancy postings. On the other hand, Hagedorn et al. (2015) shows that the extensions from the Great Recession reduced vacancy postings.

[52] Although not reported in the paper, the findings and the implications are similar for the sub-period 2008-2013.

between men and women, and more- and less-educated workers. 53 additional weeks of UI benefits improve the employer quality for white workers by 1% more than for non-white workers, but worsen the employer quality for older workers by 1% compared to younger workers. The fact that there is no clear pattern of the effects of UI on firm quality may reflect that UI did not affect firm quality through the "liquidity effect." An alternative explanation is that UI does have a "liquidity effect" but getting a good job requires more than time and resources and it also requires information provided by networks and other sources which may not be available to women and minority groups or the less-educated workers.[53]

To conclude, the structural model of Fuller et al. (2014) emphasizes that UI benefits reduce moving costs and increase the probability of switching between industries. We test this theoretical implication in our data. We do not find evidence that extended the weeks for which UI benefits are granted increases the probability of switching between industries.[54] The point estimates, not reported in the paper, are negative but statistically insignificant. These findings together with our previous results on match quality, thus, suggest that improvements in match quality comes from reshuffling of employers and employees within-industry rather than across industries. In this respect, our findings are consistent with Nekoei and Weber (2017) who find that UI benefits do not affect geographic and sectoral worker mobility.

6.3 Job Stability

Both improved matching and better employer quality from more generous UI should also lead to greater job stability. Indeed, both Centeno (2004) and Nekoei and Weber (2017) find greater job stability with more generous UI. Matching models predict that better jobs should, on average, lead to longer tenures (Jovanovic, 1979) and, at the same time, access to good jobs should reduce turnover (Krueger and Summers, 1988).

Here, we explore the effect of UI benefit extensions on the probability of being employed by the same employer one year after the transition from non-employment to employment. The estimate in column (1) of Table 8 suggests that although extensions in UI benefits for the period 2000-2013 increase the probability of remaining employed by the same employer, the effect is statistically insignificant at any standard significance level. It is worth highlighting that higher unemployment rates also increase the probability of remaining employed at the same firm.

The interaction models in columns (2)-(5) report heterogeneous effects by demographic groups. Column (2) shows that while women do not experience any significant increase in their probability of remaining employed, the probability for men decreases by 0.5 percentage points. The remaining columns of Table 8 document a positive and significant increase in the probability of remaining employed for workers who are more-likely to be liquidity-constrained. An extension in UI benefits of 53 weeks leads to an increase in the probability of remaining employed by 2.7 percentage points for non-white, 2.1 percentage points for less-educated, and 1.6 percentage points for young workers. On the contrary, the effects for white, more-educated, and old workers is around 0.5 percentage points or one fourth of the effect of the more liquidity-constrained

[53] Kugler (2003) shows that referrals through family and personal networks improve access to good jobs, but are more available to non-minority workers.

[54] Industries are defined at the 3-digits NAICS codes. Finer industry definitions do not affect our findings.

workers.

Our estimates disaggregated by demographic characteristics also help to reconcile the conflicting findings from previous studies. While Nekoei and Weber (2017) find no significant effect of UI benefits on job stability, Centeno (2004) documents that larger UI benefits lead to longer employment spells after periods of unemployment. A plausible explanation for the differences in the significance between these two studies is the sample composition. As we have showed, the magnitude and statistical significance of our results change across demographic groups. For example, we find that UI benefits have a larger effect on the job stability of younger workers, which is consistent with the larger effects reported by the paper by Centeno (2004), which has an average age in the sample 15 years younger than the sample in the paper by Nekoei and Weber (2017).

6.4 Alternative Measures of Match and Employer Quality

Finally, we examine the impact of UI benefits on educational mismatch and educational requirements of the job using CPS data as alternative measures of mismatch and job quality.

As argued by Gibbons and Katz (1992), match effects capture the intrinsic good fit between workers' skills and employers' skills. As an alternative and more specific measure of mismatch, we examine the impacts of UI benefits on the disparity between workers' educational attainments and educational requirements of their jobs, and on getting access to jobs with higher educational requirements. Table 9 shows the impact of UI on whether an individual gets a better quality job and a job better matched to their skills. In Panel A of Table 9 we analyze whether access to longer UI benefits leads to any decreases in the mismatch between worker education and the skills required for the job. Column (1) shows that higher available weeks of benefits reduce mismatch, but the effect is not statistically significant. Columns (2)-(5) show models with interactions. The main effect is significant in all of these cases except for the model in column (5), which includes the young vs. old interaction. Also, the only interaction term which is significant, but in the opposite direction, is the one with more educated workers. These estimates, thus, show that the effects are bigger for women, non-whites, and less educated workers. An increase of 53 weeks of available UI benefits reduces the mismatch by 60% for women and for non-whites, and by about 100% for those with less than a college degree.

Panel B in Table 9 shows the effects of UI benefits on the difference in education requirements of the job upon exiting unemployment compared to the worker's last job. We find that workers who have access to more weeks of UI benefits can find jobs that have higher education requirements than the ones they were doing before. Thus, we find evidence that having access to a safety net that allows workers to search longer can lead to occupational upgrading. The estimate from column (1) implies that an additional 53 weeks of UI benefits increases the likelihood that workers end up in jobs with higher education requirements by 11.7 percentage points or by 14.4%. Columns (2) through (5) show models with interactions with a male dummy, a white dummy, a college dummy, and a dummy if the person is younger than 40 years old. None of these interactions are significant, but the main effect is significant in all of these cases. As with the employer quality results, this result could be due to workers responding to UI by searching longer and finding better jobs or due to firms creating fewer vacancies with higher educational requirements. Our results are more consistent with the first interpretation as the UI generosity affects only the difference in the worker's educational requirement in the previous and current job but it has no effect on the mean educational attainment of workers.[55]

7 Conclusion

This paper contributes to the literature on the impact of UI in the labor market, by examining how UI affects match quality, sorting and employer quality. Thus, unlike most previous papers which only look at the impact of UI on wages (finding mixed evidence), we disentangle the channels through which UI affects wages. We use LEHD data and find that match quality explains 35% of the increased wages due to more generous UI, while employer quality can explain almost 50% of the wage increase from UI. By contrast, we find that sorting is not much affected by more generous UI benefits. Consistent with both improved matches and employer quality, we find that extended UI benefit duration increases the likelihood of remaining in the job one year later, thus increasing job stability. In total, the increase in wages and the increase in job stability would imply an increase in earnings of $4,593[56] due to the increased generosity in UI benefit duration. Out of this total amount, half or $2,296 can be attributed to improved quality of the employer and $1,607 to improved matching. A back-of-the-envelope cost-benefit calculation using these results suggests the costs of the extended UI benefits exceed the benefits. The cost per person of the extended benefits by 53 weeks is $15,900. while the total benefits including the increased wages and future saved state UI and SNAP or TANF for those who stay in jobs rather than becoming unemployed are $104,000 per person, which greatly exceed the costs of the program.[57]

In addition, we look at more specific measures of job quality and mismatch by examining CPS data to look at the effect of UI on educational requirements on the job and educational mismatch. These results also show that educational mismatch diminishes and educational requirements in the current relative to the previous job improve, as UI benefit duration is extended.

Our results are consistent with a "liquidity effect" of UI benefits rather than a "moral hazard effect." A "moral hazard effect" would suggest that workers stay unemployed longer but they search less and end up in worse jobs or worse matches not better ones. It would also suggest that workers end up in more unstable jobs as well. By contrast, the "liquidity effect" is consistent with the findings in our paper and would suggest also that these effects are greater for those who are more likely to face stronger liquidity constraints. When we examine heterogeneous effects for various subgroups, we find that match quality improves for women,

[55] We test this by running the same specification in levels, and we do not find any statistically significant results.

[56] The $4,593 comes from first estimating the yearly increase in average earnings due to longer UI benefit duration or $5,200 (average earnings) multiplied by 0.12 (the increase in earnings from UI benefits being extended) multiplied by 4 to turn into yearly earnings. Then, we multiply the increased job stability of 0.27 due to increased UI benefit duration times 4, so an increase in 1.84 years, and multiply by the increase in yearly earnings.

[57] The cost per person of the extended benefits by 53 weeks is $15,900 estimated at the average weekly UI benefit of $300. The benefits are the increased wages of $4,593 reported above. To this, we add the savings in state UI benefits and federal benefits due to remaining employed for 1.84 years instead of being unemployed. If the person receives on average $300 weekely in state UI benefits for 6 months or 24 weeks, the savings would be $7,200. If for the remaining 70 weeks, the person would have received either SNAP or TANF, which amounts to $660 per week for a family of 3, then the savings would be about $46,000. Thus, the benefit from the program would be close to $104,000 per person compared to the costs of $15,900.

non-whites and less-educated workers. This is consistent with greater match quality effects for those who are now able to search longer by overcoming credit constraints during their search process. By contrast, we find a greater effect of UI benefit duration on employer quality for men and no difference for the other groups. While this may in itself may not rule out "liquidity effects," it does point to the possibility that relieving liquidity constraints may not be enough for getting a job with a higher quality employer and access to networks or other factors may be important in getting better jobs.

Our results show more generous UI benefit duration could help erase some of the earnings losses due to displacements, which have been found to be not only large but also persistent. Interestingly, Lachowska et al. (2020) examine the sources of displaced workers' earnings losses and find that 50% of these can be explained by loss of valuable specific worker-employer matches and 17% by employer effects. We, instead, find that 35% of the earnings gains due to UI benefit generosity are due to improved match quality and 50% are due to improved employer quality. Our work, thus, shows that increased duration of UI benefits can mitigate the high procyclicality of the firm wage ladder found in Haltiwanger et al. (2018) and encourage reallocation of workers towards higher wage firms during recessions.

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Table 1: Descriptive Statistics Workers' and States' Characteristics, LEHD

	2000-2013			2008-2013		
	All States	States Above UI Average	States Below UI Average	All States	States Above UI Average	States Below UI Average
Share of Men	0.5153	0.5167	0.5124	0.5110	0.5114	0.5103
Share of Whites	0.8110	0.8059	0.8213	0.8044	0.7991	0.8247
Share Some College or More	0.5892	0.6007	0.5662	0.5821	0.5948	0.5578
Age (years)	40.57	40.64	40.44	41.11	41.19	40.96
Tenure(quarters)	16.87	17.39	17.31	19.43	19.95	18.42
Quarterly earnings	5,200	5,200	5,100	5,200	5,200	5,200
Gross State Product (Millions)	352,693	401,462	279,540	367,781	414,153	298,223
Unemployment Rate (%)	6.10	6.55	5.42	7.74	8.54	6.55

Notes: The average number of weeks of UI is computed over the period 2000-2013, and it is equal to 46 weeks. The states with UI benefits above the average are Arizona, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Nevada, Pennsylvania, Tennessee, and Washington. The states with UI benefits below the average are Arkansas, Iowa, Kansas, Maine, Maryland, Oklahoma, Texas, and Wisconsin.

Table 2: Descriptive Statistics of Workers' Characteristics, CPS

	2008-2013		
	All States	States Above UI Average	States Below UI Average
Share of Workers that Move to a Job with Higher Education Requirement	0.815	0.815	0.815
Difference in Education Requirement between New and Old Job	-0.0405	-0.0183	-0.0510
Education Mismatch (Years)	0.131	0.185	0.105
Years of Education	12.99	12.97	13.01
Share of Men	0.583	0.573	0.587
Share of White	0.809	0.789	0.818
Share Some College or More	0.471	0.476	0.469
Age (years)	36.83	36.61	36.93
Hourly Wage upon Exit from Unemployment (USD)	12.49	12.17	12.74

Notes: The statistics in all rows but the last are computed using the Monthly Data files from the Current Population Survey for the period 2008-2013. The last row uses the CPS monthly data files from IPUMS to construct longitudinal histories of workers. The states are classified as above or below the mean based on the "short sample" averages. The states above the average UI are Alabama, Alaska, Arizona, California, Colorado, Connecticut, Delaware, District of Columbia, Georgia, Idaho, Illinois, Indiana, Kentucky, Maine, Massachusetts, Michigan, Nevada, New Jersey, New York, North Carolina, Ohio, Oregon, Pennsylvania, Rhode Island, Tennessee, Texas, Washington, West Virginia, and Wisconsin. The states below the average UI are Arkansas, Florida, Hawaii, Iowa, Kansas, Louisiana, Maryland, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Mexico, North Dakota, Oklahoma, South Carolina, South Dakota, Utah, Vermont, Virginia, and Wyoming.

Table 3: Effects of UI Duration on log Wages

	LEHD		CPS
	(1) 2000-2013	(2) 2008-2013	(3) 2008-2013
UI Duration	0.0022**	0.0004	0.0008***
	(0.0009)	(0.0012)	(0.0003)
Unemployment Rate	-0.0325***	-0.0176	-0.0116
	(0.00980)	(0.012)	(0.0081)
Gross State Product	-0.1072	0.1179	-0.0000
	(0.2876)	(0.2349)	(0.0000)
Observations	155,000,000	66,150,000	9824
R2	0.1676	0.1646	0.0230
Individual Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes

Notes: Results in columns (1) and (2) are based on the LEHD data and include the following states: Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, Pennsylvania, Tennessee, Texas, Washington, and Wisconsin. The sample includes workers transitioning from non-employment to employment. Earnings in Columns (1) and (2) are deflated by using the CPI Index adjusted for the Regional Price Parities Index to account for differences in purchasing power between states. Results in column (3) use the "longitudinal" sample from the monthly CPS data. The CPS sample includes all 50+1 states. Column (3) includes as controls a set of individual demographic characteristics such as age, squared age, number of years of education, and dummies for the race, gender, and marital status. The wages in Column (3) are deflated by the 2010 national CPI. Standard errors are included in parenthesis and are clustered at the state level. The asterisks *, **, and *** reflect the 10%, 5%, and 1% significance levels, respectively.

Table 4: Effects of UI Duration on Residuals and Similarity Index, LEHD

	Residuals		Similarity Index	
	(1)	(2)	(3)	(4)
	2000-2013	2008-2013	2000-2013	2008-2013
UI Duration	0.0008***	0.0005*	0.00004	0.0001***
	(0.0003)	(0.0003)	(0.0001)	(0.0000)
Unemployment Rate	-0.0100**	-0.0079	-0.0006	-0.0014
	(0.0037)	(0.0046)	(0.0008)	(0.0008)
Gross State Product	-0.0011	-0.0005	-0.00001	-0.0006
	(0.0063)	(0.0067)	(0.0017)	(0.0015)
Observations	155,000,000	66,150,000	155,000,000	66,150,000
R2	0.0002	0.0002	0.00001	0.0001

Notes: The states included in the analysis are Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, Pennsylvania, Tennessee, Texas, Washington, and Wisconsin. The samples include workers transitioning from non-employment to employment. Standard errors are included in parenthesis and are clustered at the state level. The asterisks *, **, and *** reflect the 10%, 5%, and 1% significance levels, respectively.

Table 5: Effects of UI Duration on Employer Quality, LEHD

	Continuous Employer FE		Above Average Indicator of Employer FE	
	(1)	(2)	(3)	(4)
	2000-2013	2008-2013	2000-2013	2008-2013
UI Duration	0.0011***	0.0013**	0.0006**	0.0004**
	(0.0003)	(0.0004)	(0.0002)	(0.0002)
Unemployment Rate	-0.0210***	-0.0240***	-0.0072*	-0.0064*
	(0.0060)	(0.0052)	(0.0037)	(0.0036)
Gross State Product	-0.0074	-0.0168**	-0.0031	-0.0038
	(0.0059)	(0.0068)	(0.0007)	(0.0068)
Observations	155,000,000	66,150,000	155,000,000	66,150,000
R2	0.0023	0.0044	0.0004	0.0005

Notes: The states included in the analysis are Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, Pennsylvania, Tennessee, Texas, Washington, and Wisconsin. The sample includes workers transitioning from non-employment to employment. Standard errors are included in parenthesis and are clustered at the state level. The asterisks *, **, and *** reflect the 10%, 5%, and 1% significance levels, respectively.

Table 6: Effects of UI Duration on Residuals by sub-groups, LEHD

	(1) Men vs. Women	(2) Whites vs. Non-Whites	(3) More-educated vs. Less-educated	(4) Old vs. Young
UI Duration	0.0009***	0.0010***	0.0009***	0.0015***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Group Dummy	0.0308***	-0.0053	0.0057	0.0191***
	(0.0022)	(0.0049)	(0.0058)	(0.0063)
UI Duration × Group Dummy	-0.0002***	-0.0002***	-0.0003***	-0.0019***
	(0.0000)	(0.0000)	(0.0000)	(0.0001)
Unemployment Rate	-0.0100**	-0.0101**	-0.0099**	-0.0093**
	(0.0037)	(0.0037)	(0.0038)	(0.0041)
Gross State Product	-0.0011	-0.0014	-0.0012	-0.0014
	(0.0056)	(0.0063)	(0.0064)	(0.0067)
Observations	155,000,000	155,000,000	155,000,000	155,000,000
R2	0.0004	0.0002	0.0002	0.0026

Notes: The states included in the analysis are Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, Pennsylvania, Tennessee, Texas, Washington, and Wisconsin. The sample includes workers transitioning from non-employment to employment. The estimates are reported for the period 2000-2013. The variable "Group Dummy" changes from column to column. In Column (1), it takes the value of 1 if an employee is male and 0 otherwise. In Column (2), it takes the value of 1 if an employee is white and 0 otherwise. In Column (3), it takes the value of 1 if an employee's highest education is some college or more and 0 otherwise. In Column (4), it takes the value of 1 if an employee is older than or equal to 40 years old and 0 otherwise. Standard errors are included in parenthesis and are clustered at the state level. The asterisks *, **, and *** reflect the 10%, 5%, and 1% significance levels, respectively.

Table 7: Effects of UI Duration on Employer Quality by sub-groups, LEHD

	(1) Men vs. Women	(2) Whites vs. Non-Whites	(3) More-educated vs. Less-educated	(4) Old vs. Young
UI Duration	0.0011***	0.0010***	0.0012***	0.0012***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Group Dummy	0.0163***	-0.0152*	0.0500***	0.0153***
	(0.0040)	(0.0079)	(0.0059)	(0.0033)
UI Duration $\times$ Group Dummy	0.0001	0.0002***	-0.0001	-0.0002***
	(0.0001)	(0.0000)	(0.0001)	(0.0000)
Unemployment Rate	-0.0210***	-0.0210***	-0.0212***	-0.0210***
	(0.0060)	(0.0060)	(0.0059)	(0.0060)
Gross State Product	-0.0073	-0.0075	-0.0072	-0.0074
	(0.0059)	(0.0058)	(0.0057)	(0.0059)
Observations	155,000,000	155,000,000	155,000,000	155,000,000
R2	0.0026	0.0023	0.0037	0.0023

Notes: The states included in the analysis are Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, Pennsylvania, Tennessee, Texas, Washington, and Wisconsin. The sample includes workers transitioning from non-employment to employment. The estimates are reported for the period 2000-2013. The employer quality is defined as the continuous employer fixed effects. The variable "Group Dummy" changes from column to column. In Column (1), it takes the value of 1 if an employee is male and 0 otherwise. In Column (2), it takes the value of 1 if an employee is white and 0 otherwise. In Column (3), it takes the value of 1 if an employee's highest education is some college or more and 0 otherwise. In Column (4), it takes the value of 1 if an employee is older than or equal to 40 years old and 0 otherwise. Standard errors are included in parenthesis and are clustered at the state level. The asterisks *, **, and *** reflect the 10%, 5%, and 1% significance levels, respectively.

Table 8: Effects of UI Duration on the Probability of Remaining Employed, LEHD

	(1) All Workers	(2) Men vs. Women	(3) Whites vs. Non-Whites	(4) More-educated vs. Less-educated	(5) Old vs. Young
UI Duration	0.0003	0.0003	0.0005***	0.0004**	0.0003*
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Group Dummy		-0.0049***	0.0502***	0.0315***	
		(0.0013)	(0.0094)	(0.0017)	
UI Duration × Group Dummy		-0.0004*	-0.0003***	-0.0003***	-0.0002***
		(0.0000)	(0.0000)	(0.0000)	(0.0000)
Unemployment Rate	0.0019**	0.0019**	0.0017*	0.0019**	0.0020**
	(0.0009)	(0.0009)	(0.0009)	(0.0008)	(0.0009)
Gross State Product	-0.0051	-0.0056	-0.0072	-0.0064	-0.0054
	(0.0257)	(0.0256)	(0.0250)	(0.0259)	(0.0256)
Observations	155,000,000	155,000,000	155,000,000	155,000,000	155,000,000
R^2	0.0918	0.0919	0.0927	0.0922	0.0919
Individual Controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

Notes: The states included in the analysis are Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, Pennsylvania, Tennessee, Texas, Washington, and Wisconsin. The sample includes workers transitioning from non-employment to employment. The estimates are reported for the period 2000-2013. The variable "Group Dummy" changes from column to column. In Column (2), it takes the value of 1 if an employee is male and 0 otherwise. In Column (3), it takes the value of 1 if an employee is white and 0 otherwise. In Column (4), it takes the value of 1 if an employee's highest education is some college or more and 0 otherwise. In Column (5), it takes the value of 1 if an employee is older than or equal to 40 years old and 0 otherwise. Standard errors are included in parenthesis and are clustered at the state level. The asterisks *, **, and *** reflect the 10%, 5%, and 1% significance levels, respectively.

Table 9: Effects of UI Duration on Educational Requirement Matches, CPR 2008-2013

	(1) All Workers	(2) Men vs. Women	(3) Whites vs. Non-Whites	(4) More-educated vs. Less-educated	(5) Old vs. Young
	Panel A: Mismatch in Years of Education				
Available UI Duration	-0.0007	-0.0014**	-0.0014**	-0.0027***	-0.0004
	(0.0005)	(0.0007)	(0.0006)	(0.0005)	(0.0006)
Group Dummy		-0.1760***	-0.2290***		
		(0.0508)	(0.0613)		
Available UI Duration × Group Dummy		0.0013	0.0010	0.0043***	-0.0004
		(0.0008)	(0.0009)	(0.0006)	(0.0007)
Unemployment Rate	0.0444**	0.0444**	0.0441**	0.0447**	0.0445**
	(0.0206)	(0.0206)	(0.0206)	(0.0205)	(0.0206)
Gross State Product	0.0000***	0.0000***	0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Observations	14994	14994	14994	14994	14994
R^2	0.518	0.518	0.518	0.519	0.518
Individual Controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
	Panel B: Probability of Higher Educational Requirements				
Available UI Duration	0.0022***	0.0024***	0.0021**	0.0023***	0.0022***
	(0.0005)	(0.0006)	(0.0009)	(0.0005)	(0.0005)
Group Dummy		0.0387	0.0208		
		(0.0579)	(0.0637)		
Available UI Duration × Group Dummy		-0.0003	0.0001	-0.0003	-0.0003
		(0.0009)	(0.0011)	(0.0005)	(0.0005)
Unemployment Rate	-0.0543**	-0.0543**	-0.0543**	-0.0543**	-0.0543**
	(0.0206)	(0.0206)	(0.0206)	(0.0206)	(0.0206)
Gross State Product	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Observations	13759	13759	13759	13759	13759
R^2	0.007	0.007	0.007	0.007	0.007
Individual Controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

Notes: All specifications include state, year, and month fixed effects, and a set of individual demographic characteristics such as age, squared age, the number of years of education, and dummies for the race, gender, and marital status. The variable "Group Dummy" changes from columns (2) to (5). In Column (2), it takes the value of 1 if an employee is male and 0 otherwise. In Column (3), it takes the value of 1 if an employee is white and 0 otherwise. In Column (4) and Column (5), we do not include any dummy variable because we include continuous variables for the age and the number of years of education. Standard errors are included in parenthesis and are clustered at the state level. The asterisks *, **, and *** reflect the 10%, 5%, and 1% significance levels, respectively.

Figure 1: UI Duration during and in the aftermath of the Great Recession

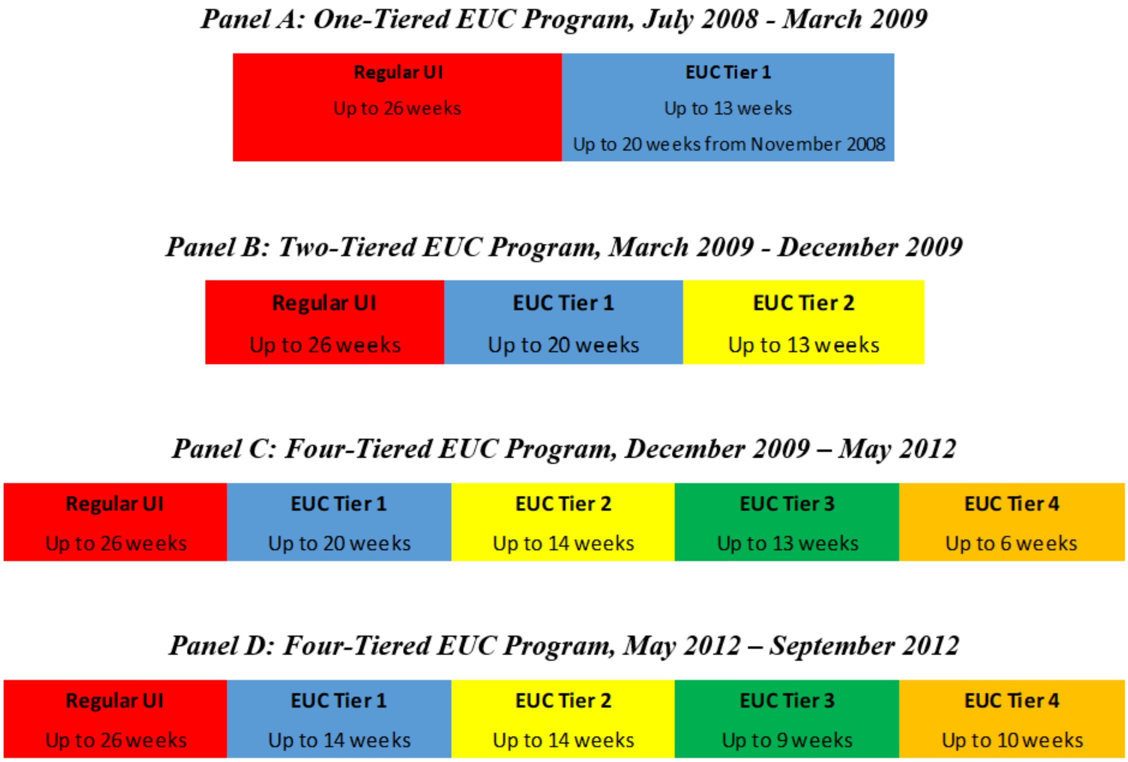
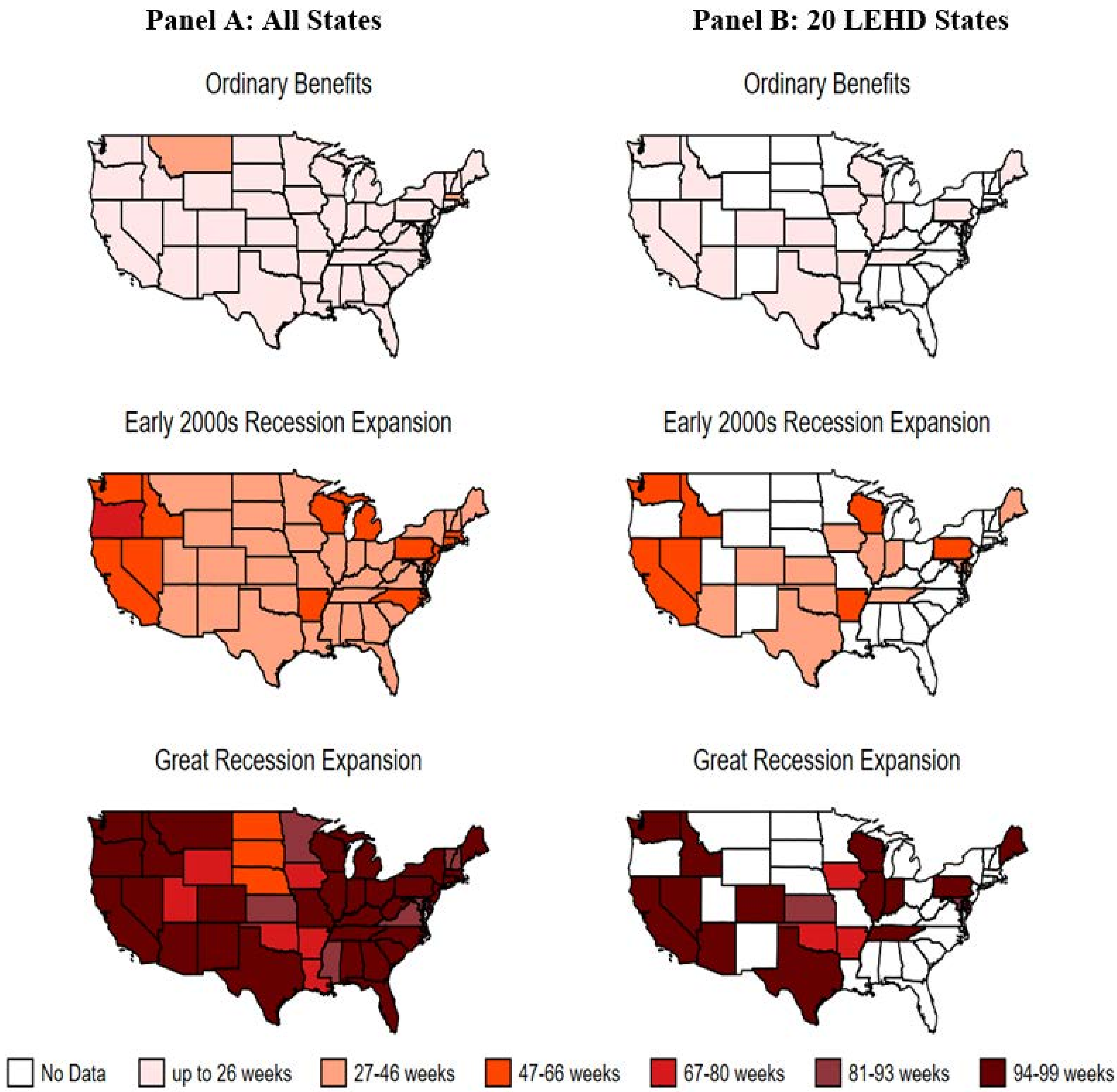
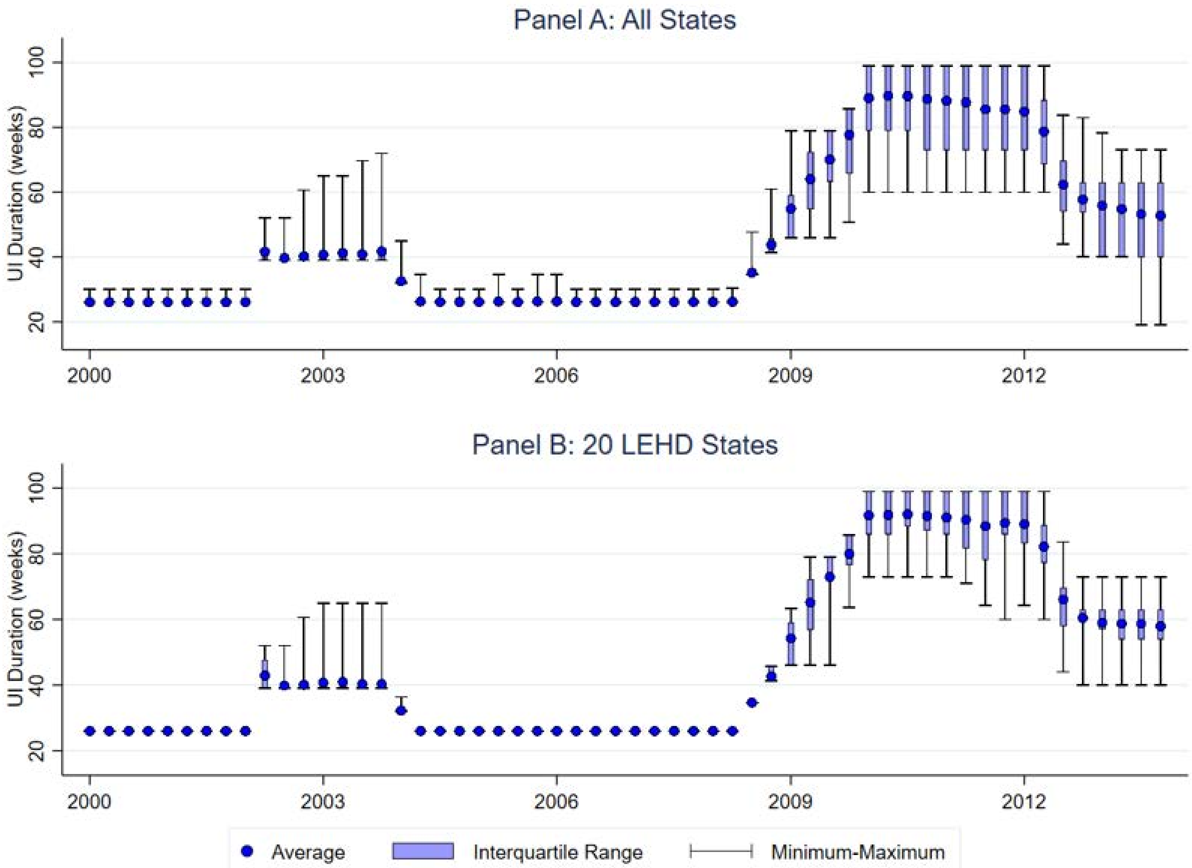


Figure 2: Maximum Weeks of UI Benefits across States and over Time



Notes: The states included in Panel B are: Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, Pennsylvania, Tennessee, Texas, Washington, and Wisconsin.

Figure 3: UI Benefits Variations within Quarters



Notes: The states included in Panel B are: Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Idaho, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Nevada, Oklahoma, Pennsylvania, Tennessee, Texas, Washington, and Wisconsin.

Figure 4: UI Benefits Variations within States

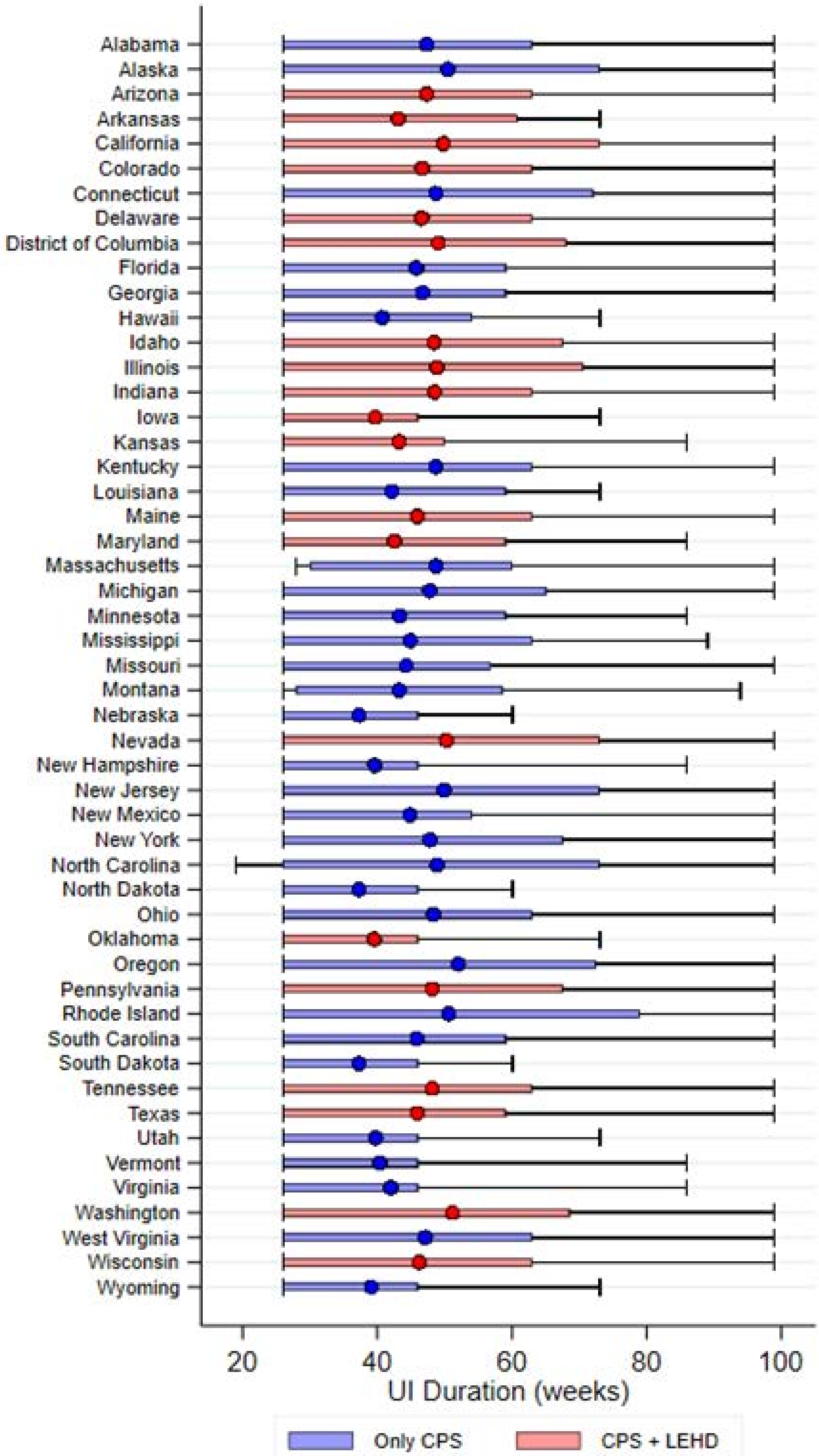


Figure A.1: UI Benefits Duration and Unemployment Rate by States